

How enforcement institutions affect impersonal exchange*

Benito Arruñada

and

Marco Casari

This version: 23 June 2009

Abstract

In an experiment we study market outcomes under alternative incentive structures for third-party enforcers. Our transactions resemble anonymous credit transactions where lenders can give loans and borrowers can repay them. When borrowers default, judges are free to enforce repayment but are themselves paid differently in each of three treatments. First, paying judges according to lenders' votes maximizes surplus and the equality of earnings. In contrast, paying judges according to borrowers' votes triggers insufficient enforcement, destroying the exchange opportunities and producing the lowest surplus and the most unequal distribution of earnings. Lastly, judges paid the average earnings of borrowers and lenders achieve results close to those based on lender voting. We employ a steps-of-reasoning argument to interpret the performances of different institutions. When voting and enforcement rights are allocated to different classes of actors, the difficulty of their task changes, and arguably as a consequence they focus on high or low surplus equilibria.

Keywords: impersonal exchange, third-party enforcement, experiments, steps of reasoning, judges' incentives, repeated interaction.

JEL codes: C91, C92, D63, D72, K40

* Arruñada: Pompeu Fabra University and Barcelona GSE, Trias Fargas, 25, 08005 Barcelona, Spain, voice: +34 935 422 572, fax: +34 935 421 746, email: benito.arrunada@upf.edu; Casari: Università di Bologna, Department of Economics, Piazza Scaravilli 2, 40126 Bologna, Italy, voice: +39 051 2098662, fax: +39 051 209 8493, email: marco.casari@unibo.it. Comments by Gabriele Camera, Juanjo Ganuza, Luis Garicano, Henry Hansmann, Lewis Kornhauser, Steven Martin, Bill Novshek, Julian Rode, Cándido Paz-Ares and seminar and conference participants at FSU workshop, AEA, Amsterdam, Carlos III, CLEF, FEDEA and Purdue, as well as research assistance by Jingjing Zhang and Sukanya Chaudury are gratefully acknowledged. Usual disclaimers apply. Funding for this research was provided by Purdue University, the Spanish Ministry of Education, through grant SEJ2005-03871/ECON, and the European Commission through the 'Reflexive Governance' Integrated Project (CIT3-513420).

1. Introduction

State enforcement—forcing parties to fulfill their obligations—makes impersonal exchange secure. It is often essential for impersonal exchange between anonymous parties, which is the hallmark of developed market economies (North and Thomas, 1973; Granovetter, 1985; North, 1990; Seabright, 2004), as empirically examined by Hoffman, McCabe and Smith (2005). Lack of enforcement compels traders in developing economies to rely on personal exchange, wasting trade opportunities, as publicized by Hernando de Soto (1989, 2000) and measured by the World Bank *Doing Business* reports. Moreover, good substitutes for State enforcement do not exist for anonymous exchanges. Relational contracts are hardly feasible in anonymous exchanges and, when feasible, they require the abstention or “forbearance” of the State (Williamson, 1991), which often heavily constrains them through judicial rulings and mandatory laws so that relational contracts always operate “in the shadow of the law” (Mnookin and Kornhauser, 1979).

Enforcement failure is possible because enforcers—both judges and lawmakers—enjoy substantial discretion. In a world of incomplete contracts, enforcers not only enforce but also define the obligations of the parties in a specific scenario—in a sense, they “complete” the contract ex post; and this double task makes it possible for them to disguise enforcement failures as contractual “completions”. At a deeper level, the State, as a sovereign actor, is always in a position of power over contractual parties. The risk of enforcement failure is therefore ever present, with judges potentially allowing contractual defaults or, generally, States failing to enforce contracts. Claims of such failures abound. For instance, weak enforcement of foreclosures by judges hinders mortgage lending in developing countries (Field and Torero, 2006; Galiani and Schargrodsky, 2006), as farm foreclosure moratoria did in the 25 US states that enacted it in the 1930s (Alston, 1984).

Given that enforcement failure can thwart markets, societies implement institutional arrangements to limit the discretion of enforcers and shape their decisions. Judges are generally restrained by judicial precedents and the possibility of appeals. Legislators are limited by constitutional rules. Specific arrangements also operate on both judges and legislators. For example, judges may be elected or appointed, and their careers may depend on seniority or on

merit assessments. Similarly, different political structures—for instance, allocations of voting rights, from limited to universal suffrage—might motivate legislators differently.

These alternative institutional arrangements may produce different enforcement results based on two factors: allocation of enforcement rights and incentives. First, each institutional arrangement allocates enforcement rights differently within society. For instance, states may grant voting rights to an elite of property owners, to all males, or to all citizens. These differences in constituencies make lenders or borrowers, employers or employees, or landlords or tenants more or less influential in defining the degree of contract enforcement. In the 1930s, US states suffering the most severe farm distress were more likely to enact mortgage moratoria. Similarly, elected US judges tend to rule in favor of local businesses (Tabarrok and Helland, 1999). Second, some institutional arrangements may provide decision makers with different incentive functions, linking their enforcement decisions to their personal compensation. So defined, incentives could predictably encourage or discourage enforcement.

To analyze these issues, we designed an experiment representing an economy composed of three classes of actors: “lenders,” “borrowers” and “judges.” Transactions between lenders and borrowers generate surplus. Hence the economy reaches a socially optimal outcome when everyone completes a transaction. Decisions are made sequentially and when borrowers do not return “loans,” judges can enforce repayment or accommodate default. We examine the consequence of allocating enforcement rights to one of the three classes while always giving the deciding class incentives for optimal enforcement.

More specifically, our experimental treatments represent three institutional arrangements in which different classes of individuals hold the key decision rights. In the “GDP” treatment, we pay judges proportionally to the aggregate income of the economy. In contrast, in two “constituency” treatments (lender and borrower), we pay judges according to how close they rule to the average vote of their constituency class. In all three treatments, judges therefore have formal enforcement powers, as they are free to enforce or not, but in constituency treatments a different class of subjects controls enforcement in actual fact, which is why we will talk of allocating *enforcement rights* to different classes of subjects: to judges in the GDP treatment,¹

¹ For simplicity, we will be talking about “judges”, but the position of the members of this class is also close to that of legislators.

lenders in the Lender constituency treatment and borrowers in the Borrower constituency treatment.

In all three treatments, the socially optimal outcome is an equilibrium—in other words, those holding enforcement rights benefit from promoting enforcement, because their personal incentives are aligned with aggregate efficiency. All relevant information to identify the socially optimal outcome is public. In two treatments (Borrower constituency and GDP) other equilibria exist where enforcement is low and the exchange opportunities disappears; hence, there is an issue of coordination.

We find that the degree of enforcement does depend on how enforcement rights are allocated, to the extent that when borrowers control judges, enforcement falls below the threshold that makes transactions profitable, and very soon no transactions take place and the market disappears. In the GDP treatment, judges' enforcement is high and the level of transactions is close to optimal. We claim that these differences in enforcement are caused by differences in the difficulty of the problem in each treatment, which makes a particular outcome salient in a given institutional arrangement, but not in another.²

When we pay judges according to borrowers' votes, borrowers control judges but are unable to grasp that it is in their best interest to ensure a minimum profit to lenders. They thus motivate judges not to enforce, judges follow suit, loans dry up and borrowers' earnings are extremely low. In contrast, when lenders control judges, lenders encourage enforcement, the economy achieves full efficiency and borrowers end up better both in terms of absolute and relative income.

Our experiment is a variation of the trust game (Berg *et al.*, 1995) where choices are binary and there is a sort of final litigation stage. Bohnet *et al.* (2001) studied a “contract game” with similarities to ours, where generally subjects are randomly matches, play a modified trust game, and everyone observes the aggregate outcome. When defaulting obligations, there is a costly

² There is evidence that people suffer cognitive failures in different domains (Tversky and Kahneman, 1974; Nagel, 1995; Camerer, 2003). If enforcers suffer similar failures, the allocation of enforcement rights may matter. This concern may seem minor since enforcers are experts, such as judges and politicians. However, experts in other fields also suffer biases (McNeil *et al.*, 1982). Furthermore, some studies find that judges suffer from “anchoring,” “hindsight,” “overconfidence,” “framing” and “representativeness” biases (Guthrie, Rachlinski and Wistrich, 2001). As for politicians, their possible biases add to those of citizens (Westen *et al.*, 2006), who ultimately drive the incentives of politicians. Furthermore, it is politicians who design the incentives of judges. Thus, the cognitive dimension of the contract enforcement problem is ultimately defined by the ability of non-expert citizens to understand the problem.

litigation procedure with random ruling, where the probability of winning the case varied depending on the treatment. They also interpret the results under the assumption of multiple types of subjects. Our design importantly differs from Bohnet et al. (2001) in the litigation stage because in the present study cases are decided at the discretion of a human judge. Moreover, instead of interacting an exact number of periods, in our design interaction is repeated with random endings (Engle-Warnick and Slonim, 2006), which introduces additional equilibria even when everyone is a self-regarding type. To rule out the feasibility of relational contracts (Brown, Falk and Fehr, 2003), we hid subject identifiers. In addition to this trust game, our study includes a guessing game to estimate subjects' reasoning ability (Nagel, 1995; Costa-Gomes and Crawford, 2006) and a static game similar to Engelmann and Strobel (2004) to elicit subjects' inequality aversion and altruism.

The rest of the paper is structured as follows. Section 2 presents the experimental design. Section 3 defines the theoretical predictions, detailing the different equilibria in both the one-shot and the indefinitely repeated game. Section 4 presents the main results of the experiment, chiefly that when borrowers enjoy enforcement rights they are trapped in an inferior equilibrium. Section 5 presents a couple of variations of the basic treatments designed to test alternative explanations. Section 6 analyzes such possible explanations for the main results. Section 7 concludes.

2. Experimental design (part 3)

A simple economy is analyzed through a series of experimental sessions, in which different sets of individuals interact for several periods. We first measured other-regarding preferences (part 1) and reasoning ability (part 2), and then had subjects interacting in a trust game with third-party enforcement (part 3). In this study we manipulated the compensation of third-party enforcers in part 3. As a result, the experiment had three main treatments: “Lender constituency,” “Borrower constituency,” and “GDP” (Table 1). Parts 1 and 2 were identical across treatments and subjects received no feedback until the end of the session. We postpone their description to section 5.

