PRELIMINARY
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On the efficient allocation of enforcement rights*

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Abstract

In an experiment we study market outcomes under alternative incentive structures for a third-party enforcer. Our transactions resemble an anonymous credit market where lenders can give loans and borrowers can repay them. In case of default, judges are free to enforce repayment but are themselves paid differently in each of three treatments. First, paying judges according to lender's 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 lenders' voting. These treatments motivate similarly the key decision makers (lenders, borrowers and judges, respectively) towards efficiency but pose them cognitive problems of unequal difficulty. Borrowers face the hardest problem when voting and fail to vote for enforcement even if they would benefit from it. We conclude that incentives are not enough for designing these institutions because, when enforcement rights are allocated to different classes of people, the difficulty of the social problem changes, and those deciding are not always able to solve it.

Keywords: impersonal exchange, third-party enforcement, experiments, limited cognition, judges' incentives, folk theorem.

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1. Introduction

State enforcement—forcing parties to fulfill their obligations—makes impersonal exchange possible and has no good substitutes. It is indeed required for impersonal exchange between anonymous parties, the hallmark of developed market economies (North and Thomas, 1973; Granovetter, 1985; Seabright, 2004). 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, there do not exist good substitutes for State enforcement. Relational contracts are unfeasible for anonymous exchange and, when feasible, they require the abstention or "forbearance" of the State (Williamson, 1991), which very often heavily constrains them instead, 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 law-makers—enjoy substantial discretion. In a world of incomplete contracts enforcers do 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); and even 25 US States enacted farm foreclosure moratoria 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 by constitutional rules. Specific arrangements also operate on both, judges and legislators. So, 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 results in terms of enforcement because of two reasons: 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, landlords or tenants more or less influential in defining the degree of contract enforcement. In the 1930s, US states suffering more farm distress were more likely to enact mortgage moratoria. Similarly, elected US judges tend to rule in favor of local business (Tabarrok and Helland, 1999). Second, some institutional arrangements may provide decision makers with different incentives. We narrowly define the incentives of enforcers as a function linking their enforcement decisions to their personal compensation. So defined, incentives could predictably encourage or discourage enforcement.

When enforcers are rational and self-interested, sound incentives should be sufficient to achieve the desired level of enforcement. Furthermore, by conveniently manipulating incentives, any allocation of enforcement rights could produce the same level of enforcement. There is evidence though that people suffer cognitive failures in different domains (Tversky and Khaneman, 1974; Nagel, 1995; Camerer, 2003). If enforces suffer similar failures, the allocation of enforcement rights may matter and incentives alone fail to achieve the desired level of enforcement.[1]

A main goal of this paper is to show why cognitive limitations do play such a prominent role. For this, we design an experiment with an economy composed of "lenders", "borrowers" and "judges" and examine the consequence of allocating enforcement rights to one of these three possible classes while always keeping incentives for optimal enforcement. More specifically, in our economy trades between lenders and borrowers generate surplus. Decisions are made sequentially and when borrowers do not return loans, judges can enforce repayment.

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 group. In all three treatment, judges therefore have formal enforcement powers, and are free to enforce or not, but a different class of subjects may in fact controls enforcement, which is why we will talk of allocating *enforcement rights* to different classes of subjects: to judges in the GDP treatment,[2] lenders in lender constituency and borrowers in borrower constituency.

In the three treatments, the interest of those holding enforcement rights is to promote enforcement, because their personal incentives are aligned with aggregate efficiency. In addition, they also have all the relevant information, which is public. Consequently, rational subjects should decide and enforce in such a way that the economy reaches the efficient equilibrium in all three treatments.

Instead, we find that the degree of enforcement does depend on how enforcement rights are allocated, to the point 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. We claim that these differences in enforcement are caused by differences in the cognitive difficulty of the problem to be solved in each institutional arrangement. 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

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[1] This concern may seem minor to the extent that enforcers are experts, such as judges and politicians. However, experts in other fields also suffer biases (McNeil *et al.*, 1982). Furthermore, some studies do find that judges suffer from "anchoring," "hindsight," "overconfidence," "framing" and "representativeness" biases (Guthrie, Rachlinksi and Wistrich, 2001). As to politicians, their possible biases add to those of citizens, who ultimately drive the incentives of politicians (Westen *et al.*, 2006). Furthermore, it is politicians who design the incentives of judges. Thus, the cognitive dimension of the contract enforcement problem is ultimately defined by the limitations of non-expert citizens. As a consequence, avoiding cognitive failures should be a priority when designing enforcement institutions.

[2] For simplicity, we will be talking about "judges" but their position is close to that of legislators. When law makers are captured by lenders, they will strive to ensure a proper return to loans and the market survives. On the contrary when legislators are captured by borrowers, the experiment suggest that the risk of the market disappearing is real.

enforce, and judges follow suit, loans dry up and borrowers' earnings are extremely low. In contrast, when lenders control judges, lenders encourage enforcement, the economy achieve 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 final litigation stage as in Bohnet et al. (2001). There is repeated interaction with random ending (Engle-Warnick and Slonim, 2006). To rule out the feasibility of relational contracts (Brown, Falk and Fehr, 2003) we hid subject identifiers. In addition to this trust game, the study includes a guessing game (Nagel, 1995; Costa-Gomes and Crawford, 2006) to estimate subjects' reasoning ability and a static game similar to Engelmann and Strobel (2004) to elicit their 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 results of the experiment and Section 5 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, 5 acted as lenders, 5 as borrowers, and 5 as judges, and all of them kept 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, g*roups 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.

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 learnt 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 the contract; and the average earnings of lenders, borrowers, and judges (Figure 2).

Fourth, the decision of the judge was elicited with the strategy method: she made a decision every period but the decision was implement 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 the contract. As we will explain later, in a given period *all judges earned the same amount*, which varied according to their performance as a group 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 had no direct payoff consequences for lenders and borrowers in the period, although, as it will be clear in a moment, they could have indirect effect. 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: "borrower constituency", "lender constituency" and "GDP" (Table 1). A common feature of all three treatments was that judges were paid according to their performance as a group, 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, the minimum earnings of a judge were 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

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

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 in-between 38 and 50 tokens.

