How enforcement institutions affect markets*

Benito Arruñada

and

Marco Casari

This version: April 2007

Abstract

In an experiment we study market outcomes under alternative incentive structures for third-party enforcers. Our transactions resemble an anonymous credit market 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 market 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: Universitat Pompeu Fabra, Department of Economics and Business, Trias Fargas, 25, 08005 Barcelona, Spain, voice: +34 935 422 572, fax: +34 935 421 746, email: benito.arrunada@upf.edu; Casari: Krannert School of Management, Purdue University, 403 West State Street, West Lafayette, IN 47907, USA, voice: + 1 765 494 3598, fax: 765 496 1567, email: casari@purdue.edu. 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 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 possible and has no good substitutes. In fact, it is required 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. Relational contracts are infeasible for anonymous exchange 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 with 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 treatment, 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 market 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 as in Bohnet *et al.* (2001). We also used repeated interactions with random endings (Engle-Warnick and Slonim, 2006). To rule out the feasibility

---

[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, Rachlinksi 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

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 analyzes possible explanations for the main results. Section 6 concludes.

2. Experimental design

Overall, 189 undergraduate students from Purdue University participated in 15 sessions or "economies" (Table 1). Each session included three parts: part 1 measured other-regarding preferences, part 2 measured reasoning ability, and part 3, which constitutes the core of the experiment, had subjects interacting in a trust game with third-party enforcement.

Part 3 was a repeated trust game between a lender (trustor) and a borrower (trustee) to which we added a judge (third-party enforcer, Figure 1). At the beginning of a 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. 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 ignored the identity of the people in their group* (stranger protocol). We will refer to a session as an *economy*.

The modified trust game had the following five features. First, there was a built-in inequality in *minimum* earnings, which were 50 tokens for lenders, 16 for borrowers, and in-between for judges. An experimental token was worth $0.45.

---

dimension of the contract enforcement problem is ultimately defined by the ability of non-expert citizens to understand the problem.

Second, 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). 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.

Third, subjects knew the "social history" of their economy, which includes all subjects in the room, but did not observe individual decisions 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. At the end of each period, each subject observed not only the choices implemented in her own group of three subjects, but also 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; and the average earnings of lenders, borrowers and judges.

Fourth, 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 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 economy (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. Hence, they had a strong incentive to look at the social history of the economy.

Fifth, every period we asked lenders and borrowers to vote on what they would like judges to decide. 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. At the end of each period, the votes of all lenders and all borrowers were announced as part of the social history.[3]

This modified trust game was repeated for at least 20 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. In expectation, this random stopping rule yielded 6 additional periods for a total duration of 26 periods. There were no practice periods. To preserve the inequality in earnings, only one period from part 3 was randomly selected for payment.

Treatments. We introduced three main treatments that manipulated the payment schedule for judges: "lender constituency," "borrower constituency," and "GDP" (Table 1). A common feature of all three treatments was that judges were paid according to their collective performance, hence in a given period they all earned the same amount.

Treatments differed in the compensation of judges:

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. Hence, judges earned a minimum of 25 tokens. Borrowers' votes were ignored.

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. 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. Judges' earnings could therefore vary between 38 and 50 tokens.

---

[3] When voting, lenders and borrowers knew how many loans had been given and their earnings in the current period.

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 the results section.

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 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 decision cards. They were informed that we would randomly form groups of three and would 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. 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. Theoretical predictions

We now derive the theoretical predictions for a rational, self-interested agent. In part 1 the best choice 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. We will now derive the Nash equilibria for a simpler version of the experimental design used in part 3[4]—both for a one-shot game and then for the indefinitely repeated game described in the previous section. Remember that decisions are sequential: first lenders decide, then borrowers, and finally judges. We assume that all agents are risk neutral and maximize their personal earnings. At the end of the section, we will discuss changes in the equilibria of part 3 when agents are risk averse or other-regarding.

3.1. Preliminary considerations

The one-period version of the trust game described in Section 2 comprises five lenders (ℓ), five borrowers (b) and five judges (j). Each participant takes two decisions at most:

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

Participants' main decisions are 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, $k = 1, ..., 5$. 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 prefers a generic judge in the economy to enforce; borrower 5 prefers this generic judge to accommodate

[4] For the theoretical predictions, we assume that (a) when making a choice, each judge knew how lenders and borrowers had voted; and (b) when voting, each borrower knew whether she had received a loan or not. On the contrary, in the experiment (a) judges learned how lenders and borrowers had voted only at the end of each period; and (b) before voting, borrowers did not know whether or not they were receiving a loan unless the number of loans was zero or five.

the borrower's default; and, finally, judge 3 thinks that 3 of the other 4 judges in that particular economy would have enforced.

In short, each participant first makes a choice about lending, compliance, or enforcement, and then states her opinion or guess on enforcement. The payoffs, $\{\pi_{\ell k}, \pi_{bk}, \pi_{jk}$ for $k = 1, ..., 5\}$, which have already been explained in Section 2 and Figure 1, result from the interaction among subjects, taking into account who was matched with whom in the period. For specifying these interactions, we define functions $m(k)$, which map each subject k to the other two subjects interacting with her.[5]

Thus, the payoff of lender k ranges in [50, 67]:

$$\pi_{\ell k} = 60\,(1 - I_{\ell k}) + 67\,(I_{\ell k}\, I_{b,m(k)} + I_{\ell k}(1 - I_{b,m(k)})I_{j,m(k)}) + 50\,(I_{\ell k}(1 - I_{b,m(k)})(1 - I_{j,m(k)}) \qquad [1].$$

The payoff of borrower k ranges in [16, 50]:

$$\pi_{bk} = 16\,(1 - I_{\ell,m(k)}) + 33\,(I_{\ell,m(k)}\, I_{bk} + I_{\ell,m(k)}\,(1 - I_{bk})I_{j,m(k)}) + 50\,(I_{\ell,m(k)}(1 - I_{bk})(1 - I_{j,m(k)})) \qquad [2].$$

The payoff of judge k under lender constituency ranges in [25, 50]:

$$\pi_{jk} = 50 - 5\ \Sigma_{k=1,\ldots,5}\ (I_{jk} - V_{\ell k}) \qquad [3].$$

The payoff of judge k under borrower constituency ranges in [25, 50]:

$$\pi_{jk} = 50 - 5\ \Sigma_{k=1,\ldots,5}\ (I_{jk} - V_{bk}) \qquad [4].$$

And the payoff of judge k under GDP ranges in [38, 50]:

$$\pi_{jk} = 1/10\ \ \Sigma_{k=1,\ldots,5}\,(\pi_{\ell k} + \pi_{bk}) \qquad [5].$$