Part 3 was a modified trust game between a lender (trustor) and a borrower (trustee) to which we added a judge or third-party enforcer. (Here we use these labels for description—in the

experiment we used neutral language). At the beginning of each session, 15 subjects randomly assumed a role, and 5 acted as lenders, 5 as borrowers, and 5 as judges—all of them retaining the same role until the end of the session. Each session or economy—we will use both labels interchangeably—lasted for several periods. In period one, subjects were randomly partitioned into 5 groups. Each group included one lender, one borrower, and one judge, who interacted together for the period. After each period, *groups were randomly rematched; subjects were not informed about the identity of the people in their group* (stranger protocol). For each session, this game was repeated for at least 20 periods. There were no practice periods. After every period above period 20, a subject was asked to roll a dice. If the result was a six, the session was over; otherwise it continued. It follows that the overall expected length of the interaction is 26 periods.

This modified trust game had the following four features. First, choices were binary. The lender (trustor) could either lend (send 10 tokens) or save (send 0), the borrower (trustee) could either comply (return 17 tokens to the trustor) or default (return nothing to the trustor), and the judge could either enforce or accommodate the borrowers' default (Figure 1). An experimental token was worth \$0.45. The instructions employed a neutral framing and avoided any ethical connotation for borrower defaulting (Appendix A). The implicit wealth multiplier was 3.4 and final earnings for (lender, borrower) could be either (60, 16) for saving, (67, 33) for repaid trust, or (50, 50) for betrayed trust. A borrower maximizes earnings when she receives a loan and then defaults. Yet, this outcome is not sustainable in the long run because lenders are likely to react by saving, hence generating the lowest possible earnings for the borrower.

Second, the design had a built-in inequality in *minimum* earnings, which were 50 tokens for lenders, 16 for borrowers, and in-between for judges. This parametrization has two advantages. On one hand, a variety of motivations—self-interest, inequality aversion, efficiency concern—make “saving” decisions the worst possible outcome. This intentional inequity aligned the predictions for other-regarding subjects with the predictions for self-regarding subjects. On the other hand, for all motivations it generates an intriguing trade-off between short-term and long-term outcomes, which puts to test subjects' ability to achieve the best outcome. For instance, inequity-averse borrowers and judges could “default/accommodate” to achieve an outcome with equal earnings. Yet, this outcome is not viable in the long run because lenders are likely to react by “saving,” hence generating the most unequal outcome possible. Hence, rational other-regarding borrowers and judges want to give lenders enough profits to induce them to lend. We

want to study how subjects deal with this situation. To make the possibility of ending in the most unequal outcome more vivid, we did not pay the average period earnings but earnings in one randomly selected period.

Third, the decision of the judge was elicited with the strategy method: she made a decision every period, but the decision was implemented only when the lender had sent the 10 tokens to the borrower *and* the borrower had defaulted. In all other cases, her decision was collected but not implemented. In addition, every period the judge was asked, with no money at stake, to state her beliefs about how many *other* judges in the session (0, 1, 2, 3, or 4) chose to enforce. As we will explain later, in a given period *all judges earned the same amount*, which varied according to the performance of all judges or the performance of the economy in such session. Hence, they had a strong incentive to look at the social history of the economy.

Fourth, every period we asked lenders and borrowers to vote on what they would like judges to decide. The vote took place after lending decisions but before borrowers' and judges' actions. These votes were labeled *opinions* in the instructions and might have payoff consequences for lenders and borrowers by influencing judges' enforcement decisions in the constituency treatments, as will be clear later in the paper. The votes were given with reference to one generic judge in the economy, not specifically with reference to the judge matched with each respondent.

The timeline of decisions in a period is the following:

- Stage 1.* Each lender chooses to lend or not.
- Stage 2.* Everyone observes the number of loans in the economy but not who received them.
Each lender and borrower votes for enforcement or not.
Each judge makes a prediction about how many judges will enforce.
- Stage 3.* Each borrower observes whether she has personally received a loan or not.
If the borrower has received a loan, she decides to comply or default.
Each judge decides to enforce repayment or not in case her borrower defaulted.
- Stage 4.* Everyone observes the period results, which included individual payoffs, all choices implemented in her own group of three subjects, and the "social history" of their economy.

The social history concerns all subjects in the room: the overall number of loans given in the economy and how many of them ended in default; the number of judges that chose to enforce; votes of all lenders and all borrowers; and the average earnings of lenders, borrowers and judges. On the contrary, subjects did not observe individual histories and could not therefore develop

reputations. More specifically, a subject learned the past actions of economy participants in aggregate form and not the individual histories of the people in her group. Our setting therefore allows for lender reactions to aggregate behavior of borrowers but precludes the possibility that individual reputations and relational contracts may develop. Contract enforcement thus relies exclusively on anonymous forces and third party enforcers.³

To sum up, an economy comprised five lenders (ℓ), five borrowers (b) and five judges (j). Every period each participant took two decisions at most:

$$(I_{\ell k}, V_{\ell k}), (I_{bk}, V_{bk}), (I_{jk}, G_{jk}) \text{ for } k = 1, \dots, 5$$

Participants' main decisions were binary $\{0,1\}$ and we represent them through a set of “ I ” variables; for instance $I_{\ell 2} = 1, I_{b5} = 0, I_{j3} = 1$ denotes that lender 2 gave a loan, borrower 5 defaulted, and judge 3 forced the borrower to pay back. More generally, the first subscript of a variable denotes the role, while the second subscript identifies each of the 5 subjects playing each of the 3 roles, $k = 1, \dots, 5$. For lenders and borrowers, the “ V ” variables represent their vote about enforcement, either 0 or 1. For judges, the “ G ” variables are the guesses about the number of other judges deciding for enforcement, and can therefore take any integer value between 0 and 4. For example, when we observe $V_{\ell 2} = 1, V_{b5} = 0, G_{j3} = 3$, this means that lender 2 preferred a generic judge in the economy to enforce; borrower 5 preferred this generic judge to accommodate the borrower's default; and, finally, judge 3 expected that 3 of the other 4 judges in that particular economy would have enforced.

Treatments. The three treatments differed in the compensation of judges. A common feature in all of them was that judges were paid according to their collective performance, hence in a given period they all earned the same amount.

- 1) In *Lender constituency*, judges' payments depended on the agreement between judges' decisions as a group and lenders' votes as a group. More precisely, if the number of judges enforcing was equal to the number of lenders favoring enforcement, judges earned 50 tokens. For every person in disagreement, judges' earnings were lowered by 5 tokens. A judge k earned $\pi_{jk} = 50 - 5 \sum_{k=1, \dots, 5} (I_{jk} - V_{\ell k})$, with a minimum of 25 tokens. Borrowers' votes were ignored.

³ Litigation costs are set to zero in the experiment. Whatever their importance in the field, we believe these costs are secondary to the purpose of this paper.

- 2) In *Borrower constituency*, judges' payments depended on the agreement between judges' decisions as a group and borrowers' votes as a group. More precisely, if the number of judges enforcing was equal to the number of borrowers also favoring enforcement, judges earned 50 tokens. For every person in disagreement, judges' earnings were lowered by 5 tokens. A judge k earned $\pi_{jk} = 50 - 5 \sum_{k=1, \dots, 5} (I_{jk} - V_{bk})$, with a minimum of 25 tokens. Lenders' votes were ignored.
- 3) In *GDP*, judges earned the average of all lenders and borrowers in the economy. Therefore, what mattered was not just the earnings of the specific lender and borrower matched with that judge but the earnings of all 10 of them in the economy. A judge k earned $\pi_{jk} = 1/10 \sum_{k=1, \dots, 5} (\pi_{lk} + \pi_{bk})$, which ranges between 38 and 50 tokens. All votes were ignored.

Within the Borrower constituency and GDP treatments, we also implemented variants where some roles were replaced by pre-programmed computers. We refer to those as “Robot Borrower constituency” and “Robot GDP,” and give a full description in section 5.

After reading the instructions for part 3, subjects completed a quiz on the rules for part 3 and the subjects who made the most mistakes in the quiz were excluded. They received \$10 in addition to their part 1 and part 2 earnings. A session included between 6 and 18 subjects (Table 1). Each subject participated in only one of the sessions between February and April 2006. Recruitment was done mostly in introductory economics classes. A session lasted on average less than two hours, including instruction reading. A participant earned on average \$24, and earnings were paid privately at the end of a session.

3. Theory predictions

In all treatments of part 3 there are multiple subgame perfect Nash equilibria. To derive them, we will assume that all agents are risk neutral and will rate economy outcomes according to what we label “surplus,” defined as the average payoff of all lenders and borrowers minus their initial endowment of 76 tokens (60+16). In an actual economy, this definition of surplus is a partial measure of social efficiency, since judges' earnings are excluded. However, judges are generally a small minority of agents and hence surplus would be a good approximation of the social

efficiency of the economy.⁴ We will define an economy outcome “low surplus” when all lenders save (zero surplus) and “high surplus” when all lenders lend (surplus of 24). The economy can achieve the socially optimal outcome only when all lenders lend.

We obtain three predictions. Predictions 1 and 2 concern agents who maximize their personal earnings. Prediction 3 concerns other-regarding agents.

Prediction 1. *In all treatments, there exists a low-surplus equilibrium.*

We will establish below that there exist a low-surplus equilibrium for the one-shot game in each treatment. As a consequence it remains an equilibrium for the repeated game. In the Borrower constituency treatment, borrowers have “control” over third-party enforcers. In a one-shot game suppose all lenders save, all borrowers’ strategy is to default and all vote for accommodating, while all judges rule to accommodate. This strategy profile is a subgame perfect Nash equilibrium, which is supported through a symmetric pure strategy and generates aggregate outcomes $(\sum_k I_{\ell k}, \sum_k V_{\ell k}), (\sum_k I_{bk}, \sum_k V_{bk}), (\sum_k I_{jk}) = (0, -), (0, 0), (0)$, where the terms in parentheses highlight decisions of lenders (number of loans, number of votes for compliance), borrowers (number of voluntary returns, votes for compliance), and judges (number of enforcement decisions), respectively. The symbol “-” stands for any choice. Payoffs are $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 50$.

Under GDP, when judges take a decision in a one-shot game, their earnings have already been determined. That is the crucial point, determining that, for *any* pattern of lenders’ and borrowers’ decisions, self-regarding judges have no incentive to rule for either enforcement or default. Suppose all lenders save, all borrowers’ strategy is to default and all judges rule to accommodate. This strategy profile is a subgame perfect Nash equilibrium and generates aggregate outcomes $(0, -), (0, -), (0)$ with payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 38$.