Within treatments 1 and 2, we also implemented a variant where some roles were replaced by pre-programmed computers. We refer to those as "robot GDP" and "robot borrower constituency" and give a full description in the results section. In Part 1 we elicited the preferences of all subjects with respect to efficiency and equality in a static context, along the lines of Engelmann and Strobel (2004). We showed each subject Table 2 and Table 3 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). At the moment of taking these decisions, subjects faced role uncertainty because roles were assigned randomly at the end of the session. Participants were instructed to choose among options A, B, and C 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 run 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 filled a quiz on the rules for Part 3 and the subjects that made 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 only in 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, which were paid in private 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 Part 3. We will do it 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, with N_ℓ=5 lenders, N_b =5 borrowers and N_j = 5 judges, can be represented by:

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

As the expression above suggests, each participant takes at most two decisions, of which the main one we represent through a set of binary *"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, ℓ for lender, *b* for borrower, and *j* for judge 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 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, *"π,"* which have already been explained in Section 2 and Figure 1, result of 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:[4]

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

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

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

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

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

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

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

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

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

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

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

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 as judges' earnings are excluded, will range between 24 ("high surplus"), when all lenders lend, and 0 ("low surplus") when all lenders save.[5]

The best choice for a lender is to lend when her expected payoff is higher than by 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" ER in the economy, defined as the ratio between the sum of loans returned (both voluntarily by borrowers or after judicial enforcement) and the sum of loans given:

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

The expected return on a loan depends on how many are voluntarily returned $(\Sigma_k I_{bk})$ and, of those on default, how high is the enforcement rate by the 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^*:

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

Lending is profitable when more than 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 3 judges decide for compliance the threshold is met for any number of voluntary returns. When at least 3 loans out of 5 are voluntary returned, the threshold is met for any judicial ruling (also for 3/4, 2/3 or 1/1). There are of course mixed cases: 2 judges plus 2 voluntary returns out of 5 (also for 1/3); 1 judge plus 2 voluntary returns out of 4 (also for 1/2).

3.2. Equilibria in the one-shot game

Although in the experiment interaction is repeated, we first put forward the equilibria for the simpler case of a one period interaction.

Under lender constituency, there exists a unique sequential equilibrium, which is symmetric and in pure strategies,

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (5, 5), (0, -), (5)$$

where "-" stands for any choice.

In equilibrium 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

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[5] While 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.

[6] 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.

action. The key players here are the lenders, because they control the third party enforcer. In equilibrium, payoffs are $\pi_\ell = 67$, $\pi_b = 33$, and $\pi_j = 50$, which yield high surplus.

Under borrower constituency, there also exists a unique sequential equilibrium, which is also symmetric and in pure strategies,

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (0, -), (0, 0), (0)$$

Now the key players are the borrowers, because they have control of the third party enforcer. If lenders give loans, borrowers have always an incentive to default and to vote for default. Hence, in equilibrium lenders do not give loans, judges accommodate and payoffs are $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 50$, below the social optimum. Given that borrowers would benefit from receiving loans, one may suggest that they could "promise" to vote for loan enforcement. However, such promise is not credible because borrowers and judges choose after lenders have already given the loan, and hence a "high surplus" outcome is not an equilibrium.

Similarly, under GDP there exists a unique sequential equilibrium, which is symmetric and in mixed strategies,

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (0, -), (0, -), (2.5r)$$

Given the sequential form of the interaction, judges' earnings have already been determined when they decide. Hence, they have no incentive to rule for enforcement nor for default. Being indifferent, we assume that they will choose randomly and hence lending becomes unprofitable because $E[ER]=0.5 < ER^*$. In this treatment, judges are the key players but have no credible incentives to enforce.[7] In equilibrium lenders do not give loans, hence borrowers' strategy does not matter. Payoffs are $\pi_\ell = 60$, $\pi_b = 16$, and $\pi_j = 38$, which are at a "low surplus" level.

Figure 3 represents the one-shot equilibria in the three treatments. The enforcement rate is on the vertical axis and is divided into two parts by the zero-profit line for lenders. Any point below the zero-profit line yields an expected loss for lenders, any point above an expected gain. The number of loans is on the horizontal axis. Besides the equilibrium outcomes, we also drew an isoprofit curve for the borrowers in order to understand income distribution. Outcomes to the right of the isoprofit curve give to the borrowers an expected payoff higher than 33, outcomes to the left give less than 33.

3.3. Equilibria in the indefinitely repeated game

If the game is played repeatedly, new equilibriums 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.

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[7] As explain later, any degree of risk aversion of judges and especially any preference for equality of outcomes makes this result stronger.

Under lender constituency, playing the game repeatedly produces the same equilibrium obtained in the one-shot scenario. There still exists the following unique sequential equilibrium, which leads to high surplus:

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (5, 5), (0, -), (5)$$

Under borrower constituency there exist multiple equilibria, which yields either low-surplus or high-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 as lenders lend less in the following periods.

The high-surplus equilibria exist in the repeated game because the lenders can "threat" to switch from lending to saving in future periods unless borrowers' behavior steadily ensures a positive profit to lenders. Borrowers' tradeoff is such that they benefit from voting for enforcement because, executing their threat, lenders would react to the enforcement level going below the critical threshold by quitting 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 the vote to maintain 100% enforcement and when in the current period they decide to switch to 0% enforcement. The following inequality must hold for all periods (i.e., for any generic period),

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

$$33\ T > 50 + 16\ t + 33\ (T - t)$$

$$33\ t > 50 + 16\ t$$

$$t^* > 50/17 = 2.94 \qquad [7]$$

That is, borrowers profit from voting for enforcement if lenders react to default by quitting lending for at least three periods.