The economy can achieve the socially optimal outcome only when all lenders lend, as more loans increase the total earnings in the economy. We will rate 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). This surplus, which is a partial measure of social efficiency,

---

[5] When $I_{\ell,m(k)}=0$, we conventionally define $I_{bk}=0$.

since judges' earnings are excluded, will range for each transaction between 24 ("high surplus") when all lenders lend, and 0 ("low surplus") when all lenders save.[6]

The best choice for a lender is to lend when her expected payoff is higher than it would be if saving, $E[\pi_{\ell k}|I_{\ell k} = 1] > E[\pi_{\ell k}|I_{\ell k} = 0]$. Because a lender is anonymously matched with a borrower and a judge in the economy, the expected profitability of lending crucially depends on the expected "enforcement rate" in the economy, *ER*, defined as the ratio between the sum of loans returned (both voluntarily by borrowers or after judicial enforcement) and the sum of loans given:

$$ER = [\Sigma_k I_{bk} + (\Sigma_k I_{\ell k} - \Sigma_k I_{bk}) R_j] / (\Sigma_k I_{\ell k}) \qquad [6].^7$$

The expected return on a loan depends on how many are voluntarily returned by borrowers $(\Sigma_k I_{bk})$ and, for those on default, the enforcement rate by judges, $R_j = \Sigma_k I_{jk} /5$. We remain agnostic about how these expectations are generated. In equilibrium, however, expectations should be fulfilled. Lenders will lend only if they expect an enforcement rate above a certain *Enforcement threshold, ER^*:*

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

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

[6] In the GDP treatment, surplus is proportional to the GDP of the economy. In the other two treatments, it is not. In an actual economy, judges are a small minority of agents and hence surplus would be a good approximation of the GDP of the economy.

[7] Given that this ratio is used by lenders to estimate their return from lending, I_{lk} 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, $ER^{**} > ER^*$. This makes no difference for equilibrium in lender constituency, given the 100% enforcement rate in equilibrium. Instead, it makes some difference in the other two treatments, although not major ones. In borrower constituency, there are still multiple equilibria. More precisely, there is a high-surplus equilibrium, which is unique, although now lenders earn less. Moreover, there are multiple low-surplus equilibria, which are now a larger set than before. Under GDP, the set of high-surplus equilibria gets smaller because $ER^{**} > ER^*$, while the set of low-surplus equilibria gets larger.

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.[8]

3.2. Equilibria in the one-shot game

Although subjects in the experiment face repeated interactions, we first analyze the equilibria for the simpler case of one-period interaction.

In lender constituency, there exists a unique sequential equilibrium where all lenders vote for compliance. It is in the judges' best interest to adhere to the lenders' opinions and rule for compliance. Hence lenders can count on a 100% enforcement rate. It follows that all lenders have an incentive to give loans, while borrowers can choose any action. The key players here are the lenders, because they control the third-party enforcer. We will adopt the following notation to describe the aggregate outcomes in equilibrium, $(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (5, 5), (0, -), (5)$, 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. We omit from the above description judges' guesses about other judges, *G*, because they entail no payoff consequences in any of the treatments. The equilibrium is supported through a symmetric pure strategy and yields a high surplus outcome with payoffs $\pi_\ell = 67$ for lenders, $\pi_b = 33$ for borrowers, and $\pi_j = 50$ for judges.

There is also a unique sequential equilibrium in borrower constituency, but it yields a low-surplus outcome, which is not socially optimal. Now the key players are the borrowers, because they have control of the third-party enforcer. If lenders give loans, borrowers always have an incentive to default and to vote for default. Hence, in equilibrium, lenders do not give loans and judges accommodate. The equilibrium is supported through a symmetric pure strategy and generates aggregate outcomes $(0, -), (0, 0), (0)$ with payoffs $\pi_\ell = 60, \pi_b = 16,$ and $\pi_j = 50$. Given that borrowers would benefit from receiving loans, one may suggest that they could "promise" to vote for loan enforcement. However, such promises are not credible because borrowers and

[8] 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.

judges choose after lenders have already given the loan, and hence a "high-surplus" outcome is not an equilibrium.

Under GDP, there exist multiple equilibria. When judges take a decision, their earnings have already been determined. That is the crucial point. For *any* pattern of lenders' and borrowers' decisions, self-regarding judges have no incentive to rule for either enforcement or default. One could assume that judges will choose randomly because it makes no difference to them. In that case, lending becomes unprofitable because an enforcement probability $r = 0.5 < ER^*$ drives the economy to a low-surplus equilibrium. This equilibrium is supported through a symmetric mixed strategy and generates aggregate outcomes *(0, -), (0, -), (5r)* with payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 38$. Alternatively, one could argue that a judges' equilibrium strategy is always to enforce. In that case, lending is profitable and drives the economy to a high-surplus equilibrium. Judges have no incentives to deviate from this strategy. This equilibrium is supported through a symmetric pure strategy and generates aggregate outcomes *(5, 5), (0, -), (5)* with payoffs $\pi_\ell = 67$, $\pi_b = 33$, and $\pi_j = 50$. Any level of enforcement from 0 through 5 can be an equilibrium.

When judges have a preference for equality, the multiplicity of equilibria under GDP disappears. As we will see later, we find widespread empirical preference for equality of earnings, and that introduces a ranking among outcomes, which leads toward a low-surplus equilibrium. The equilibrium is supported through a symmetric pure strategy where no judge enforces, and generates aggregate outcomes *(0, -), (0, -), (0)* with payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 38$.

3.3. Equilibria in the indefinitely repeated game

If the game is played repeatedly, new equilibria may appear because subjects consider the effect that their current decisions may have on the future decisions of all subjects. After the first 20 periods of interaction, we use a random stopping rule with a probability 1/6 of continuing for at least another period. It follows that the overall expected length of the interaction is 26 periods. At any point in time, however, subjects expect that the interaction will continue for at least 6 periods.

In lender constituency, playing the game repeatedly yields the same high-surplus outcomes as the one-shot scenario, *(5, 5), (0, -), (5)*. A unique sequential equilibrium in symmetric pure strategies still exists.