Under Lenders constituency, lenders have “control” over third-party enforcers and yet, in a one-shot game, there exists low-surplus equilibrium because judges do not learn the voting outcome until the end of the period. Suppose all lenders save and all vote to accommodate, all borrowers’ strategy is to default and all judges rule to accommodate. This strategy profile is a

⁴ In the GDP treatment, surplus is proportional to the GDP of the economy. In the other two treatments, it is not.

subgame perfect Nash equilibrium and generates aggregate outcomes $(0, 0)$, $(0, -)$, (0) with payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 50$.⁵

Prediction 2. *In all treatments, there exists a high-surplus equilibrium.*

When the game is repeated indefinitely, new equilibria may appear because subjects consider the effect that their current decisions may have on the future decisions of all subjects. At any point in time, subjects can expect that the interaction will continue for at least 6 additional periods because, after the first 20 periods of interaction, we use a random stopping rule with a probability $\delta = 5/6$ of continuing for at least another period.

Before showing the high-surplus equilibria for each treatment note that the best choice for a lender is to lend when her expected payoff is higher than it would be if saving, $E[\pi_{\ell k} | I_{\ell k} = 1] > E[\pi_{\ell k} | I_{\ell k} = 0]$. This condition is satisfied when the enforcement rate in the economy, ER , is more than $ER^* = 58.82\%$. ER is defined as the ratio between the sum of loans returned (both voluntarily by borrowers, $\sum_k I_{bk}$, or after judicial enforcement, $R_j = \sum_k I_{jk}/5$) and the sum of loans given, $\sum_k I_{\ell k}$.⁶

$$ER = [\sum_k I_{bk} + (\sum_k I_{\ell k} - \sum_k I_{bk}) R_j] / (\sum_k I_{\ell k}) \quad [6].$$

We remain agnostic about how these expectations are generated. In equilibrium, however, expectations should be fulfilled.⁷

In the repeated game under Borrower constituency, borrowers need to balance the immediate gain they obtain by voting and inducing judges to accommodate against the future losses this will

⁵ Notice that a design where lenders and borrowers vote, observe the voting outcome, and then choose actions would have reduced the likelihood to end up in a low-surplus outcome. This alternative design provides a commitment technology where legal promises cannot be renege neither by the citizens nor by the State. This technology is rarely available in the field.

⁶ Given that this ratio is used by lenders to estimate their return from lending, $I_{\ell k}$ is at least one and the denominator is always positive. The risk attitude of lenders has a clear impact on the enforcement threshold [6] that is acceptable for giving loans. The more risk averse lenders are, the higher the threshold, $ER^{**} > ER^*$.

⁷ The *Enforcement threshold*, ER^* is

$$E[\pi_{\ell k} | I_{\ell k} = 1] > E[\pi_{\ell k} | I_{\ell k} = 0] \Leftrightarrow 67 E[ER] + 50 (1 - E[ER]) > 60 \Leftrightarrow E[ER] > E[ER^*] = 10/17 \approx 0.5882$$

Lending is profitable when more than about 58.82% of the loans are returned. This enforcement rate can be satisfied with various combinations of borrowers' and judges' choices. For instance, when at least three judges decide for compliance, the threshold is met for any number of voluntary returns. When at least three loans out of five are voluntary returned, the threshold is met for any judicial ruling (also for 3/4, 2/3 or 1/1). When at least two loans out of five are voluntary returned, we need two judges ruling for enforcement, and so on. The enforcement threshold is met ($ER > 58.82\%$) with four loans when two loans are voluntarily returned and one judge rules for enforcement; with three loans, when one loan is voluntarily returned and two judges rule for enforcement; with two loans, when one loan is voluntarily returned and one judge rules for enforcement.

cause if, as a consequence, lenders will stop lending in following periods. Suppose all lenders lend in period 1 and keep lending as long as $ER > ER^*$ and otherwise save forever. Suppose all borrowers' default and three vote for enforcement while three judges rule for enforcement. This strategy profile is a subgame perfect Nash equilibrium in pure strategies and generates in every period aggregate outcomes $(0, -)$, $(0, 3)$, (3) with expected payoffs $E[\pi_l]=60.2$, $E[\pi_b]=39.8$, $\pi_j=50$.

The threat of lenders to switch from lending to saving in future periods unless borrowers ensure lenders a profit will inflict sufficient damage to borrowers to induce enforcement above ER^* . To illustrate this point consider that the present value of borrowers' earnings when they vote to maintain 60% enforcement, i.e. above ER^* , is higher than if (in the current period) they decide to switch to 40% enforcement:

$$\sum_{t=0}^{+\infty} \delta^t E[\pi_{bk} | \Sigma_k V_{bk} = 3] > \sum_{t=0}^{+\infty} \delta^t E[\pi_{bk} | \Sigma_k V_{bk} = 2]$$

$$\left[\frac{3}{5}33 + \frac{2}{5}50\right] + \sum_{t=1}^{+\infty} \delta^t \left[\frac{3}{5}33 + \frac{2}{5}50\right] > 50 + \sum_{t=1}^{+\infty} \delta^t 16 \quad [7].$$

Given that the present value of 1 token received every period in an indefinitely repeated game is $\sum_{t=0}^{+\infty} \delta^t = 1/(1 - \delta)$, the above inequality holds for a continuation probability of $\delta = 5/6$. Hence, if only 40% of borrowers vote to enforce, no borrower would gain.

In the repeated game under GDP, suppose all lenders lend in period 1 and keep lending as long as $ER > ER^*$ and otherwise save forever. Suppose all borrowers' default while all judges rule for enforcement. This strategy profile is a subgame perfect Nash equilibrium and generates in every period aggregate outcomes $(0, -)$, $(0, -)$, (5) with expected payoffs $E[\pi_l]=67$, $E[\pi_b]=33$, $\pi_j=50$. Nobody gains from unilaterally deviating from this strategy profile. The same strategy profile is a subgame perfect Nash equilibrium in the repeated game under Lender constituency.

Prediction 3. *Predictions 1 and 2 still hold when subjects have moderate concerns for equality and for efficiency. High-surplus equilibria will become more focal.*

All treatments admit multiple equilibria. Besides Predictions 1 and 2 there also exist a range of intermediate-surplus equilibria. Self-regarding subjects prefer to end up in a high-surplus equilibria rather than in a low-surplus equilibria no matter if their role is lender or borrower.

Under GDP, the same holds true for judges, while in the other treatments self-interested judges are indifferent between low- and high-surplus equilibria.

When subjects have other-regarding preferences, there may be two effects: first, the ranking of equilibria within the equilibrium set may be different from self-regarding subjects; and second, the equilibrium set itself may change. We will maintain the assumption that all agents have identical preferences, although now they may be other-regarding; i.e., exhibit a preference for efficiency, or equality, or both. A subject has a preference for efficiency if she is willing to pay a cost in terms of personal earnings in order to increase the aggregate earnings in the economy more than such cost.⁸ A subject has a preference for equality if she is willing to pay a cost in terms of personal earnings in order to increase the equality of earnings among the subjects in the economy, in particular to increase the absolute and relative earnings of the poorest persons in the economy (the borrowers).⁹

When everyone has a preference for efficiency, the equilibrium ranking coincides with the Pareto-dominance criterion. Hence, in all treatments lenders, borrowers, *and also judges* strictly prefer high-surplus over low-surplus equilibria. In short, preferences for efficiency reinforce the focality of high-surplus equilibria of Prediction 2.

Notice that low-surplus equilibria exhibit a higher inequality of earnings than high-surplus equilibria, because borrowers, who are the poorest subjects, earn 27% as much as lenders in low-surplus outcomes (16 tokens) and earn between 49% and 100% as much as lenders in high-surplus outcomes (from 33 to 50 tokens). Therefore, inequality averse subjects prefer high-surplus outcomes of Prediction 2 more strongly than self-regarding subjects. The payoff structure of the design was chosen precisely for this reason of increasing the focality of high-surplus outcomes.

In addition, when agents are other-regarding, the equilibrium set may change. We put forward two instances. First, under GDP, self-regarding judges regard all high-surplus outcomes equally, while inequality-averse judges strictly prefer the outcome with the highest earnings for

⁸ One can measure the strength of other-regarding preferences using the willingness to sacrifice personal earnings in order to achieve an other-regarding goal. If a subject is willing to pay 2 units to increase the total earnings of the economy by 10 units, we then say that her “efficiency multiplier” is 5 or lower. If she is willing to pay 3 units to increase the earnings of borrowers by 18 units, we also say that her “equality multiplier” is 6 or lower. The stronger the other-regarding preference, the lower its corresponding multiplier.

⁹ The closest model for our preference specification is Charness and Rabin (2002). The models of Fehr and Smidt (1999) and Bolton and Ockenfels (2000) are also related.

borrowers. As a consequence, other-regarding judges may choose the lowest possible enforcement rate, ER^* , which keeps all lenders lending, in order to favor borrowers at no extra cost to themselves.¹⁰ Second, the low-surplus equilibria of Prediction 1 are still in the equilibrium sets with moderately other-regarding concerns. Only when subjects have “strong” other-regarding preferences the set of low-surplus equilibria in GDP and Borrower constituency may shrink and may even disappear altogether. It may happen if lenders have a strong other-regarding concern and hence are motivated to lend in order to increase equality and/or efficiency.¹¹

In conclusion, the likelihood of achieving a high-surplus equilibrium is unambiguously enhanced if subjects are other-regarding.

4. Main results

We present the main findings below in Results 1 and 2, regarding the surplus generated and in the distribution of earnings. We will report Results 3 through 7 in section 6, which explores possible reasons for these main findings and settle on a bounded rationality explanation.

Result 1. Economy surplus is remarkably different across treatments. In Lender constituency, subjects reach 100 percent of the potential surplus, and in GDP they reach 69 percent, whereas in Borrower constituency, they reach only 10 percent.

Table 4 and Figure 2 provide support for Result 1. We define “economy earnings” as the sum of borrowers’ and lenders’ earnings over all periods and all groups, and compute the “economy surplus” subtracting from the economy earnings the “economy endowments” (that is, the sum over all periods and all groups of the 16 tokens that borrowers receive at the beginning

¹⁰ The change in the GDP equilibrium set is independent from the “strength” of inequality-averse preferences: judges incur no cost to lower the enforcement rate as long as the economy remains in a high-surplus outcome. Moreover, the change is independent from judges having both a preference for equality and efficiency. A similar change may occur in Lender’s constituency. Inequality-averse lenders would drive this change. If a lender votes to accommodate borrowers, she can expect a cost of 3.4 (i.e., $1/5 \cdot 17$), which will result in a gain of 17 for borrowers. There is no sacrifice in terms of efficiency, and the equality multiplier is 5.

¹¹ For a lender, giving a loan entails an “opportunity cost” between 8.75 and 17, but will increase earnings in the economy by 34 and earnings of borrowers by an amount between 17 and 25.75. Given $0 \leq ER < 33/68$, the cost for a

of each period plus the 50 tokens that lenders receive). Judges' earnings are irrelevant in these two indicators.