Formally, the high-surplus equilibrium is characterized by an expected enforcement rate, ER^*, barely enough to deliver an expected profit to the lenders:

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (5, -), (0, 5r), (3) \text{ for } r=ER^*$$

The value for r indicates a mixed-strategy that will result in an expected number of about 2.94 borrowers voting for enforcement. Any $r \in (ER^*, 1]$ is not an equilibrium because there is room for a borrower to switch sometimes to vote for default and increase her expected earnings. Instead, if less than 2.94 borrowers vote for enforcement, lenders make a loss and will all switch to saving. In equilibrium, expected payoffs are $E[\pi_\ell]= 60$, $E[\pi_b] = 43$, and $E[\pi_j] = 45.8$.

The low-surplus equilibria are:

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (0, -), (0, 5r), (5r) \text{ for } r \in [0, 33/68)$$

This set of symmetric equilibria includes the one-shot equilibria with zero-enforcement as a special case. Notice that for $r \in (33/68, ER^*)$ there does not exist an equilibrium. In that interval, a one-borrower switch in strategy from voting for compliance with probability r to voting with probability 1 will make lending profitable; hence $4r+1 < 5 \cdot ER^*$ must hold. In equilibrium, payoffs are $\pi_\ell = 60$, $\pi_b = 16$, and $40 < E[\pi_j] \leq 50$. A discussion of the coordination issues generated by the multiplicity of equilibria is delayed to the result section.

Under GDP, there also exist multiple equilibria, which yield either low- or high-surplus outcomes. The high-surplus equilibria are:

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (5, -), (0, -), (5q) \text{ for } q \in [ER^*, 1]$$

where the parameter q represents the expected fraction of judges that rule for enforcement. The equilibrium set includes both pure and mixed strategy equilibria. In equilibrium expected payoffs are $60 \leq E[\pi_\ell] \leq 67$, $33 \leq E[\pi_b] \leq 43$, and $40 < E[\pi_j] \leq 50$.

The low-surplus equilibria are:

$$(\Sigma_k I_{\ell k}, \Sigma_k V_{\ell k}), (\Sigma_k I_{bk}, \Sigma_k V_{bk}), (\Sigma_k I_{jk}) = (0, -), (0, -), (5q) \text{ for } q \in [0, 33/68)$$

This set of symmetric equilibria includes the one-shot equilibria with zero-enforcement as a special case. Notice that for $q \in (33/68, ER^*)$ there does not exist an equilibrium. In that interval, a one-judge switch in strategy from ruling for compliance with probability r to ruling with probability 1 will make lending profitable; hence $4q+1 < 5 \cdot ER^*$ must hold. In equilibrium, payoffs are $\pi_\ell = 60$, $\pi_b = 33$, and $\pi_j = 38$.

Figure 4 represents the above equilibria within the same coordinates of Figure 3, revealing the multiplicity of equilibria that exists under GDP and borrower constituencies when the interaction is indefinitely repeated.

3.4. Extension to risk averse and other-regarding preferences

To discuss how the equilibria of the repeated game changes when we remove the assumption of risk neutral, self-regarding agents, we will maintain the assumption that all agents have identical preferences, and will examine separately, first, the case of risk averse, self-regarding agents and then the case of risk neutral, other-regarding agents.

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,* $ER^{**} > ER^*$. Given the 100% enforcement rate in equilibrium, this makes no difference for equilibrium under lender constituency. It makes instead some difference in the other two treatments, although not a major one.

Under borrower constituency there still are multiple equilibria. More precisely, there is a high-surplus equilibrium, which is unique like before 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.

With reference to other-regarding concerns, we look only at altruism, or concern for the overall earnings in the economy (efficiency), and inequality aversion, which we define as changes in the earnings of the poorest persons in the economy (the borrowers). The closest model to these preferences is Charness and Rabin (2002); for other important models see Fehr and Smidt (1999) and Bolton and Ockenfels (2000). We spell out four implications of concerns for efficiency and equality for the equilibria in the indefinitely repeated game.

First, in the GDP treatment there is a particularly important consequence on the set of high-surplus equilibria. While self-regarding judges equally like all high-surplus outcomes, judges who have a concern for equality prefer the outcome with the lowest possible enforcement rate,

ER^*, which becomes the only high-surplus equilibria. In this case, at no cost for themselves, such judges can lower enforcement rate and improve the earnings of borrowers. Hence*, when there is any concern for equality, the equilibrium sets for borrower constituency and GDP treatments are identical.*

Second, in the situations with multiple equilibria, self-regarding lenders and borrowers strictly prefer what we called the high-surplus equilibria to the low-surplus equilibria; *concerns for efficiency and equality* make that preference even stronger, hence *should make high-surplus equilibria more salient in the coordination decision*. 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 say that her "efficiency multiplier" is 5 or lower. If a subject is willing to pay 3 units to increase the earnings of borrowers by 18 units, we say that her "equality multiplier" is 6 or lower. The stronger is the other-regarding preference and the lower the multiplier.

Borrower constituency provides the best example of why high-surplus equilibria should be more salient in the coordination decision for other-regarding subjects. If her vote is pivotal, a borrower has a personal incentive to vote for enforcement. In addition to any personal gain, if the vote is pivotal the efficiency multiplier is 85 and the equality multiplier is in the interval [42.5, 85]. If the vote is not pivotal its absolute cost is only 2 or less.[8] *Other-regarding subjects should be more likely to coordinate on a high-surplus equilibrium than self-regarding subjects.*

Third, other-regarding considerations can have an impact also in lender constituency. If a lender votes to accommodate borrowers, she can expect a cost of 3.4 (i.e. 1/5 ·17) and that will result in a gain of 17 for borrowers. There is no sacrifice in terms of efficiency and the equality multiplier is 5. When all lenders vote for enforcement, the same gain for borrowers can still be achieved when a judge rules to accommodate. She will pay a cost of 5 for having deviated from lenders' opinions, which yields a lower equality multiplier of 3.4. One can conclude that, *depending on the degree of concern for equality, the equilibrium in lender constituency may entail a high-surplus outcome with an enforcement rate lower than 100%.* Moreover, for moderate other-regarding preferences, lenders' and not judges' choices will drive the change.[9]

Fourth, consider an other-regarding lender in the low-surplus equilibrium that we computed for self-regarding agents. For a lender, giving a loan entails a 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.[10] The efficiency multiplier is in the interval [2, 3.9] and the equality multiplier is in [1.5, 1.9]. *One can conclude that in the borrower constituency and GDP treatments with other-regarding lenders, the set of low-surplus equilibria may shrink and may even disappear altogether.*

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[8] The expected cost for a borrower voting for enforcement is (1/5 ·17) and the gain for the economy is 34·5. The gains of borrowers are $5\cdot[ER(33-16)+(1-ER)(50-16)]$. A similar calculation can be made for judges. If the a judge ruling is pivotal, the efficiency multiplier is $(-5\cdot 4 + 34\cdot 5]/5=30$ and the equality multiplier is $\{5\cdot[ER(33-16)+(1-ER)(50-16)]\}/5$, i.e. in the interval [17, 30].