In borrower constituency, multiple equilibria exist, which yield either high-surplus or low-surplus outcomes. In the repeated game, borrowers need to balance the immediate gain they obtain by voting and inducing judges to accommodate against the future losses this will cause if, as a consequence, lenders will lend less in the following periods.

The high-surplus equilibria exist because in the repeated game, lenders can "threaten" to switch from lending to saving in future periods unless borrowers ensure lenders a profit. The borrowers' tradeoff is such that they benefit from voting for enforcement because lenders would react to the enforcement level going below the critical threshold by executing their threat and halting lending for at least *t* periods. To calculate *t*, let us compare the equilibrium earnings that borrowers get over a future of *T* periods in two situations—when they vote to maintain 100% enforcement and when (in the current period) they decide to switch to 0% enforcement. The following inequality must hold for any generic period,

$$E[\pi_{bk} \mid \Sigma_k V_{bk} = 5] > E[\pi_{bk} \mid \Sigma_k V_{bk} = 0]$$

$$33\,T > 50 + 16\,t + 33\,(T - t) \Leftrightarrow t^* > 50/17 \approx 2.94 \qquad [7].$$

That is, borrowers always profit from voting for enforcement if lenders react to default by halting lending for three or more periods, which is always a viable option because their expected horizon is at least six periods.

Formally, the high-surplus equilibrium in borrower constituency is supported by any mixed strategy with an expected fraction of borrowers voting for enforcement, $r = ER^*$. The aggregate outcomes in equilibrium, *(5, -), (0, 5r), (3)*, have expected payoffs $E[\pi_\ell] \approx 60$, $E[\pi_b] \approx 40$, and $45 < E[\pi_j] \leq 50$.[9] Any $r > ER^*$ is not an equilibrium because there is room for a borrower to switch—sometimes to vote for default and increase her expected earnings. Instead, for any $r < ER^*$, lenders make a loss and will all quit lending.

---

[9] For borrowers employing a symmetric mixed strategy, $r = ER^*$ and for judges adopting an asymmetric pure strategy of 3 always enforcing, $E[\pi_\ell] = 60.20$, $E[\pi_b] = 39.80$, and $E[\pi_j] = 45.83$. In the text, we provide an indicative interval that contains expected payoffs when also using other strategies to support a high-surplus outcome.

The low-surplus equilibria in borrower constituency is supported by strategies with an expected fraction of borrowers voting for enforcement, $r < 33/68$. The aggregate outcomes in equilibrium, *(0, -), (0, 5r), (k)*, have expected payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $45 < E[\pi_j] \leq 50$. The parameter k is the expected number of judges enforcing.[10] The one-shot equilibrium with zero-enforcement is a special case. Notice that for $r \in (33/68, ER^*)$, an equilibrium does not exist, because in this interval, a one-borrower switch in strategy from voting for compliance with probability r to voting with probability one will make lending profitable; hence, $4r+1 < 5 \cdot ER^*$ must hold.

Under GDP, multiple equilibria exist, which yield either high- or low-surplus outcomes. The high-surplus equilibria are supported by strategies with an expected fraction of judges ruling for enforcement at or above the enforcement threshold, $q \in [ER^*, 1]$. The aggregate outcome in equilibrium is *(5, -), (0, -), (5q)* with expected payoffs $60 \leq E[\pi_\ell] \leq 67$, $33 \leq E[\pi_b] \leq 40$, and $\pi_j = 50$.

The low-surplus equilibria under GDP are supported by strategies with an expected fraction of judges ruling for enforcement, q, less than 33/68. The aggregate outcome in equilibrium is *(0, -), (0, -), (5q)* with payoffs $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 38$. The one-shot equilibria with zero-enforcement are a special case. Notice that for $q \in (33/68, ER^*)$, no equilibrium exists. In that interval, a one-judge switch in strategy from ruling for compliance with probability q to probability one will make lending profitable; hence, $4q+1 < 5 \cdot ER^*$ must hold.

3.4. Equilibrium selection criteria

While the equilibrium in lender constituency is unique, we have multiple equilibria in borrower constituency and GDP. We put forward possible selection criteria to solve the coordination problem that characterizes the latter two treatments in the indefinitely repeated setting. First we discuss Pareto-dominance and then the effect of other-regarding preferences.

Under borrower constituency and GDP there are both low- and high-surplus equilibria. If subjects use Pareto-dominance as their focal criterion, they will avoid low-surplus equilibria,

---

[10] That is, $k=0$ for $r \leq 0.1$; $k=1$ for $0.1 \leq r \leq 0.3$; $k=2$ for $r \geq 0.3$. See also footnote 9.

which would leave only one equilibrium in borrower constituency *(5, -), (0, 5r), (3)* with $r = ER^*$, and a multiplicity of high-surplus equilibria in GDP.

When subjects have other-regarding preferences, there may be two effects: first, the ranking of equilibria 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. When everyone has a preference for efficiency, the equilibrium ranking coincides with the Pareto-dominance criterion and the equilibrium sets of the three treatments do not change. In short, preferences for efficiency reinforce the focality of high-surplus equilibria.

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). 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. As we will explain in the following sections, inequality aversion reinforces the focality of high-surplus equilibria in all treatments. In addition, inequality aversion changes the equilibrium set in the GDP treatment.

All low-surplus equilibria exhibit a higher inequality than all high-surplus equilibria, because the poorest subjects (borrowers) always earn 16 tokens in low-surplus outcomes (27% as much as lenders) and earn between 33 and 50 in high-surplus outcomes (49%–100% as much as lenders). Therefore, inequality averse subjects should be more likely than self-regarding subjects to coordinate on a high-surplus equilibrium in both the GDP and borrower constituency treatments.

In addition, when subjects are inequality averse, the set of equilibria for GDP changes and becomes identical to the set in borrower constituency. While self-regarding judges regard all high-surplus outcomes equally, inequality-averse judges strictly prefer the outcome with the highest earnings for borrowers. As a consequence, GDP judges will choose the lowest possible enforcement rate, $r = ER^{*}$, which becomes the only high-surplus equilibria. This change is particularly important because, on one hand, it simplifies the coordination task in GDP and, on the other hand, it makes GDP more comparable to borrower constituency.[11]

Deeper changes in the equilibrium set may occur when all subjects have “strong” other-regarding preferences.[12] The unique equilibrium in lender constituency may shift to a different high-surplus outcome with an enforcement rate lower than 100%, although never to a low-surplus outcome.[13] The set of low-surplus equilibria in GDP and borrower constituency may shrink and may even disappear altogether.[14] In conclusion, the likelihood of achieving a high-surplus equilibrium is unambiguously enhanced if subjects are other-regarding.