Both indicators differ widely across treatments. In Lender constituency, the steady-state economy surplus is at its predicted equilibrium level of 100%. In Borrower constituency it is only 10%, close to the low-surplus equilibrium level of 0%, which suggests that subjects coordinated on their least-preferred outcome. In the GDP treatment, subjects, despite facing an equilibrium set similar to Borrower constituency, manage to achieve 69% of the potential surplus, which suggests that they attempt to coordinate on the high-surplus equilibria. These differences are significant at a 5% level using a one-tail Mann-Whitney test.¹²

On these and all the numbered results relative to part 3, we rely on statistics computed with reference to periods 11–20, which best represent the steady state. Data for periods 1–10 show some degree of adjustment over time—mainly by judges in GDP and by lenders in both borrower and lender constituencies. This adjustment makes the contrast between treatments less stark. For periods above 20, comparisons between average values are distorted because the random stopping rule produced sessions of uneven length. Table 4 reports some basic statistics, including those for the “discarded” periods. Table 4 also includes the results for the two “robot” variants, which will be described after Result 4.

Result 2. Borrowers earn their smallest share of economy earnings under Borrower constituency. The share is higher under Lender constituency and GDP. The conclusion is similar when one considers borrowers' absolute earnings instead of the relative share.

Table 4, Figure 3 and Figure 4 support Result 2. Paradoxically, borrowers end up worse off under Borrower constituency when they hold voting rights that command enforcement than in the other treatments. When lenders “control” judges, borrowers' earnings are significantly higher both in absolute and in relative terms. Borrowers' shares are 24.0% under Borrower constituency vs. 33.0% under Lender constituency, and borrowers' absolute earnings are 18.8 vs. 33.0 tokens,

lender is $17(1-ER)$ and the gain for a borrower is $ER(33-16)+(1-ER)(50-16)$. The efficiency multiplier is in the interval [2, 3.9] and the equality multiplier is in [1.5, 1.9].

¹² The *p-values* for different groupings of samples are: GDP vs. borrower constituency, $p = 0.028$; GDP including robots vs. borrower constituency including robots, $p = 0.001$; GDP only robots vs. borrower constituency only

respectively. In the GDP treatment, borrowers also fare well, enjoying a marginal increase in their share of earnings (33.2%) with respect to Lender constituency, but suffering a small decline in their absolute earnings (30.8). These differences are significant at a 5% level using a one-tail Mann-Whitney test.¹³

The following sections explore possible explanations for Results 1 and 2.

5. Experimental design (robots, parts 1 and 2)

In part 1, we elicited the preferences of all subjects with respect to equality and efficiency in a static context, along the lines of Engelmann and Strobel (2004). We showed each subject Table 2 and Table 3 (without the last rows) and asked them to write their decisions on a personal card. Each table presented subjects a choice between alternative allocations of money among three persons (roles 1, 2, and 3). Subjects faced role uncertainty as they made these decisions because roles were assigned randomly at the end of the session. Participants were instructed to choose among options *A*, *B*, and *C* and then *D*, *E*, and *F* as if they knew they were Person 2. When computing earnings, we randomly formed groups and randomly assigned roles. Only the choice of the participant selected as person 2 mattered for deciding her group allocation. The choices of persons 1 and 3 were ignored. Half of the groups were paid according to choices made in Table 2 and the other half to choices in Table 3.

In part 2 we ran a one-shot guessing game in which all subjects had to write a real number between 0 and 100 on their personal decision cards. They were informed that we would randomly form groups of three, and would then compute a target number for each group by taking two thirds of the group average. Within each group, the subject closest to her target number received 6 points, which were evenly split in case of a tie.

At that point, the experimenter collected all decision cards and wrote the results for part 1 and part 2 on the cards, which were returned to the subjects at the end of the session.

robots, $p = 0.05$. Although stark, differences with lender constituency are significant at a 10 percent level only because of the small sample size of lender constituency: vs. borrower constituency, $p = 0.10$; vs. GDP, $p = 0.067$.

¹³ With one exception, the p -values for the comparison on either absolute or relative borrowers' income are the same as for the comparison for market surplus. The exception is that there is no significant difference between lender constituency and GDP.

Regarding predictions, the best choice for a rational, self-interested agent in part 1 is C in Table 2 and F in Table 3. In part 2, the Nash equilibrium is to choose the number 0. The prize is split equally and individual earnings are 2 points.

To study issues of coordination and other-regarding motivations, we employed modified treatments where robots replaced humans in some roles of part 3 (Table 1).

- *Robot Borrower constituency* is a variation of the Borrower constituency treatment, where borrowers are humans and lenders and judges are robots. Robot lenders will lend whenever they expect a profit. They base profit expectations on the past average enforcement in the economy. Some lender robots consider only decisions made in the last period, while others consider up to four. Robot judges rule in perfect accordance to borrowers' opinions.
- *Robot GDP* is a variation of the GDP treatment, where judges are humans while lenders and borrowers are robots. Robot lenders are programmed in the same way as in the previous variation. Robot borrowers always default.

The participants knew they were playing against robots. Robots followed deterministic rules, with the exception of robot lenders in period one, which chose based on coin flips. These rules were announced when reading the instructions (Appendix B). These robot treatments considerably simplify coordination problems as there were only five human subjects in the economy. Moreover, in the robot treatments there was common interest among all subjects, as robots were not actually paid. Importantly, other-regarding participants would ignore robot earnings.

6. Explaining our main results

Results 1 and 2 are consistent with theory predictions. Yet, one should explain why in different treatments subjects selected different equilibrium. In particular, one should explain the poor performance of Borrower constituency. We will now analyze three conjectures: (a) difficulties in understanding market interactions; (b) other-regarding preferences; and (c) coordination problems on strategies. This analysis will show that other-regarding preferences and coordination problems do not reasonably explain our main results. Conjecture (a) best explains

the data by employing a steps-of-reasoning model to capture the difficulties in understanding market interactions.

Result 3. The difficulty of actors to make multiple steps of reasoning provides a selection criterion to explain a high-surplus outcome in the GDP and Lender constituency treatments and a low-surplus outcome in the Borrower constituency treatment.

We operationalize the cognitive difficulty of each treatment using two variables: (1) the number of steps of reasoning that the key actors have to make in order to predict aggregate outcomes—specifically, the number of decisions to be made by other subjects and which the key actors have to predict; and (2) the correspondence between the immediate impact and the distant effect of the choices made by the key actors. According to these two variables, we can rank treatments as follow:

- Lender constituency is an “easy” treatment. (1) Lenders are the key actors and face one step of reasoning (i.e. when voting, a lender has to predict how judges will decide). (2) Furthermore, lenders get an immediate benefit from voting for enforcement. Hence there is alignment between the immediate and distant impact of voting for enforcement in the current period.
- GDP is a “moderately difficult” treatment. (1) Judges are the key actors and also face one step of reasoning (i.e. when choosing on enforcement, a judge has to predict lenders’ reactions). (2) Judges face an additional difficulty, however, because their decisions have no immediate effect on earnings—judges’ incentives come from lenders’ reaction in the following periods. Hence there is a partial misalignment between immediate and distant incentives.
- Borrower constituency is a “difficult” treatment. (1) Borrowers are the key actors and face two steps of reasoning. When voting, a borrower first has to predict how judges will decide and, second, how lenders will react to judges’ enforcement decisions. (2) Furthermore, in the high-surplus equilibria, there is a stark conflict between immediate and distant effects, as enforcement could produce an immediate loss and a distant gain—by voting for enforcement, a borrower could indirectly cause an expected negative impact on her period earnings.

When agents do an infinite number of steps of reasoning, the above ranking is irrelevant. Our subjects instead are characterized by a limited number of iterations of reasoning, which we measured in the experiment through the guessing game of part 2. Our classification provides a noisy proxy, although a useful one. As reported in Table 6, about 36% did either zero or one step of reasoning. When subjects do a limited number of steps of reasoning, we predict that every time short-run and long-run incentives of the key actors are misaligned, their choices will be closer to their short-run incentives, which is exactly what we observe in the data (Table 6).

Empirical evidence at the individual level, provided by the regression results shown in Table 7 and Table 8, illustrate that the choices key actors made in part 3 depended on how many iterations of reasoning they completed in part 1. In the GDP treatment, judges with zero iterations of reasoning enforced significantly less than other judges. This pattern holds both for the human and robot variants of the GDP treatment and irrespective of whether we consider all periods of a session or just those where each judge's decision was pivotal for reaching or not reaching the zero-profit enforcement threshold, ER^* (Table 7, columns 1, 2, 5). Judges also learned over time the need to be at least above ER^* .

In the Borrower constituency treatment, borrowers with zero or one iterations of reasoning voted less frequently for enforcement than other borrowers. This result is particularly significant in the robot variant (Table 8, columns 7, 8). There were two interesting differences in comparison with the GDP treatment, however. First, the cutoff was zero iterations for the GDP treatment and one iteration for the Borrower constituency treatment, which directly mirrors the ranking of treatments' cognitive difficulty that we gave previously. Second, there was no evidence of adjustment over time in the Borrower constituency treatment. Although borrowers in Borrower constituency adjusted in the correct direction to the observed voluntary return rates in the economy, the adjustment was insufficient to move into a high-surplus equilibrium, despite the remarkable incentives to do so. Although based on a small sample size, these regressions exhibit an overall pattern that points toward the difficulty subjects have in understanding the systematic consequences of their immediate choices within a market mechanism. This behavior may be related with the failure to backward induct, which has been documented among others by McKelvey et al. (1992) and Binmore et al. (2002). Depending on the institutional arrangement, some of the outcomes in the equilibrium set were easier for subjects to achieve than others.

Alternative conjectures (b) and (c) are presented below in Results 4, 5, 6, and 7. Overall, they do not provide a satisfactory explanation for the main results.

Result 4. Only 21% of participants were strictly self-regarding. The others showed either a concern for equality of earnings, a concern for overall efficiency, or both.

Table 2 and Table 3 provide support for Result 4, which refers to all participants in part 3 (21% is the proportion of subjects choosing *C* in Table 2 and *F* in Table 3). The issue is whether these widespread other-regarding preferences for static outcome had an impact on part 3 equilibrium selection. Remember that in the design of part 3 all motivations—self-interest, inequality aversion, efficiency concern—make “saving” decisions the worst possible outcome. Such design of part 3 also generates an intriguing trade-off between short-term and long-term outcomes, which puts to test subjects’ ability to achieve the best outcome.¹⁴ Tables 7 and 8 present regressions of the choices key actors made in part 3 depending on their other-regarding attitudes. In the GDP treatment, other-regarding preferences of judges had a minimal impact on their decision to enforce contracts (Table 7). On the contrary, in the Borrower constituency treatment, borrowers with a strong concern for equality and efficiency (strongly other-regarding) voted more often against enforcement (Table 8).