[9] Remember our assumption that all agents have identical preferences. Moreover, the decision of the judge will lower the earnings of other judges by 20, hence of the economy as a whole by 25.

[10] Given $0\leq ER<33/68$, the cost for a lender is $17(1-ER)$ and the gain for a borrower is $ER(33-16)+(1-ER)(50-16)$.

4. Results

We will present now three sets of results from the experimental data. First, Result 1 is a preliminary statement about subjects' preferences. Second, the main findings are Results 2 and 3, 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. Finally, Results from 4 through 7 explore possible reasons for these main findings and settle on a bounded rationality explanation.

Result 1. Only 21% of subjects are strictly self-regarding. About half of the subjects have some concern for equality of earnings and about half have some concern for overall efficiency.

Table 2 and Table 3 provide support for Result 1, which refers to all participants to Part 3. The percentage of self-regarding subjects with respect to both efficiency and equality was obtained by crossing Table 2 and Table 3. As already noted in the theory section, the presence of a large fraction of subjects concerned for equality has the important consequence that the equilibrium sets for the GDP and borrower constituency treatments are identical. In particular, in any of the GDP sessions there were either two or three judges with a concern for equality.[11]

Result 2. Market surplus is remarkably different across treatments. Whereas in lender constituency subjects reach 100 percent of the potential surplus and in GDP they reach 69 percent, in borrower constituency they reach only 10 percent.

Table 4 and Figure 5 provide support for Result 2. 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 market surplus is at its predicted equilibrium level of 100%. In borrower constituency market surplus is close to the low-surplus equilibrium level of 0%, which suggests that subjects coordinated on their worst 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]

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[11] In one session only 2 judges were concerned for equality, which implies that there still are multiple high-surplus equilibria, although the set is much smaller than before, [0.588, 0.6], instead of [0.588, 1].

[12] The p-value for different grouping of samples: 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.*

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, which generally 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 3. 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 6 and Figure 7 support Result 3. Somehow paradoxically, borrowers end up worse off under borrower constituency, when they do hold voting rights that command enforcement, than in the other treatments. When lenders "control" judges, borrowers' earnings are sensibly 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 earning (now 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]

We put forward three conjectures to explain Results 2 and 3, which will then examine one by one: (a) other-regarding preferences; (b) coordination problems; and (c) difficulties in understanding market interactions. Summing them up, the results show that other regarding preferences and coordination problems do not hold water. It is conjecture (c), the difficulties in understanding how the market works, which better explains the data.

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 points toward a role for other-regarding concerns. For instance, those lenders who lend when enforcement rates are well below ER^* could simply be concerned with efficiency or equality (Table 5). Another instance is judges' behavior in the GDP treatment. The enforcement rate well below 100% could reflect their concern for equality.

On the other hand, other choices are hardly explained by other-regarding concerns. For instance, in lender constituency there is no evidence of other-regarding behavior on the side of

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[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.

neither lenders nor judges. Most importantly, subjects in borrower constituency did not have the high-surplus equilibrium as their focal outcome.

In order to rule out the impact of other-regarding preferences we use two variants of the treatments where we replace some humans with robots:

- *Robot borrower constituency*: in a variant 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 5, Figure 6, Figure 7 and in Table 4. In the robot variants the five subjects in a session are the only ones paid and hence concerns for efficiency are perfectly aligned with their own self interest while concerns for equality should have no impact. Yet, the *differences* between borrower constituency and GDP change only marginally from within human to within robot variant economies. The *difference* 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.[14]

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 and 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. Second, within the borrower constituency treatment judges could suffer a coordination failure in enforcement choices while matching the voting behavior of borrowers.

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

The robot variants provide evidence on the first point, and suggest that subjects playing the same role coordinated successfully. In the robot GDP sessions, judges learn to coordinate on a high-surplus equilibrium (Figure 8). 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 9). In particular,

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[14] We compare the differences in order to maintain constant the behavior of lenders, which were humans in some cases and robots in the other cases. As we will explain below, the built-in model of robot lender is responsible for the higher market surplus achieved. As a side note, consider some peculiar other-regarding preferences. If subjects care only about the 50/50 outcome, borrower constituency still fare badly in comparison to lender constituency (an average of 0.33/5 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 tokens. Evidence from other experiments suggest that there may be some spite but rarely at the degree required to take such action.

borrowers behave anti-cyclically toward robot lenders. When robot lenders have given many loans, borrowers vote less for compliance. Eventually the loans dry up, and then borrowers increasingly vote for compliance again, hence robot lenders start giving loans once more.

Result 6 addresses the second coordination issue, highlighting that in the borrower constituency treatment judges choices closely follow the average voting behavior of borrowers.

Result 6. Our judges respond 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 10 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, according to the data presented in Table 5) but learn to enforce transactions (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 rule on average closely following the opinions of their constituency, as shown in Figure 10.[15] The transmission is perfect in lender constituency (i.e., always enforcement) while it is close in borrower constituency, with averages of 1.5 borrowers voting for enforcement and 1.87 judges ruling for enforcement. The 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 differential performance in terms of market surplus across treatments depends on the cognitive difficulty of the task faced by the key subjects of each treatment. In particular, lender constituency is the easiest and borrower constituency is the most difficult.