4. Main results

We present two sets of results from the experimental data. The main findings are Results 1 and 2, which report the differences obtained across treatments in the surplus generated and in the distribution of earnings, with particular reference to the fact that borrower constituency falls consistently into the low-surplus equilibrium. Second, Results 3 through 7 explore possible reasons for these main findings and settle on a bounded rationality explanation.

---

[11] The change in 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.

[12] 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.

[13] 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.

[14] Lenders with a concern for equality and/or efficiency would drive the change. 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 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].

Result 1. Market 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 “market earnings” as the sum of borrowers’ and lenders’ earnings over all periods and all groups, and compute the “market surplus” subtracting from the market earnings the “market endowments” (that is, the sum over all periods and all groups of the 16 tokens that borrowers receive at the beginning 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 market 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.[15]

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 learning—mainly by judges in GDP and by lenders in both borrower and lender constituencies. This learning 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.

[15] 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 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, $p = 0.067$.

Result 2. Borrowers earn their smallest share of market 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, 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.[16]

5. Explaining our main results

We will now examine three conjectures to explain our main results 1 and 2: (a) other-regarding preferences; (b) coordination problems; and (c) difficulties in understanding market interactions. This examination will show that other-regarding preferences and coordination problems do not reasonably explain our main results. Conjecture (c) best explains the data by employing a steps-of-reasoning model to capture the difficulties in understanding market interactions.

[16] 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.

Result 3. Almost half of our subjects show some concern for equality of earnings and about half have some concern for overall efficiency. Only 21% of them are strictly self-regarding.

Table 2 and Table 3 provide support for Result 3, 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 presence of subjects with other-regarding preferences suggest that these preferences might have some effect on the main results obtained. In addition, as already noted in the theory section, the presence of a large fraction of subjects concerned for equality makes the equilibrium sets for the GDP and borrower constituency treatments identical. Note in particular that in any of the GDP sessions, there were either two or three judges with a concern for equality.[17]

Result 4. Other-regarding preferences affect subjects' choices but do not drive the differences across treatments in terms of market surplus and distribution of market earnings. When some roles are replaced by pre-programmed robots, the differences between borrower constituency and GDP treatments remain equally strong.

Some of the evidence obtained in part 3 might also be driven by other-regarding concerns. For instance, lenders who lend when enforcement rates are well below ER^* could simply be concerned with efficiency or equality (Table 5). Similarly, judges who do not enforce in the GDP treatment could be reflecting their concern for equality.

On the other hand, other choices are hardly consistent with other-regarding concerns. Most importantly, subjects in borrower constituency did not have the high-surplus equilibrium, which is also the most equal, as their focal outcome. In lender constituency, there is also no evidence of other-regarding behavior on the side of either lenders or judges.

In order to fully rule out the impact of other-regarding preferences, we used two variants of the treatments where we kept humans playing as key actors but replaced the other humans with robots:

---

[17] In one session, only two judges were concerned for equality, which implies that there are still multiple high-surplus equilibria, although the set is much smaller than before, [0.588, 0.6] instead of [0.588, 1].

- *Robot borrower constituency*: In a variation of the borrower constituency treatment, we keep humans as borrowers and introduce robot lenders and robot judges. Robot lenders will lend whenever they expect a profit. They base profit expectations on the past average enforcement in the market. 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*: In a variation of the GDP treatment, we keep humans as judges and introduce robot lenders and robot borrowers. Robot lenders are programmed the same as in the previous variation. Robot borrowers always default.

Results are displayed in Figure 2, Figure 3, Figure 4, and in Table 4. In the robot variants, the five subjects in a session are the only ones paid; hence, concerns for efficiency are perfectly aligned with their own self interest, while concerns for other persons should have no impact. Yet, the *differences* between borrower constituency and GDP change only marginally from within human to within robot variant economies. The *differences* in terms of market surplus are 59 (human) vs. 57 (robot) percentage points and in terms of share of market earnings are 9 vs. 7 percentage points. We focus on these differences in order to maintain constant the behavior of lenders, which were humans in some cases and robots in the other cases because, as we explain immediately below, the naivety of our robot lenders is responsible for the higher market surplus achieved by humans in both robot variations.[18]

In borrower constituency, market surplus increases from 10% in the human variation to 40% in the robot variation (Table 4). The main reason for this increase 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 5). 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

[18] 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 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

6).[19] As a consequence, while market surplus was stable over time with robot lenders, it steadily declined with human lenders. In addition, 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. This fact also supports that subjects are boundedly rational with evidence that cannot be contaminated by other-regarding factors.

As conjecture (a) is not supported by the data, we turn to conjecture (b). While lender constituency has a unique equilibrium, the other two treatments have multiple equilibria; hence subjects may coordinate their choices poorly. First, 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. Result 5 addresses this issue. Second, within the borrower constituency treatment, judges could suffer a coordination failure in enforcement choices while matching the voting behavior of borrowers. Result 6 addresses this issue.

Result 5. When some roles are replaced by pre-programmed robots, subjects readily solve any coordination issue.

The two robot variants suggest that subjects playing the same role, judges or borrowers, coordinated successfully. In the robot GDP sessions, judges learn to 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 5). 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.

---

tokens. Evidence from other experiments (Levine, 1998) suggests that spite should not reach the degree required to take such action.

[19] 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.

Result 6. Our judges responded well to the incentives provided by the institutional setup, learning to enforce in the GDP treatment and ruling closely to the voting of the relevant constituency in each of the constituency treatments.

Support for Result 6 is shown in Figure 8 and Table 5. The judges in our experiment decide very differently in each of the three treatments, adapting very 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 the market and increasing the earnings of all participants. Second, under borrower and lender constituencies, judges on average rule closely following the opinions of their constituency, as shown in Figure 8.[20] 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.

After having examined conjectures (a) and (b), we finally look at conjecture (c) in Result 7 below.

Result 7. The difficulty of actors to make multiple steps of reasoning provides a selection criterion to explain a high-surplus outcome in the GDP treatment and a low-surplus outcome in the lender constituency treatment.