Result 5. Other-regarding preferences do not drive the differences across treatments in terms of economy surplus and distribution of economy earnings. When some roles are replaced by pre-programmed robots, the differences between Borrower constituency and GDP treatments remain equally strong.

Results are displayed in Figures 2-5 and Table 4. In the robot treatments we still observed the paradox that borrowers’ absolute earnings were lower in Robot Borrower constituency than in robot GDP (Table 4). Borrowers in Robot Borrower constituency could have imitated the enforcement strategy followed by judges in GDP and achieve higher payoffs, but they did not.¹⁵

¹⁴ An initial endowment equal for all roles, say 20 tokens, can generate ambiguity in the interpretation of the results. Borrowers may comply and judges enforce because they are inequity averse, not because they understand the long-term consequences of their actions. Our design generates cleaner evidence.

¹⁵ Specific other-regarding preferences cannot explain the results either. If subjects care only about the 50/50 outcome, borrower constituency still fare badly in comparison to GDP (an average of 0.33 loans per period versus

Consider the distance in terms of economy surplus between Borrower constituency and GDP. This distance is 59 (human) vs. 57 (robot) percentage points, which is very similar. The distance in terms of share of economy earnings for borrowers is 9 (human) vs. 7 (robot) percentage points, which is also very similar.

In Borrower constituency, economy surplus increases from 10% in the human variation to 40% in the robot variation (Table 4). The main reason for this increase in absolute levels lies in our choice of backward-looking robot lenders, which allowed borrowers to sustain a pattern of cycles of enforcement/lending switching into no enforcement/no lending and back to enforcement/lending throughout a session (Figure 6). Because of their design, robot lenders could be fooled throughout a whole session, while even the most optimistic human lenders seem to have understood the strategy after one or two cycles, and completely stopped giving loans (Figure 7).¹⁶ As a consequence, while economy surplus was stable over time with robot lenders, it steadily declined with human lenders.

As conjecture (b) is not supported by the data, we turn to conjecture (c). Given the multiplicity of equilibria, subjects may coordinate their choices poorly. We identify an “alpha” and a “beta” coordination issue. Subjects’ choices could be badly coordinated with other subjects playing the same role, a failure that may affect borrowers’ voting in Borrower constituency and judges’ enforcement choices in GDP (alpha coordination). Result 6 addresses this issue. Moreover, within the Borrower and Lender constituency treatments, judges could suffer a coordination failure in enforcement choices while matching the voting behavior (beta coordination). Result 7 addresses this issue.

Result 6. When some roles are replaced by pre-programmed robots, subjects readily solve any coordination issue.

In the two robot variants beta coordination was not an issue and results suggest that subjects successfully solved the alpha coordination issue. In the robot GDP sessions, judges learn to

0.9). If borrowers are spiteful toward lenders, that could explain the results only when assuming an unrealistically high level of spite, because in the steady state a borrower must pay 17 tokens to lower a lender’s earnings by 7 tokens. Evidence from other experiments (Levine, 1998) suggests that spite should not reach the degree required to take such action.

coordinate on a high-surplus equilibrium (Figure 7). Their task is comparable to the one in the GDP variant with all human subjects. In the Robot Borrower constituency, although borrowers may appear erratic, they are actually coordinating on a more sophisticated pattern of cycles (Figure 6). In particular, borrowers behave anti-cyclically toward robot lenders. When robot lenders have given many loans, borrowers vote less frequently for compliance. Eventually the loans dry up, borrowers increasingly vote for compliance again, and then robot lenders start giving loans once more. We now turn to beta coordination issues.

Result 7. Our judges responded well to the incentives provided by the institutional setup, increasing over time the enforcement level in the GDP treatment and ruling closely to the voting of the relevant constituency in each of the constituency treatments.

Support for Result 7 is shown in Table 5. The judges in our experiment decide very differently in each of the three treatments, adapting rather well to the different incentives given by each of them. First, under GDP, judges perform poorly in the first periods (they enforce on average 49.3% in periods 1–10) but learn to enforce transactions over time (the rate of enforcement increases to 69.3% in periods 11–20), sustaining exchanges and increasing the earnings of all participants. Second, under Borrower and Lender constituencies, judges on average rule closely following the opinions of their constituency.¹⁷ The transmission is perfect in Lender constituency (i.e., judges always enforce), while it is close in Borrower constituency, with averages of 1.5 borrowers voting for enforcement and 1.87 judges ruling for enforcement. This discrepancy would bias results in favor of the high-surplus equilibrium and hence judges in Borrower constituency cannot be blamed for the low-surplus results.

In summary, the best explanation for the main results is conjecture (a), which concerns participants' difficulties in understanding market interactions.

¹⁶ The correlation coefficients between number of loans and borrowers' opinions *in the same period* are -0.29 in borrower constituency and -0.44 in robot borrower constituency. Borrowers know the number of loans given in the period before stating their opinion about enforcement.

¹⁷ Furthermore, adjustment over time seems to happen faster than in the GDP treatment.

7. Conclusions

We designed an experiment to examine how different political and judicial institutions may fail to produce enforcement and thus make impersonal exchange impossible. With enforcement, the market flourishes, without enforcement, the market disappears. We argue that this variability is caused by the difficulty of the social problem defined by each set of institutions. Our institutions allocate enforcement rights to different classes of people—classes that are defined by their role as parties to a credit transaction, and for which understanding the systemic consequences of enforcement is more or less difficult. We observe that those institutions allocating enforcement rights to parties facing an “easy” problem with respect to enforcement are successful in supporting welfare-improving transactions. On the contrary, markets disappear when the institutions allocate enforcement rights to parties facing a more serious problem with respect to enforcement, and this is irrespective of how much these parties could benefit from sustaining transactions.

The inspiring story for our experiment was a credit market with two transacting parties and a third-party enforcer. A series of anonymous transactions take place between rich lenders and poor borrowers, with pairs meeting at random in the economy. If no transaction takes place, a lender earns more than three times as much as a borrower. Each bilateral transaction always generates a surplus; hence, the economy reaches full efficiency when everyone completes a transaction. Each lender can lend to a borrower, and when the borrower voluntarily returns the loan, the surplus is split, with most of it going to the borrower. After this mutually beneficial transaction, inequality is reduced, with a borrower holding about half the wealth of a lender. When a borrower defaults, the judge (third party) can either force the borrower to repay the loan or accommodate the default. If the judge enforces repayment, the outcome is as with voluntary return. Instead, if the judge accommodates the default, the lender takes a net loss of the principal and the final earnings of the borrower are equal to those of the lender. In the economy, there is a panel of judges and every default is assigned randomly to one judge. Hence, when assessing the expected enforcement rate, lenders must consider the decisions of all judges in the panel.

Lenders enjoy freedom to transact, as they are free to lend or not. Our judges also enjoy full discretion, as they are always free to enforce repayment or not, although they are paid differently depending on the institutional arrangement. We consider three alternative arrangements in which

we allocate enforcement rights to different parties: in the Lender constituency treatment, judges are paid according to lenders' average voting on enforcement; in the Borrower constituency treatment, according to borrowers' average voting; and in the GDP treatment, according to the earnings of all lenders and borrowers in the economy. The key actors in each treatment—those to whom we allocate enforcement rights—are therefore voting lenders, voting borrowers, and judges, respectively.

In all treatments, these key actors always face individual incentives in line with high enforcement and full efficiency; however, the experimental results are mixed. Lenders did vote for enforcement and the economy reached full efficiency in the Lender constituency treatment. Judges in the GDP treatment did enforce and ended up approaching full efficiency. In contrast, borrowers overwhelmingly voted not to enforce and, as a consequence, judges did not enforce in Borrower constituency, so efficiency was extremely low in this treatment. Borrowers remained in an equilibrium where, paradoxically, their earnings were lower and income inequality higher than in Lender constituency.

We claim that these different results are driven by the varying difficulty of the problems that the key actors face across treatments. Lenders voting on enforcement face an easy task because voting for enforcement benefits them immediately and also helps sustaining future transactions. Therefore, immediate and systemic consequences coincide. In contrast, these two consequences go in opposite directions in Borrower constituency, where, when asked to vote on enforcement, borrowers face a tradeoff between an easy-to-see immediate profit and a future systemic benefit.

Our claim that the differences observed in enforcement levels are caused by differences in the difficulty of the problem created by each institutional arrangements is based on three considerations. First, concerns for efficiency or equality should move borrowers to vote for enforcement instead of accommodation. Second, when facing robot players instead of human agents, results are comparable. Results from robot treatments allow us to rule out a determinant role of other-regarding preferences and to discard coordination failure as an alternative explanation. Finally, we report econometric evidence linking decisions and steps-of-reasoning abilities at the individual level. We observe that those subjects iterations of reasoning in a separate task are the least likely to favor enforcement.

The results are striking because all our decision makers, including borrowers, have incentives to enforce. In other words, all are interested in extending the market. We can therefore conclude

that on our setup incentives are not an exhaustive criterion to design market-enforcing institutions. The key actors must also face a task they can handle easily. Consequently, the functioning of an impersonal market is fragile because some institutions pose agents problems that are too difficult, and their poor understanding of the systematic consequences of their decisions leads to enforcement failures that destroy exchange opportunities.

The experimental methodology allows us to focus on enforcement of impersonal trade and remove many real-world details that could otherwise confound our findings. In particular, it allows us to rely on complete contracts, restricting the role of our judges to squarely enforcing the terms of the exchange without playing any role in defining them *ex post*. It also allows us to focus on third-party enforcement, ruling out self-enforcement and relational contracts.

It is tempting to establish parallelisms between our treatments and different allocations of voting and enforcement rights in the field. At their most general, our treatments might be suggestive of different forms of democracy, in which the third-party enforcers (either the government, the judiciary, or both) are directly controlled by different social groups. Our results could thus contribute to the literature on the links between democracy, the rule of law, and growth (Barro, 1996). They also hint that certain forms of education might promote growth by alleviating the enforcement problem.

When extrapolating from the experiment, one must keep in mind, however, the *implicit* set of assumptions about reality embedded in the specificities of the experimental design. In particular, our design of Borrower constituency could resemble a malfunctioning democracy, but future work may reveal that the poor performance of Borrower constituency is reversed by allowing communication, or by having borrowers vote before lenders decide to lend or not, or by letting borrowers implement some commitment device.