We argue that the driving force behind the different performance across treatments commented in Result 2 are the differential levels of cognitive ability necessary to understand the equilibrium strategy in each of the three treatments.

We operationalize cognitive difficulty 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. (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

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

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’ reaction. (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. First when voting, a borrower 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.

The above ranking would be irrelevant for fully rational agents. Our subjects instead are characterized by a limited number of iterations of reasoning, which we measured through the guessing game of Part 2 of the experiment. As reported in Table 6, about 36% did either zero or one step of reasoning, instead of the infinitely many predicted for fully rational agents. When subjects are boundedly rational, the above treatment ranking may help to predict the direction of the bias in subject choices. In particular, when short-run and long-run incentives of the key actors are misaligned we expect their choices to be closer to their short-run incentives, which is exactly what one observes in the data. Table 7 shows the initial choices of the key actors in all treatments. In lender constituency, lenders always guide the economy on the high-surplus outcome. In GDP, judges in the human variant enforce below the zero-profit threshold for lenders while in the robot variant are barely at that level. In borrower constituency, borrowers vote on average well below the zero-profit threshold both in the human and in the robot variants. The behavior in the remaining periods of the experiment does not qualitative change these findings (Table 5). In conclusion, the data points out that in borrower constituency, borrowers lack understanding of the market consequences of their voting behavior.

The above findings are reinforced by the regression results of Table 8 and Table 9, which show that, at the individual level, the choices that key players take in Part 3 depend on how many iterations of reasoning they do 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 irrespectively whether we consider all periods of a session or just those where each judge’s decision was pivotal for reaching or not the zero-profit enforcement threshold, ER^* (Table 8, columns 1, 2, 5). There is also significant learning as judges better understand 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 9, columns 7, 8). There are two interesting differences in comparison with the GDP treatment, however. First, the cut-off is zero iterations for the GDP treatment and one iteration for the borrower constituency treatment, which directly ties into the treatment ranking we gave above in terms of cognitive difficulty. Second, there is 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, they did not make it to move their choices toward 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 of subjects in understanding the systematic consequences of their immediate choices within a market mechanism.

One final note on robot borrower constituency. Market surplus changes from 10% in the human variant to 40% in the robot variant (Table 4). The main reason for this improvement lies in our choice of backward looking robot lenders, which allowed borrowers to sustain throughout a session a pattern of cycles of high enforcement-many loans-no enforcement-few loans (Figure 9). Because of their design, robot lenders could be fooled all the time while human lenders seem to have understood the strategy after a couple of cycles, and stopped giving loans for good (Figure 11).[16] As a consequence, while with robot lenders efficiency, measured as market surplus, was stable over time, it was steadily declining with human lenders. In addition, we still observe the paradox of borrowers' absolute earnings being 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 didn't. That also supports that subjects are boundedly rational with evidence that cannot be contaminated by other-regarding factors.

5. Conclusions

We design an experiment to examine how different political and judicial institutions produce enforcement and thus make market transactions possible. When contracts are enforced, the market flourishes, when they are not, the market disappears. We show that this variability is caused by the cognitive difficulty of the social problem defined by each set of institutions. Our institutions consist of allocating enforcement rights to different classes of people, classes which are defined by their role as parties to a credit contract, 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 cognitive problem with respect to enforcement are successful in supporting the market. On the contrary, markets disappear when the institutions allocate enforcement rights to parties suffering serious cognitive problems 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 contracting 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

[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.

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 profits 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 lender defaults, the judge (third party) can either force the borrower to repay the loan or accommodate the default. If the judges 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 contract 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 enforcement and efficiency. Therefore, rational lenders and borrowers should both vote for enforcement and rational judges should enforce. The experimental results are mixed, however. Lenders do vote for enforcement and the economy reaches full efficiency in the lender constituency treatment. Judges in the GDP treatment do enforce and end up approaching full efficiency. In contrast, borrowers overwhelmingly vote not to enforce and, as a consequence, judges do not enforce and the efficiency in borrower constituency is extremely low. Borrowers end up stack in an equilibrium where, paradoxically, their earnings are lower and income inequality is higher than in lender constituency.

We claim that these different results are driven by the varying difficulty of the cognitive problems that result from the different allocations of enforcement rights across treatments. We observe a clear a negative correlation between cognitive difficulty and enforcement. 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 cognitive problem created by each institutional arrangements is based on three considerations. For a start, concerns for efficiency or equality should move borrowers to vote for enforcement instead of accommodation. Moreover, when facing robot players instead of human agents, results are comparable. These robot treatments also 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 cognitive abilities at the individual level. We observe that those subjects that do less iterations of reasoning in a separate task are the least likely to favor enforcement.

The results are most striking because all our decision makers, including borrowers, have incentives to enforce. 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 too difficult problems, 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.

The roles played by our experimental subjects, and their problems in the different treatments resemble those faced by real economic agents. One might therefore be tempted to draw parallelisms with real institutions. At its most general, our treatments might be suggestive of different forms of democracy, in which the third party enforcer (either the government, the judiciary or both) are directly controlled by different groups in society. In the GDP treatment, this control is exerted by incentives to maximize society's welfare, as it is ideally the case with democratic governments and independent judiciaries. Unfortunately, these comparative exercises hold little water, given the experimental nature of the evidence and the fact that it excludes essential aspects of reality. For instance, in the experiment, we have, by assumption, implicitly precluded any risk that property rights might be expropriated. However, real societies might face a severe risk of expropriation when they allocate enforcement rights asymmetrically between classes of subjects.