Although the main results in the GDP and borrower constituency treatments are compatible with the theoretical predictions of Section 3, subjects' focal outcome was different depending on the institutional arrangement. We argue that the selection criterion is driven by the difficulty actors experience in making multiple steps of reasoning.

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 market 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. 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 5).

---

[20] Furthermore, learning seems to happen faster than in the GDP treatment.

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 learning 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. Depending on the institutional arrangement, some of the outcomes in the equilibrium set were easier for subjects to achieve than others.

6. Conclusions

We designed an experiment to examine how different political and judicial institutions may fail to produce enforcement and thus make market transactions 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 the market. 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 market transactions.

Our experiment simulates 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 the market.

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 (the "It's the economy, stupid" of the first Bill Clinton campaign). 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

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 (http://post.economics.harvard.edu/faculty/field/papers/FieldTorerocs.pdf, accessed October 30, 2006).

Galiani, Sebastian and Schargrodsky, Ernesto. "Property Rights for the Poor: Effects of Land Titling." Universidad Torcuato Di Tella Business School, *Working Paper*, March 13, 2006 (http://200.32.4.58/~eschargr/PropertyRightsMarch132006.pdf, accessed October 30, 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. "Modeling Altruism and Spitefulness in Experiments." *Review of Economic Dynamics*, 1998, 1(3), pp. 593–622.

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 Naturals 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

Treatments	*Use of robots*	*Date*	*Number of participants in parts 1 and 2*	*Participants in part 3*	*Length of part 3 (periods)*
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	26.9 (average)

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

	Earnings options		
	A	*B*	*C*
Person 1	8.0	11.0	12.0
Person 2 *(decision-maker)*	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

	Earnings options		
	D	*E*	*F*
Person 1	20.5	12.0	7.5
Person 2 *(decision-maker)*	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. Market surplus and the distribution of market earnings by treatment

	GDP	*Borrower constituency*	*Lender constituency*	*Robot GDP*	*Robot borrower constituency*
Market 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 market 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 market 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

	GDP	*Borrower constituency*	*Lender constituency*	*Robot GDP*	*Robot borrower constituency*
Enforcement rate, both voluntary and judicial: †					
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%
Voluntary compliance by borrowers:					
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%
Judges' enforcement and lenders' and borrowers' voting					
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
Equal borrower/lender outcome: ^^					
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

Iterations of reasoning	*Choice in the guessing game*	*Number of subjects*	*%*
0	[66.67, 100]	23	13.9%
1	(44.45, 66.67]	37	22.4%
2	(26.63, 44.45]	64	38.8%
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.

Table 7. Judges' decisions in the GDP treatment

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

	With humans as lenders and borrowers				*With robots as lenders and borrowers*	
Independent variables:	*All periods* (1)	*Pivotal periods only* (2)	*All periods* (3)	*Pivotal periods only* (4)	*All periods* (5)	*All periods* (6)
Voluntary return rate in previous period	-0.5323 (0.4583)	-	-0.5157 (0.4322)	-	-	-
Dummy equal 1 for subjects with zero iterations of reasoning	-1.5542 (0.3612)***	-0.9755 (0.3253)***	-	-	†	-
Dummy equal 1 for subjects with zero or one iterations of reasoning	-	-	0.0182 (0.3561)	0.2404 (0.4250)	-	-0.8881 (0.6932)
1/period	-2.6695 (0.9341)***	-0.5741 (0.6138)	-2.5952 (0.9092)***	-0.5725 (0.6141)	-1.3816 (0.4969)***	-1.1345 (0.4748)**
Constant	0.6696 (0.3653)*	0.0589 (0.3598)	0.3971 (0.3687)	-0.0368 (0.4246)	1.1877 (0.6194)*	1.3301 (0.7252)*
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^*. † 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%.

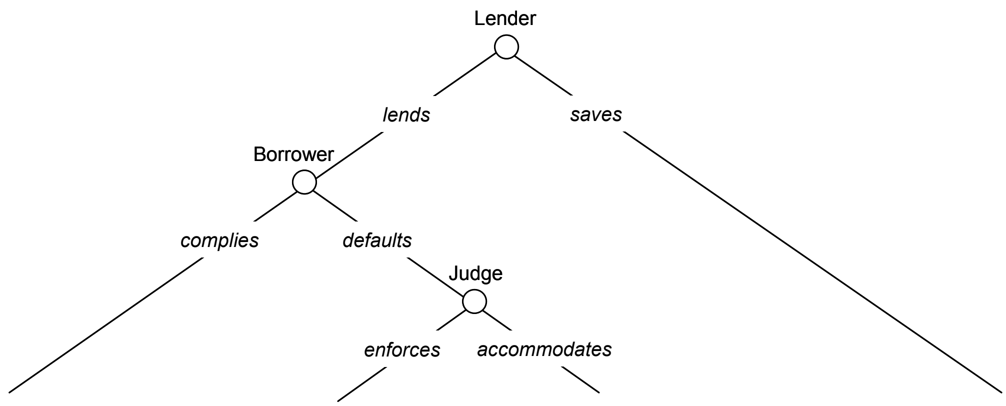
Table 8. Borrowers' voting in borrower constituency

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

	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*
Independent variables:	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Voluntary return rate in previous period	-0.2265 (0.3980)	-	-0.2297 (0.3978)	-	-0.7765 (0.4485)*	-	-0.8834 (0.4956)*	-
Dummy equal 1 for subjects with zero iterations of reasoning	-0.3056 (0.1979)	†	-	-	-0.6768 (0.5717)	-1.2462 (0.6769)*	-	-
Dummy equal 1 for subjects with 0 or 1 iterations of reasoning	-	-	0.1802 (0.3127)	-0.2113 (0.5996)	-	-	-1.0734 (0.3443)***	-1.0814 (0.4573)**
1/period	-0.1037 (0.9532)	-0.3766 (0.4944)	-0.0735 (0.9377)	-0.3317 (0.4462)	0.6865 (0.8853)	0.9692 (1.1841)	0.7510 (0.9513)	1.0791 (1.2288)
Constant	-0.9359 (0.3275)***	-0.3591 (0.2805)	-1.0268 (0.3288)***	-0.6105 (0.3414)*	0.2212 (0.3869)	-0.1199 (0.4761)	0.9760 (0.3837)**	0.4949 (0.5236)
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^*. † 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%.