Our GDP treatment can itself be interpreted as a commitment device because experimental voters cannot change the role and compensation of enforcers. It therefore resembles societies with effective separation of powers, especially those with (1) an independent judiciary where judges' careers are uncoupled from the short-term desires of their constituencies; and (2) voters who are quite responsive to economic performance. In the field, both of these institutions show varying performance, however, and this diversity might also result in the experiment if judges' compensation were modified to resemble fixed judicial salaries or short-term political horizons.

Last, our design of Lender constituency apparently resembles an elitist democracy or an oligarchy of the sort prevalent in the 19th century or more recently in some Asian countries. It is left for future work to determine, however, the extent to which outcomes of the Lender constituency treatment depend on our implicit assumptions about enforcement. Our experimental subjects enjoy decision rights on the enforcement of contracts but cannot modify the endowments, because we implicitly assume perfect and cost-free enforcement of property rights (that is, endowments are not expropriable by political action). Were we to introduce more consistent assumptions about imperfect enforcement of both contractual and property rights, one may conjecture a tradeoff between both imperfections, making Borrower constituency more effective and Lender constituency less effective than under our assumptions.

References

- Alston, Lee J. "Farm Foreclosure Moratorium Legislation: A Lesson from the Past." *American Economic Review*, 1984, 74(3), pp. 445-457.
- Berg, Joyce; Dickhaut, John and McCabe, Kevin. "Trust, Reciprocity, and Social History." *Games and Economic Behavior*, 1995, 10(1), pp. 122-142.
- Barro, Robert J. "Democracy and Growth," *Journal of Economic Growth*, 1996, 1(1), pp. 1-27
- Binmore, Ken, John McCarthy, Giovanni Ponti, Larry Samuelson, and Avner Shakede (2002) "A Backward Induction Experiment." *Journal of Economic Theory*, 104, 1, 48-88.
- Bohnet, Iris; Frey, Bruno S. and Huck, Steffen. "More Order with Less Law: On Contract Enforcement, Trust and Crowding." *American Political Science Review*, 2001, 95(1), pp. 131-144.
- Bolton, Gary E. and Ockenfels, Axel. "ERC: A Theory of Equity, Reciprocity, and Competition." *American Economic Review*, 2000, 90(1), pp. 166-93.
- Brown, Martin; Falk, Armin, and Fehr, Ernst. "Relational Contracts and the Nature of Market Interactions." *Econometrica*, 2004, 72(3), pp. 747-80.
- Camerer, Colin F. *Behavioral Game Theory: Experiments in Strategic Interaction*, Princeton: Princeton University Press, 2003
- Charness, Gary and Rabin, Matthew. "Understanding Social Preferences with Simple Tests." *Quarterly Journal of Economics*, 2002, 117(3), pp. 817-69.
- Costa-Gomes, Miguel A. and Crawford, Vincent P. "Cognition and Behavior in Two-Person Guessing Games: An Experimental Study." *American Economic Review*, 2006, 96(5), forthcoming.
- de Soto, Hernando. *The Other Path: The Invisible Revolution in the Third World*. New York: Harper & Row, 1989.
- de Soto, Hernando. *The Mystery of Capital: Why Capitalism Triumphs in the West and Fails Everywhere Else*. New York: Basic Books, 2000.
- Engelmann, Dirk and Strobel, Martin. "Inequality Aversion, Efficiency, and Maximin Preferences in Simple Distribution Experiments." *American Economic Review*, 2004, 94(4), pp. 857-69.

- Engle-Warnick, Jim and Slonim, Robert. "Inferring Repeated-Game Strategies from Actions: Evidence from Trust Game Experiments." *Economic Theory*, 2006, 28(3), pp. 603-632.
- Fehr, Ernst and Schmidt, Klaus M. "A Theory of Fairness, Competition, and Cooperation." *Quarterly Journal of Economics*, 1999, 114(3), pp. 817-68.
- Field, Erica and Torero, Maximo. "Do Property Titles Increase Credit Access Among the Urban Poor? Evidence from a Nationwide Titling Program." Harvard University Department of Economics, *mimeo*, March 2006.
- Galiani, Sebastian and Schargrotsky, Ernesto. "Property Rights for the Poor: Effects of Land Titling." Universidad Torcuato Di Tella Business School, *Working Paper*, March 13, 2006.
- Granovetter, Mark. "Economic Action and Social Structure: The Problem of Embeddedness." *American Journal of Sociology*, 1985, 91(3), pp. 481-510.
- Guthrie, Chris; Rachlinski, Jeffrey J. and Wistrich, Andrew J. "Inside the Judicial Mind." *Cornell Law Review*, 2001, 86(4), pp. 777-830.
- Hoffman, Elizabeth, McCabe, Kevin A. and Smith, Vernon L. "What Makes Trade Possible?," in Francesco Parisi and Vernon L. Smith (eds.), *The Law and Economics of Irrational Behavior*, Stanford CA, Stanford University Press, 2005, pp. 165-185.
- Levine, David K. "Modelling Altruism and Spitefulness in Experiments." *Review of Economic Dynamics*, 1998, 1(3), pp. 593-622.
- McKelvey, Richard D. and Palfrey, Thomas R. (1992). "An Experimental Study of the Centipede Game." *Econometrica*, 60, 4, 803-36.
- McNeil, Barbara J.; Pauker, Stephen G.; Sox, Harold C. and Tversky, Amos. "On the Elicitation of Preferences for Alternative Therapies." *New England Journal of Medicine*, 1982, 306(21), pp. 1259-62.
- Mnookin, Robert H. and Kornhauser, Lewis. "Bargaining in the Shadow of the Law: The Case of Divorce." *Yale Law Journal*, 1979, 88(5), pp. 950-97.
- Nagel, Rosemarie. "Unravelling in Guessing Games: An Experimental Study." *American Economic Review*, 1995, 85(5), pp. 1313-26.
- North, Douglass C. *Institutions, Institutional Change and Economic Performance*, Cambridge: Cambridge University Press, 1990.
- North, Douglass C. and Thomas, Robert Paul. *The Rise of the Western World: A New Economic History*, Cambridge: Cambridge University Press, 1973.

Seabright, Paul. *The Company of Strangers: A Natural History of Economic Life*, Princeton and Oxford: Princeton University Press, 2004.

Tabarrok, Alexander and Helland, Eric. "Court Politics: The Political Economy of Tort Awards." *Journal of Law and Economics*, 1999, 42(1), pp. 157-88.

Tversky, Amos and Kahneman, Daniel. "Judgment under Uncertainty: Heuristics and Biases," *Science*, 1974, 185(4.157), pp. 1124-1131

Westen, Drew; Blagov, Pavel S.; Harenski, Keith; Kilts, Clint and Hamann, Stephan. "Neural Bases of Motivated Reasoning: An fMRI Study of Emotional Constraints on Partisan Political Judgment in the 2004 U.S. Presidential Election." *Journal of Cognitive Neuroscience*, 2006, 18(11), 1947-58.

Williamson, Oliver E. "Comparative Economic Organization. The Analysis of Discrete Structural Alternatives." *Administrative Science Quarterly*, 1991, 36(2), pp. 269-96.

Table 1. Experimental treatments

<i>Treatments</i>	<i>Use of robots</i>	<i>Date</i>	<i>Number of participants in parts 1 and 2</i>	<i>Participants in part 3</i>	<i>Length of part 3 (periods)</i>
Lender constituency	None	12 Apr 06	18	15	37
		18 Apr 06	18	15	29
Borrower constituency	None	2 Mar 06	15	15	22
		7 Mar 06*	18	15	37
		8 Mar 06	18	15	25
	Robot lenders and robot judges	3 Mar 06	6	5	30
		23 Feb 06*	6	5	20
		26 Apr 06	6	5	29
GDP	None	28 Feb 06	15	15	27
		1 Mar 06	15	15	24
		9 Mar 06	18	15	22
		19 Apr 06	18	15	23
	Robot lenders and robot borrowers	19 Feb 06	6	5	35
		21 Feb 06	6	5	20
		24 Apr 06	6	5	23
		Totals	15 sessions	189	165

Notes: part 1: Static preferences for efficiency and equity; part 2: Guessing game; part 3: Judicial enforcement of transactions. * A \$4 show-up fee was paid in these sessions and no show-up fee was paid in all other sessions.

Table 2. Preferences for equality

	<i>Earnings options</i>		
	<i>A</i>	<i>B</i>	<i>C</i>
Person 1	8.0	11.0	12.0
Person 2 (<i>decision-maker</i>)	8.0	8.5	9.0
Person 3	8.0	4.5	3.0
Total points	24	24	24
Frequency of choices, N = 165	63 38.2%	22 13.3%	80 48.5%

Notes: Self-regarding agents should choose *C*.
The table includes subjects that participated in part 3 only.

Table 3. Preferences for efficiency

	<i>Earnings options</i>		
	<i>D</i>	<i>E</i>	<i>F</i>
Person 1	20.5	12.0	7.5
Person 2 (<i>decision-maker</i>)	6.5	7.0	7.5
Person 3	5.0	5.0	5.0
Total points	32	24	19
Frequency of choices, N = 165	52 31.5%	30 18.2%	83 50.3%

Notes: Self-regarding agents should choose *F*.
The table includes subjects that participated in part 3 only.

Table 4. Economy surplus and the distribution of economy earnings by treatment

	<i>GDP</i>	<i>Borrower constituency</i>	<i>Lender constituency</i>	<i>Robot GDP</i>	<i>Robot Borrower constituency</i>
Economy surplus (min 0%, max 100%):					
Periods 1–10	57.5%	23.3%	86.0%	68.0%	38.0%
Periods 11–20	69.0%	10.0%	100.0%	96.7%	40.0%
Session average*	66.2%	13.1%	95.8%	86.4%	37.6%
Borrowers' share of economy earnings (min 21%, max 50%):					
Periods 1–10	34.4%	27.8%	33.3%	32.7%	28.4%
Periods 11–20	33.2%	24.0%	33.0%	36.9%	30.0%
Session average*	34.1%	24.9%	33.1%	36.0%	29.1%
Absolute earnings of borrowers (min 16, max 50 tokens):					
Periods 1–10	30.96	22.69	32.15	30.28	24.39
Periods 11–20	30.79	18.83	33.00	36.63	25.75
Session average*	31.37	19.68	32.74	34.78	24.84

Notes: Surplus is defined as the average payoff of all lenders and borrowers minus their initial endowment of 76 tokens (60+16). The average number of loans can be obtained by dividing the economy surplus by 20%; i.e., 100%/20% equals 5 loans. * All periods.