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Table 1. Experimental treatments

Treatments	*Use of robots*	*Session number*	*Date*	*Number of participants in Parts 1 and 2*	*Participants in Part 3*	*Length of Part 3 (Periods)*
Lender constituency	None	16	12 Apr 06	18	15	37
		17	18 Apr 06	18	15	29
Borrower constituency	None	11	2 Mar 06	15	15	22
		13*	7 Mar 06	18	15	37
		14	8 Mar 06	18	15	25
	Robot lenders and robot judges	12	3 Mar 06	6	5	30
		8*	23 Feb 06	6	5	20
		20	26 Apr 06	6	5	29
GDP	None	9	28 Feb 06	15	15	27
		10	1 Mar 06	15	15	24
		15	9 Mar 06	18	15	22
		18	19 Apr 06	18	15	23
	Robot lenders and robot borrowers	5	19 Feb 06	6	5	35
		6	21 Feb 06	6	5	20
		19	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. * No show-up fees were paid with the exceptions of a $4 payment made in session 13 (because it lasted over 2 hours) and in session 8. Sessions 1-4 were pilot sessions. Session 7 not reported because of an early software crash.

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 to 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 to 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

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 earnings splits instances 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. All subjects that participated to Part 3 are included. The classification adopted follows Nagel (1995).

Table 7. Choice of the key actors in the first 5 periods of a session

Treatment	*Number of observations (Number of session x 5 periods)*	*Lenders voting for enforcement (avg. no. per period)*	*Borrowers voting for enforcement (avg. no. per period)*	*Judges ruling for enforcement (avg. no. per period)*	*Percentage of periods with three or more subjects for enforcements*
Lender constituency	10	4.2		-	100.0%
Borrower constituency	15	-	1.1	-	13.3%
GDP	20	-	-	1.8	30.0%
Robot borrower constituency	15	-	2.1	-	40.0%
Robot GDP	15	-	-	3.1	73.3%

Notes: For lenders to make a positive profit, on average at least 2.94 out of 5 loans should be returned.

Table 8. 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 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%.

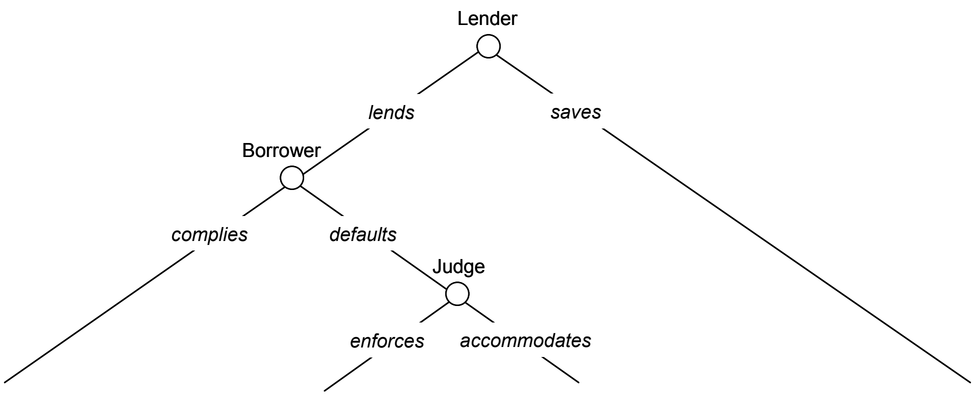
Table 9. Borrowers' voting in borrower constituency

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

	With humans as lenders and judges				*With robots as lenders and judges*			
Independent variables:	*All periods* (1)	*Pivotal periods only* (2)	*All periods* (3)	*Pivotal periods only* (4)	*All periods* (5)	*Pivotal periods only* (6)	*All periods* (7)	*Pivotal periods only* (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 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. Results screen for Part 3 (GDP treatment)