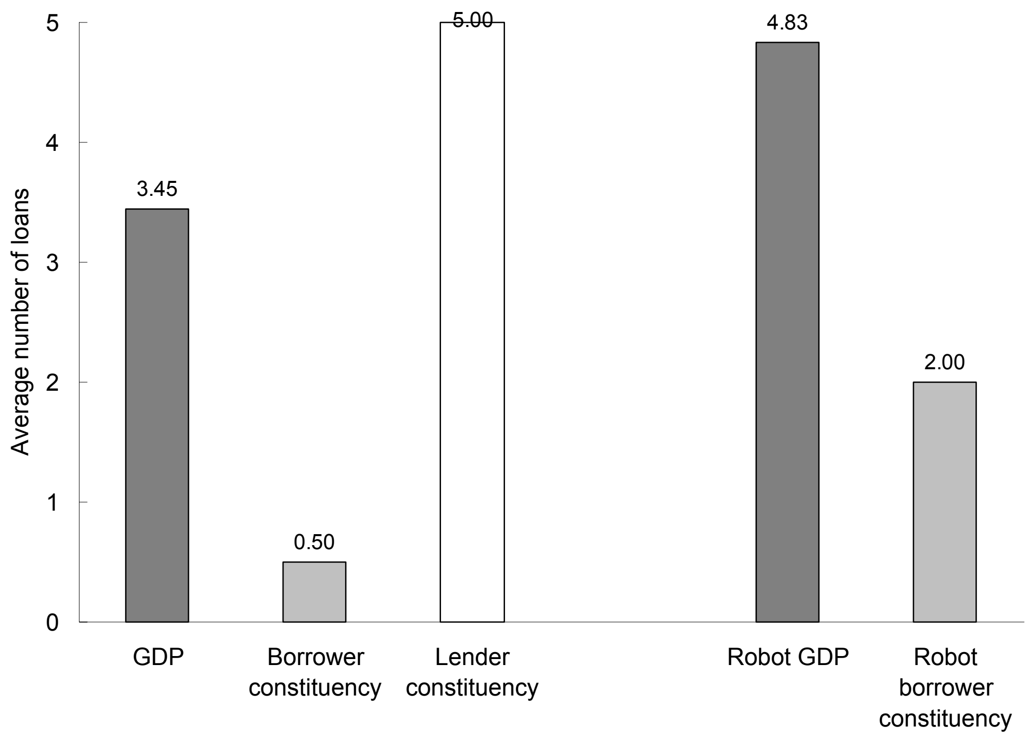
Figure 1. Modified trust game of part 3



Earnings:

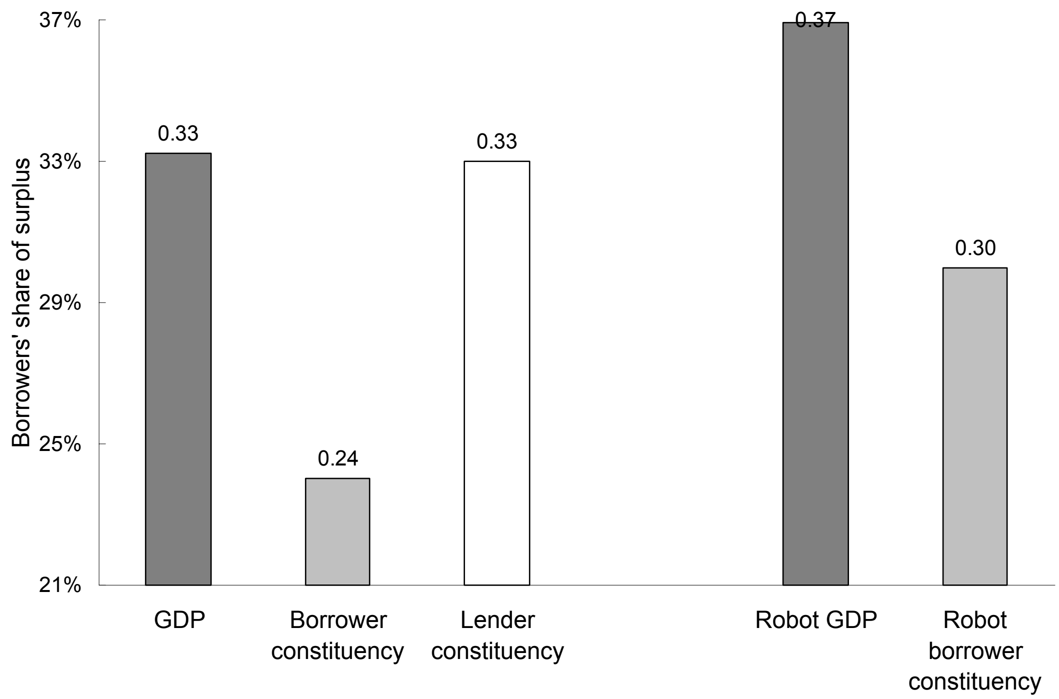
Lender	60-10+17=67	60-10+17=67	60-10+ 0=50	60- 0+ 0=60
Borrower	16+34-17=33	16+34-17=33	16+34- 0=50	16+ 0- 0=16
Judge	π_{jk} (depends on treatment)	π_{jk} (depends on treatment)	π_{jk} (depends on treatment)	π_{jk} (depends on treatment)