Table 5. Levels of enforcement decisions by judges and voting by lenders and borrowers

	<i>GDP</i>	<i>Borrower constituency</i>	<i>Lender constituency</i>	<i>Robot GDP</i>	<i>Robot Borrower constituency</i>
<i>Enforcement rate, both voluntary and judicial: †</i>					
Periods 1–10	49.3%	33.8%	90.2%	71.0%	35.3%
Periods 11–20	69.3%	37.0%	100.0%	73.9%	36.6%
Session average	62.2%	38.5%	97.3%	70.9%	35.7%
<i>Voluntary compliance by borrowers:</i>					
Periods 1–10	7.3%	8.9%	22.2%	0.0%	14.4%
Periods 11–20	13.7%	13.3%	11.0%	0.0%	23.2%
Session average	10.9%	15.3%	17.1%	0.0%	19.5%
<i>Judges' enforcement and lenders' and borrowers' voting</i>					
Judges enforcing	3.10	1.87	5.00	3.73	1.93
Lenders voting for enforcement	4.43	3.27	5.00*	n/a	n/a
Borrowers voting for enforcement	0.85	1.50*	0.70	n/a	1.93
<i>Equal borrower/lender outcome: ^^</i>					
Periods 1–10	1.53	0.80	0.45	0.80	0.97
Periods 11–20	0.90	0.33	0.00	1.23	1.17
Session average	1.21	0.43	0.14	1.21	1.05

Notes: † The lender zero-profit threshold is $ER^* = 58.8\%$. * Votes had payoff consequences for judges.

^^ Average number of 50/50 split earnings per period.

Table 6. Guessing game and iterations of reasoning

<i>Iterations of reasoning</i>	<i>Choice in the guessing game</i>	<i>Number of subjects</i>	<i>%</i>
0	[66.67, 100]	23	13.9%
0, 1	(44.45, 66.67]	37	22.4%
0, 2	(26.63, 44.45]	64	38.8%
0, 3 or more	[0, 26.63]	41	24.9%
Totals	[0, 100]	165	100%

Notes: Choices from part 2 of the experiment, classified as in Nagel (1995), with all subjects that participated in part 3 included. Each subject had to guess a target equal to two thirds of the average of three real number targets guessed by herself and two other subjects between 0 and 100. The Nash equilibrium is zero. The average guess was 39.90.

The best response to that average guess is 22.80, which is classified in our “3 or more” category.

Table 7. Judges' decisions in the GDP treatment

(Dependent variable: 1 = judge ruled for enforcement, 0 = otherwise)

Independent variables:	With humans as lenders and borrowers				With robots as lenders and borrowers	
	All periods	Pivotal periods only	All periods	Pivotal periods only	All periods	All periods
	(1)	(2)	(3)	(4)	(5)	(6)
Voluntary return rate in previous period	-0.549 (0.470)	-	-0.512 (0.438)	-	-	-
Zero iterations of reasoning	-1.821 (0.492) ^{***}	-0.660 (0.545)	-	-	†	-
Zero or one iterations of reasoning	-	-	0.150 (0.375)	0.618 (0.413)	-	-0.888 (0.693)
Strictly self-regarding	0.383 (0.410)	-0.218 (0.501)	0.277 (0.394)	-0.160 (0.441)	-	-
Strongly other-regarding	0.265 (0.332)	-0.316 (0.437)	-0.128 (0.383)	-0.750 (0.413) [*]	-	-
1/period	-2.714 (0.945) ^{***}	-0.577 (0.617)	-2.602 (0.915) ^{***}	-0.600 (0.639)	-1.382 (0.497) ^{***}	-1.135 (0.475) ^{**}
Constant	0.679 (0.367) [*]	0.166 (0.416)	0.396 (0.364)	0.004 (0.441)	1.188 (0.619) [*]	1.330 (0.725) [*]
No. obs.	460	210	460	210	370	390
No. subjects	20	20	20	20	15	15

Notes: Probit regressions (in columns) with robust estimator clustered around individuals. Pivotal periods are those in which each judge's decision was pivotal for reaching or not reaching the zero-profit enforcement threshold, ER^* . Each subject was coded through four dummy variables using part 1 and part 2 choices: zero iteration of reasoning, zero or one iterations of reasoning, strictly-self regarding, strongly other regarding. The latter dummy equals 1 when a subject chose A and D. † Regressor dropped because it perfectly predicted ruling against enforcement (structural zeroes). When (6) is run on only pivotal periods (regression not included in this table), the dummy variable for iterations of reasoning also dropped because of structural zeroes. Session dummies are included in the regression but not reported in the table. Period 1 excluded in (1) and (3) because of the lag regressor. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8. Borrowers' voting in Borrower constituency

(Dependent variable: 1 = borrower voted for enforcement, 0 = otherwise)

Independent variables:	With humans as lenders and judges				With robots as lenders and judges			
	All periods	Pivotal periods only	All periods	Pivotal periods only	All periods	Pivotal periods only	All periods	Pivotal periods only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Voluntary return rate in previous period	-0.231 (0.398)	-	-0.233 (0.398)	-	-0.777 (0.449)*	-	-0.883 (0.496)*	-
Zero iterations of reasoning	-0.511 (0.230)**	†	-	-	-0.677 (0.572)	-1.246 (0.677)*	-	-
Zero or one iterations of reasoning	-	-	-0.015 (0.347)	-0.605 (0.584)	-	-	-1.073 (0.344)***	-1.081 (0.457)**
Strictly self-regarding	-0.093 (0.201)	-0.154 (0.171)	-0.057 (0.192)	-0.204 (0.198)	-	-	-	-
Strongly other-regarding	-0.668 (0.275)**	-1.116 (0.293)***	-0.522 (0.281)*	-1.024 (0.317)***	-	-	-	-
1/period	-0.047 (0.968)	-0.405 (0.546)	-0.032 (0.953)	-0.335 (0.468)	0.687 (0.885)	0.969 (1.184)	0.751 (0.951)	1.079 (1.229)
Constant	-0.637 (0.394)	-0.047 (0.283)	-0.792 (0.366)**	-0.112 (0.258)	0.221 (0.387)	-0.120 (0.476)	0.976 (0.384)**	0.495 (0.524)
No. obs.	405	148	405	160	380	190	380	190
No. subjects	15	15	15	15	15	15	15	15

Notes: Probit regressions (in columns) with robust estimator clustered around individuals. Pivotal periods are those in which each borrower's decision was pivotal for reaching or not reaching the zero-profit enforcement threshold, ER^* . Each subject was coded through four dummy variables using part 1 and part 2 choices: zero iteration of reasoning, zero or one iterations of reasoning, strictly-self regarding, strongly other regarding. The latter dummy equals 1 when a subject chose A and D. † Regressor was dropped because it perfectly predicted ruling against enforcement (structural zeroes). Session dummies are included in the regression but not reported in the table. Period 1 excluded in (1), (3), (5), and (7) because of the lag regressor. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1. Modified trust game

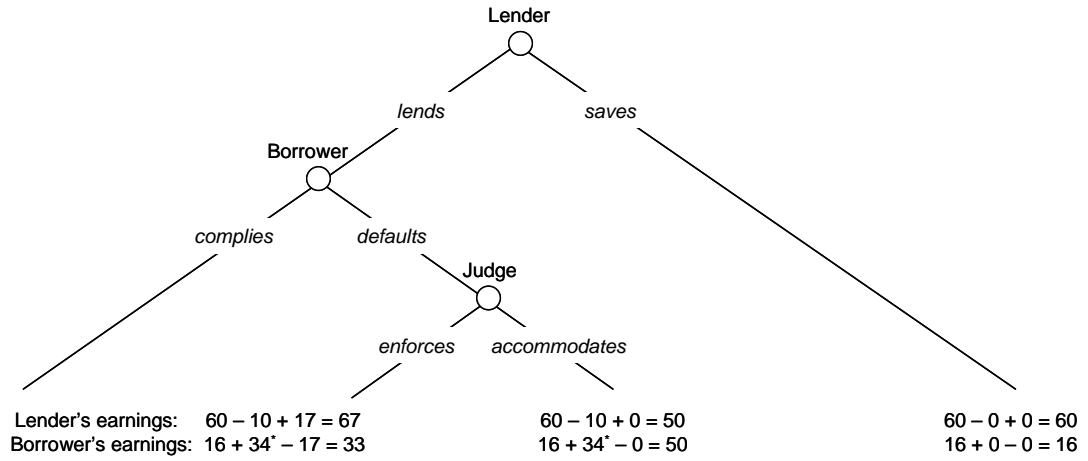
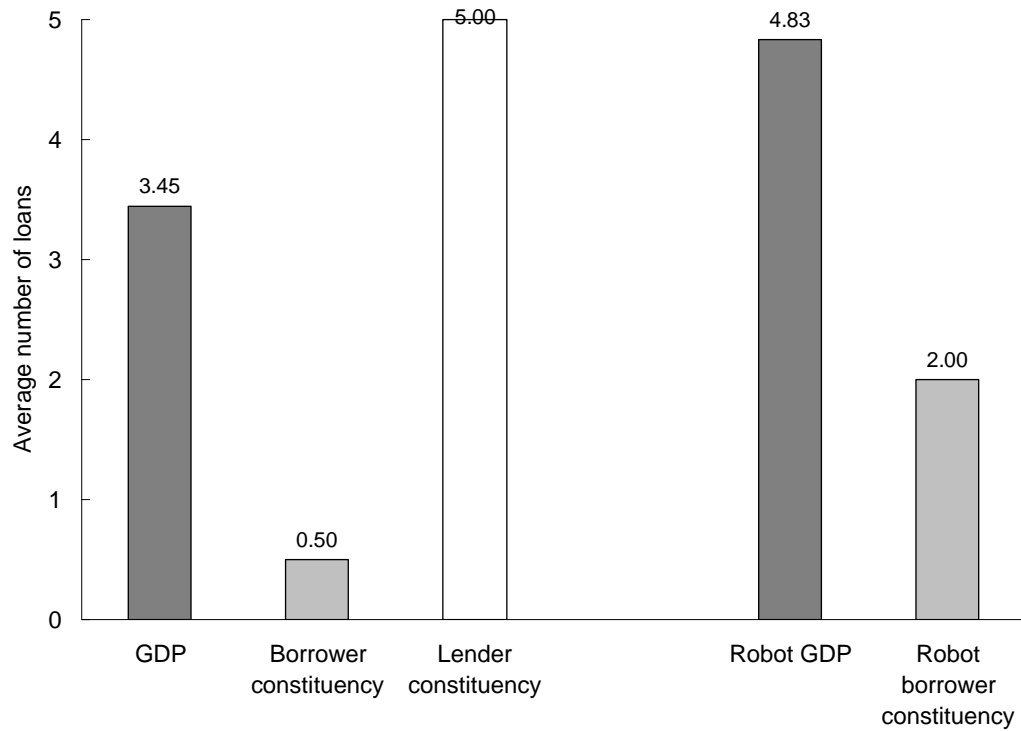
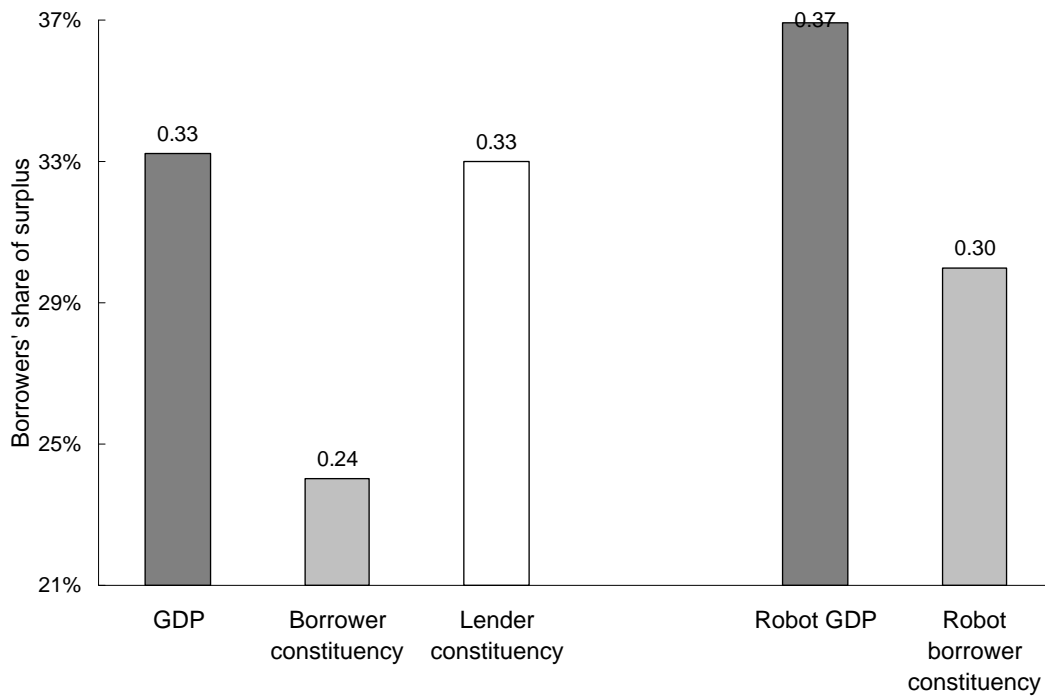