Figure 3. One-shot equilibria

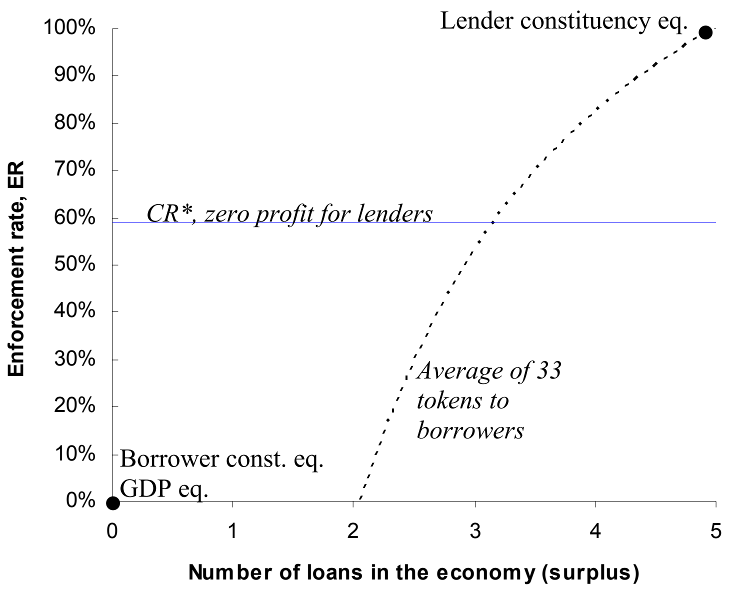


Figure 4. Repeated game equilibria

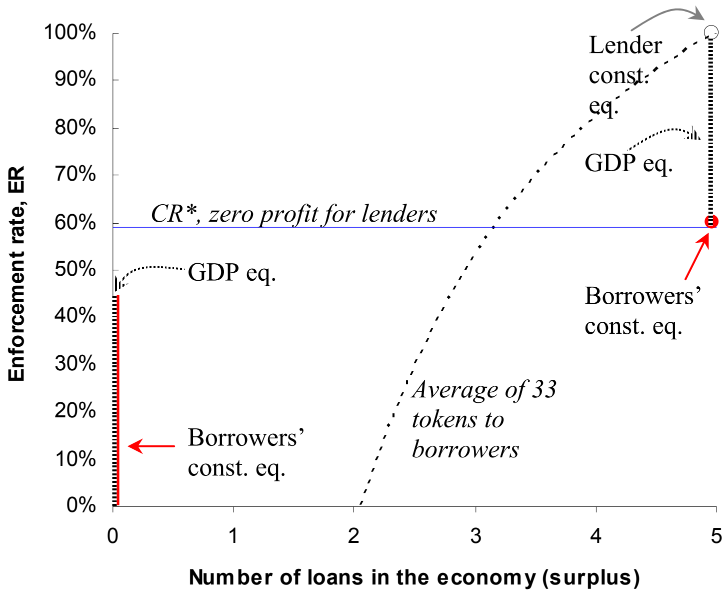
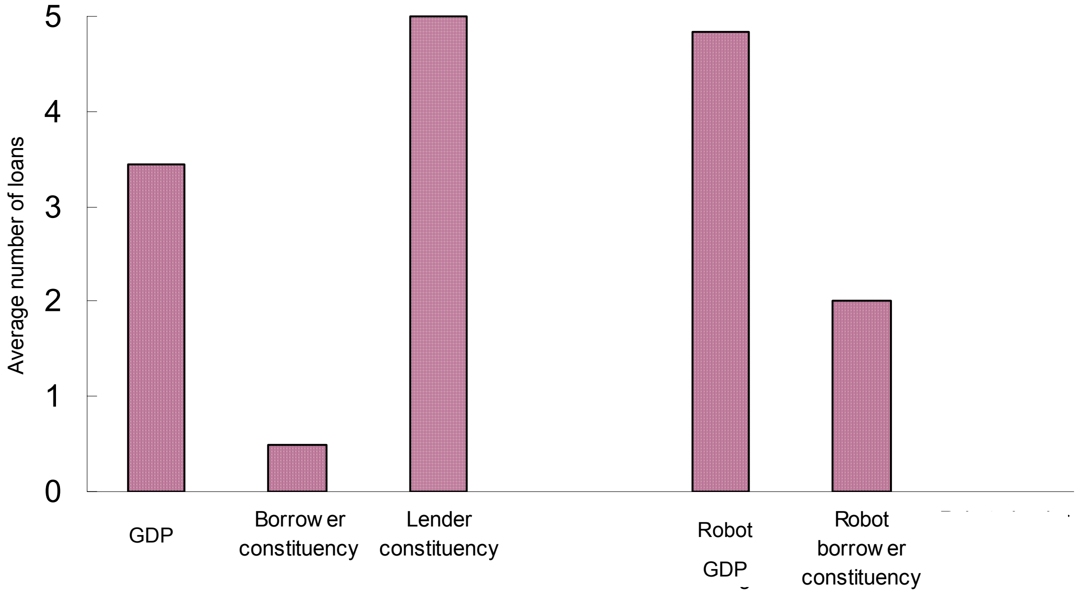
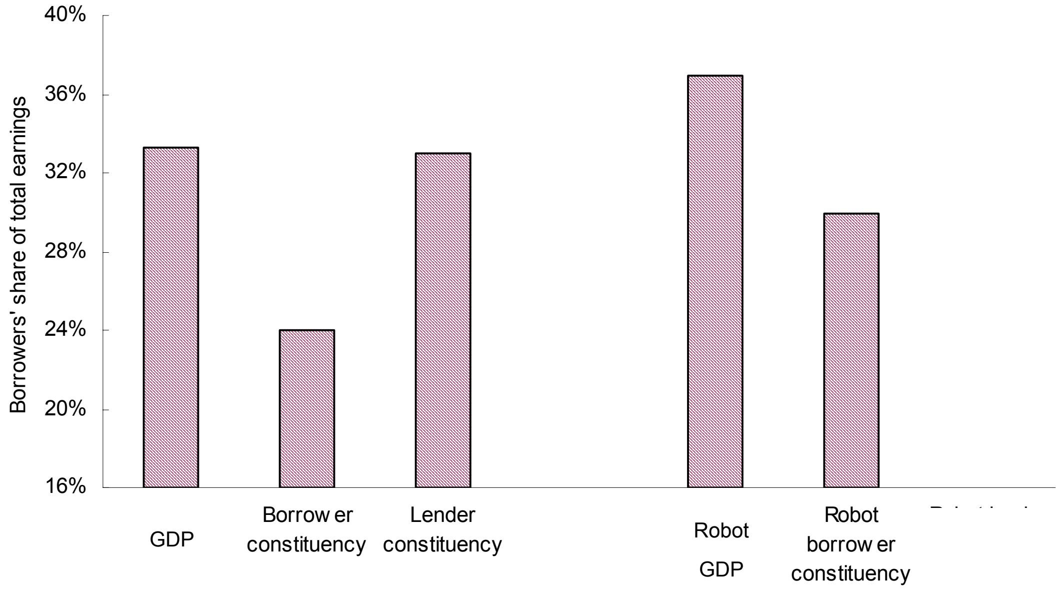


Figure 5. Average number of loans by treatment



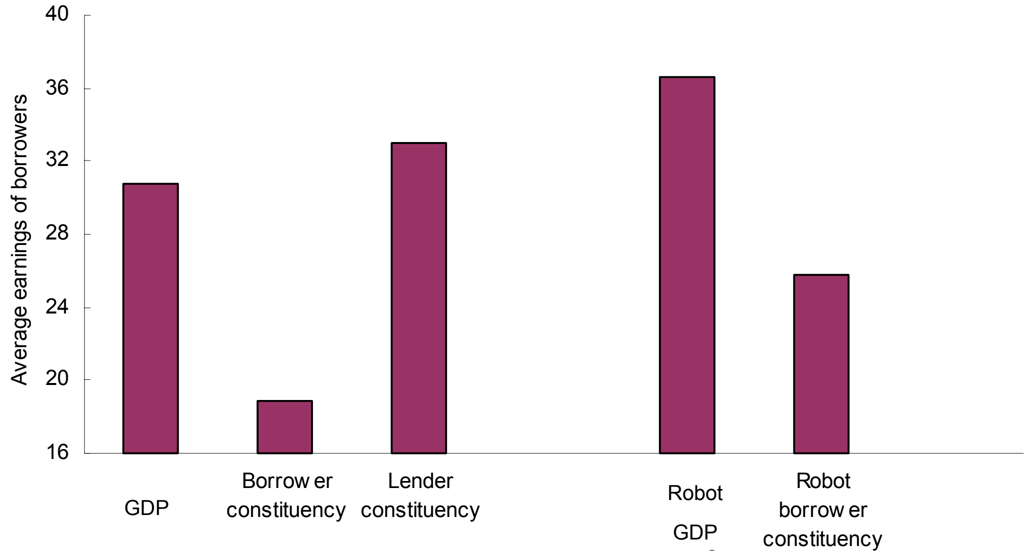
Notes: Periods 11-20 only. Aggregate surplus is zero with zero loans and reaches its full potential with five loans