Figure 2. Average number of loans by treatment



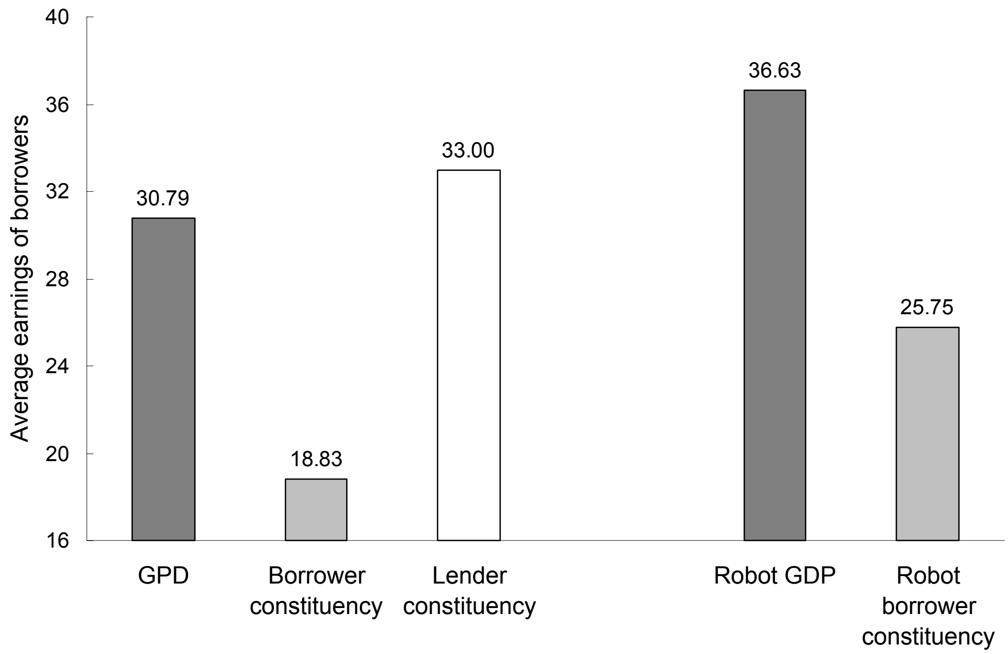
Notes: Periods 11–20 only. Aggregate surplus is zero with zero loans and reaches its full potential with five loans