Figure 2. Average number of loans by treatment



Notes: Periods 11–20 only. Aggregate surplus is zero with zero loans and reaches its full potential with five loans

Figure 3. Borrowers' share of total earnings



Note: Periods 11–20 only

Figure 4. Absolute earnings of borrowers

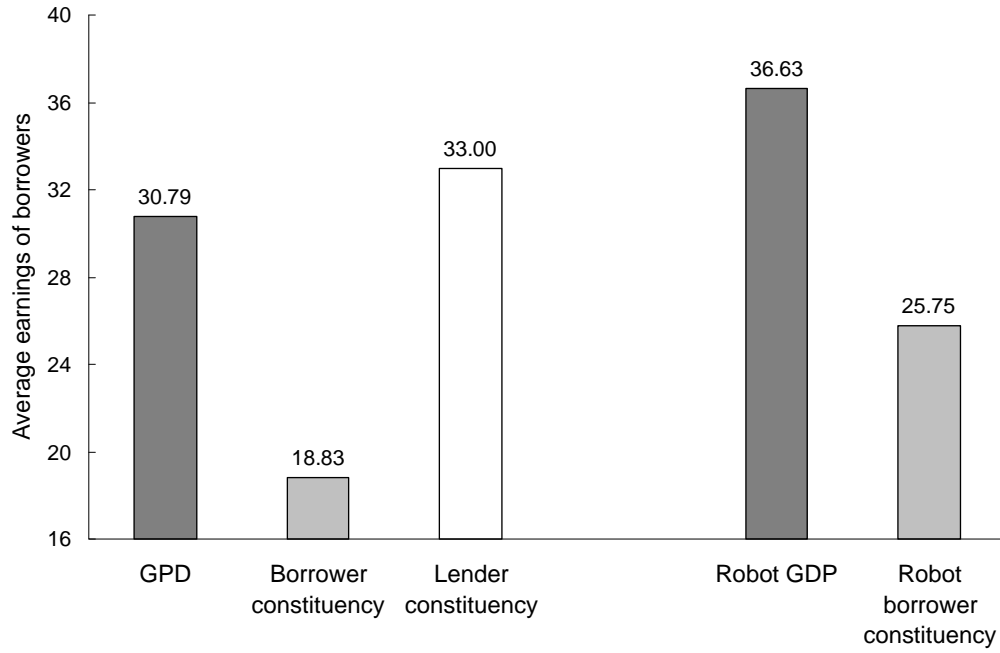
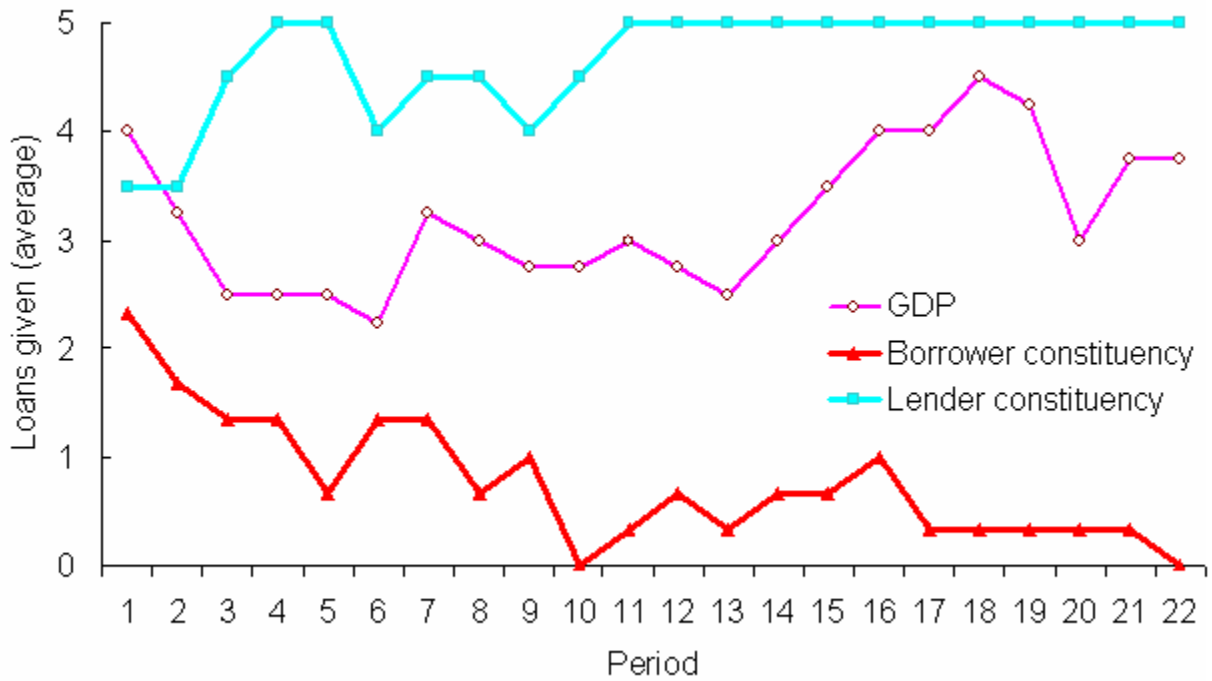


Figure 5. Economy size over time



Note: Shorter session 22 periods, longer session 37 periods. Graph illustrates periods 1-22 only.

Figure 6. Time profile of robot borrower constituency sessions

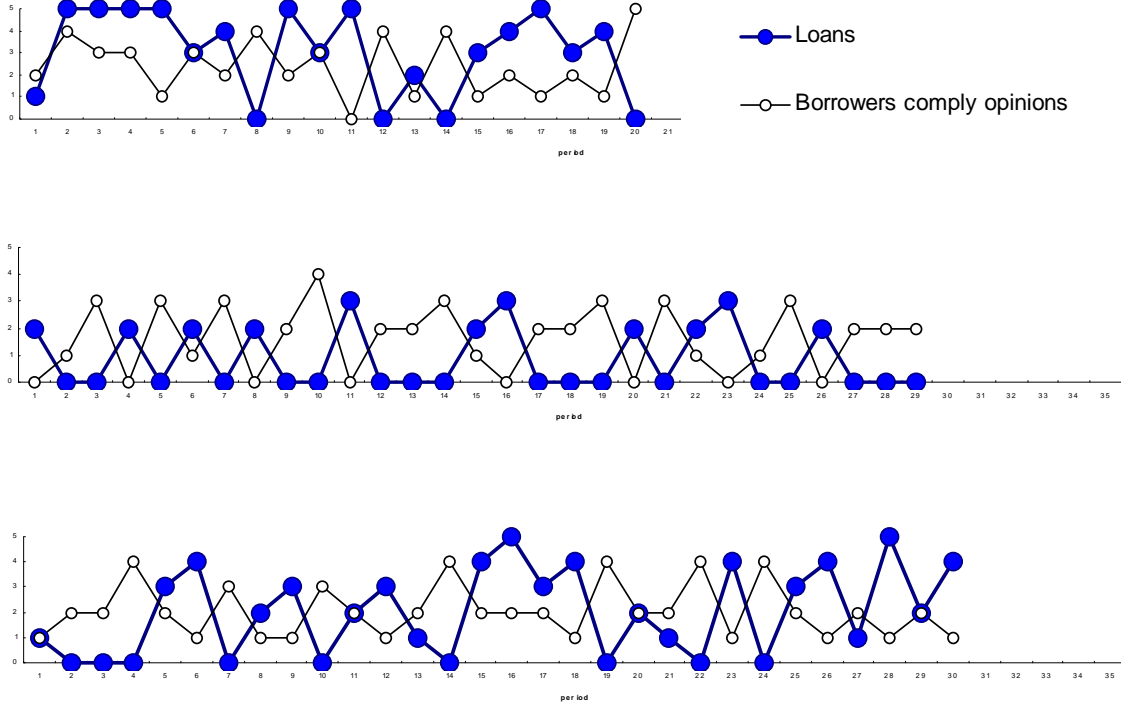


Figure 7. Time profile of robot GDP sessions