Figure 3. Borrowers' share of total earnings



Note: Periods 11–20 only

Figure 4. Absolute earnings of borrowers



Note: Periods 11–20 only

Figure 5. Time profile of robot borrower constituency sessions

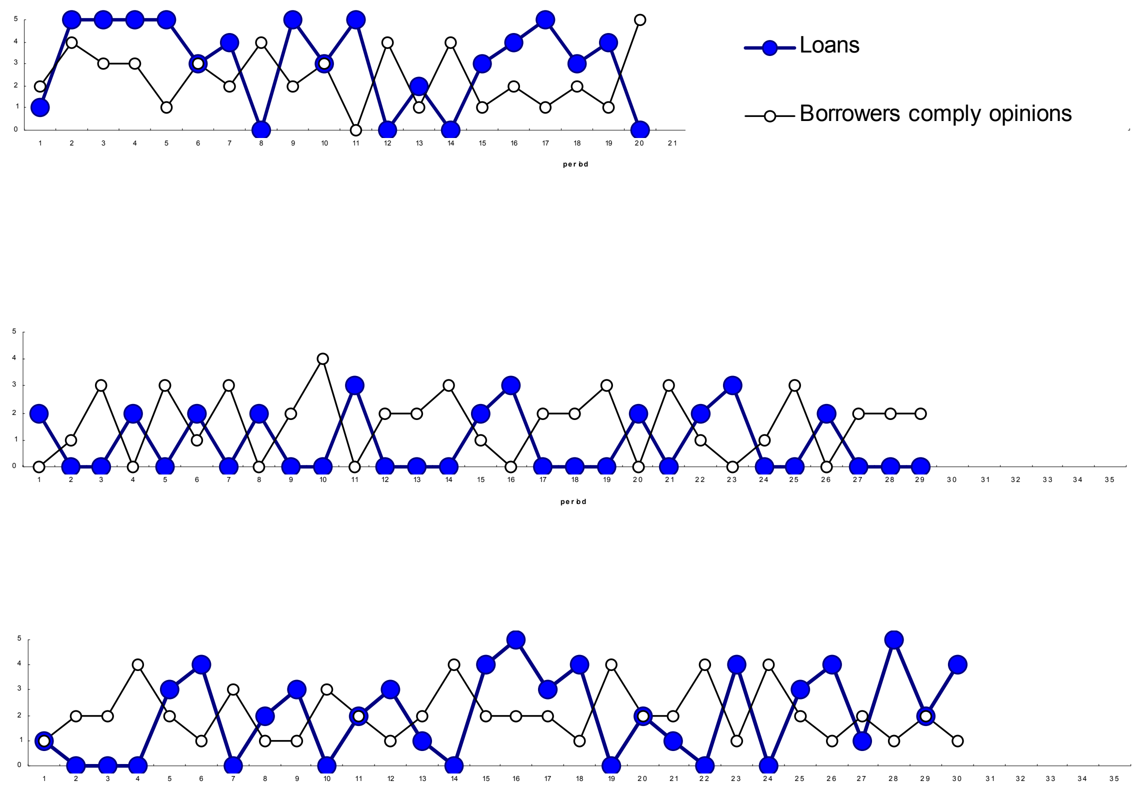


Figure 6. Time profile of borrower constituency sessions with human lenders

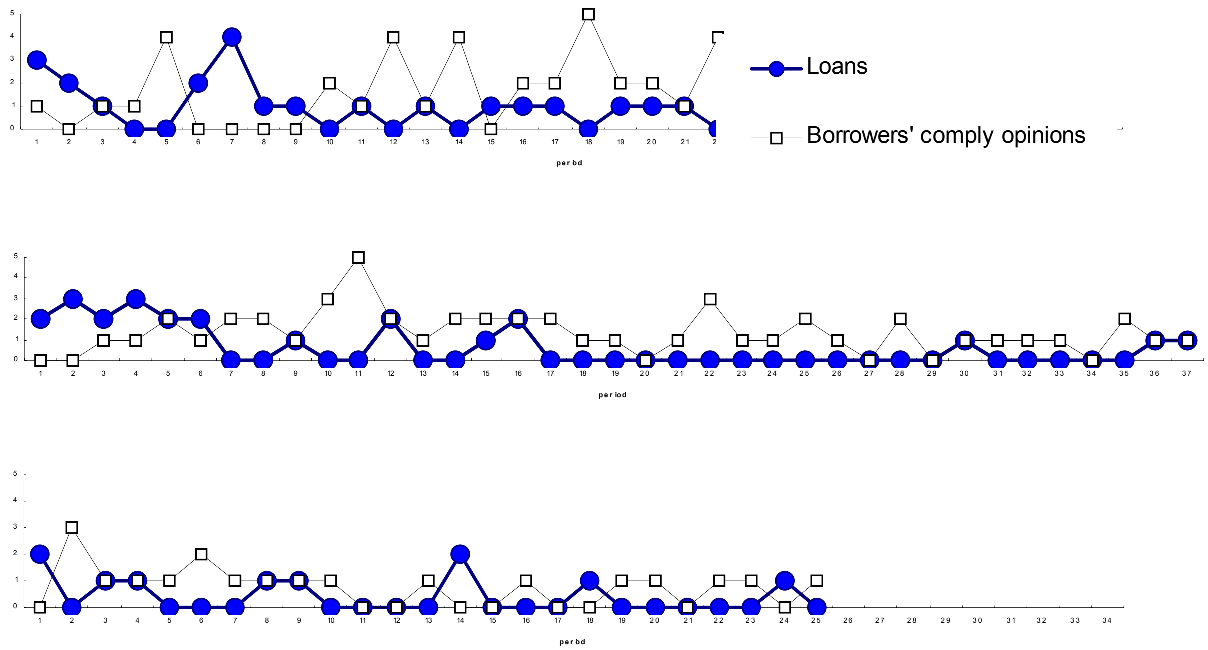


Figure 7. Time profile of robot GDP sessions

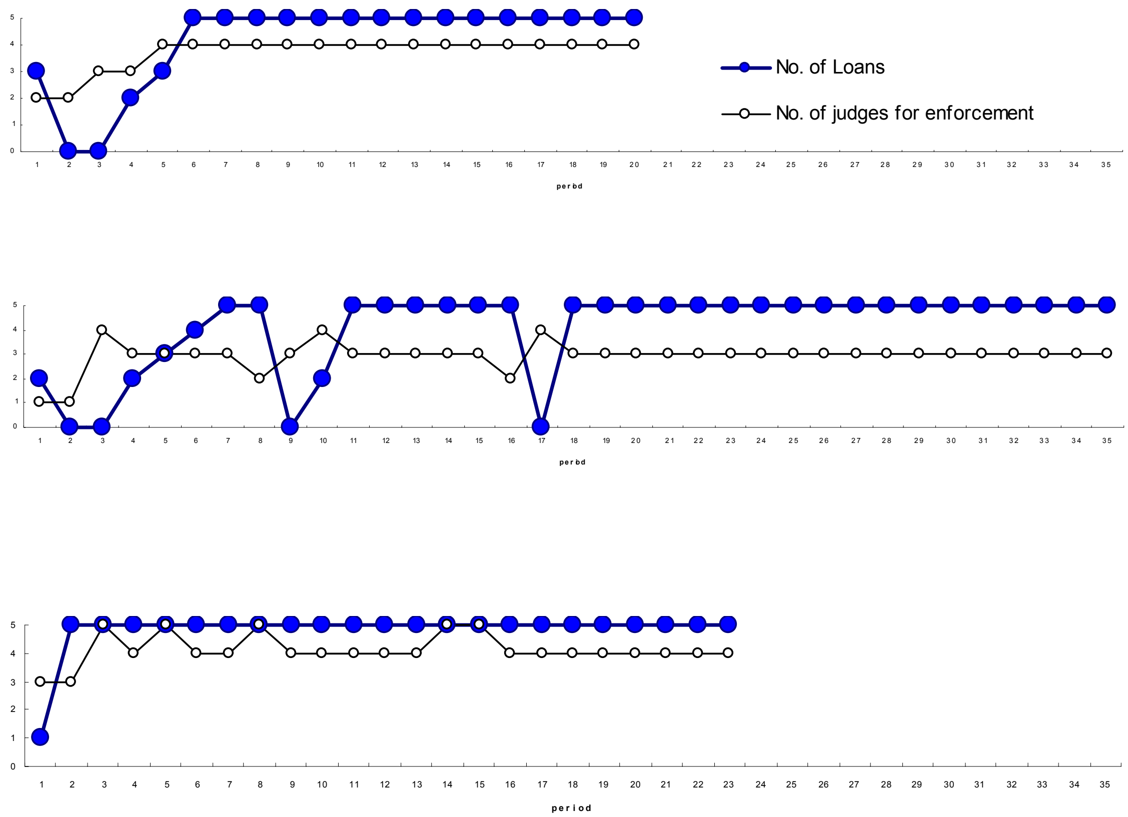
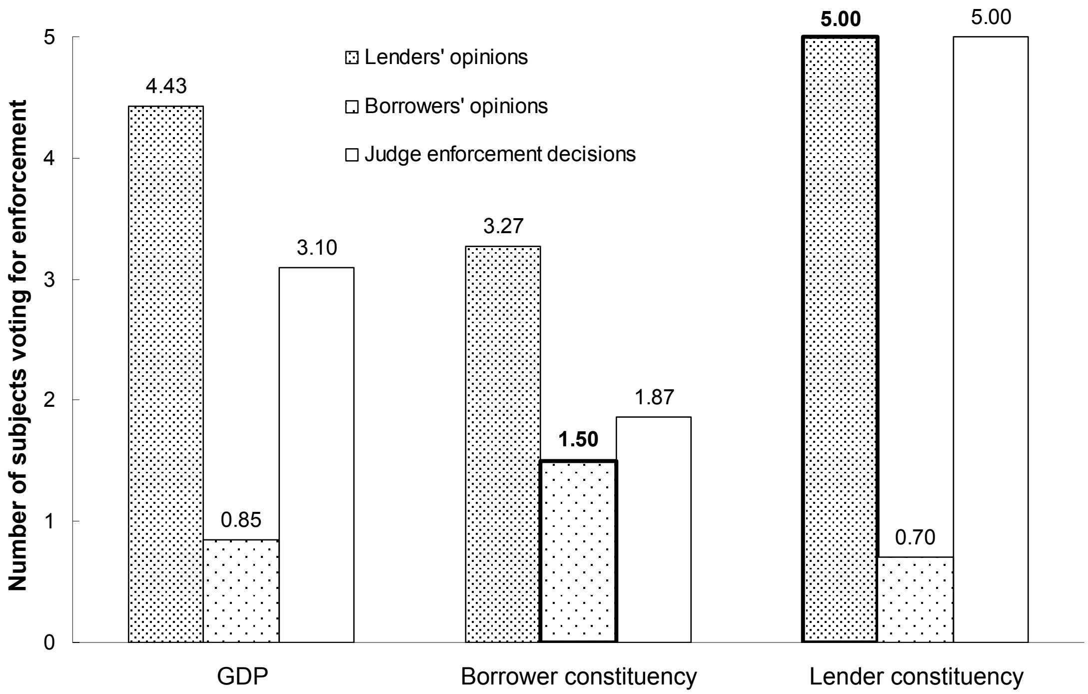


Figure 8. Voting and judges' enforcement decisions



Notes: Periods 11–20 only. In bold, opinions with payoff consequences.