Figure 6. Borrowers’ share of total earnings



Note: Periods 11-20 only

Figure 7. Absolute earnings of borrowers



Note: Periods 11-20 only

Figure 8. Time profile of robot GDP sessions

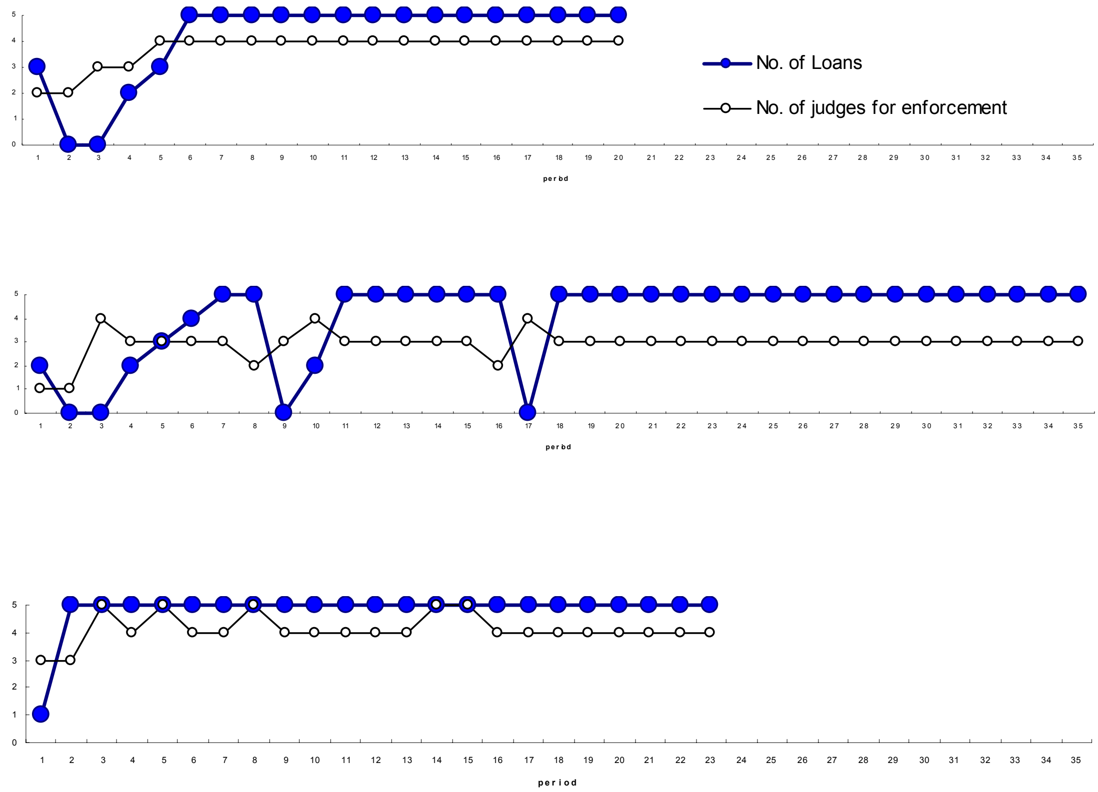


Figure 9. Time profile of robot borrower constituency sessions

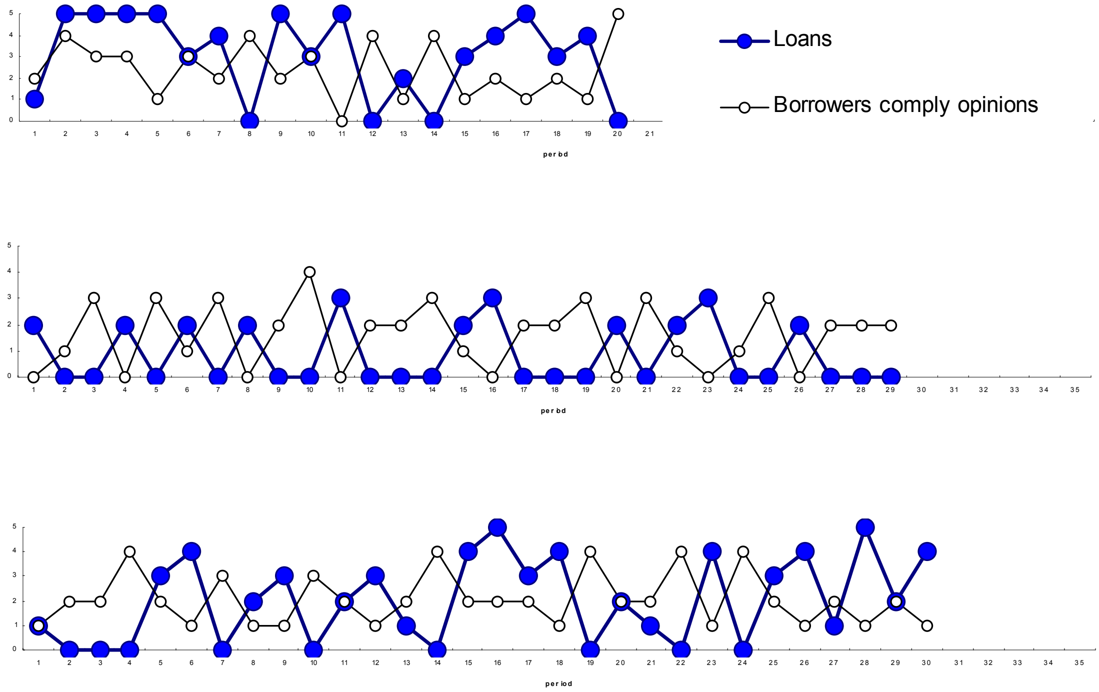
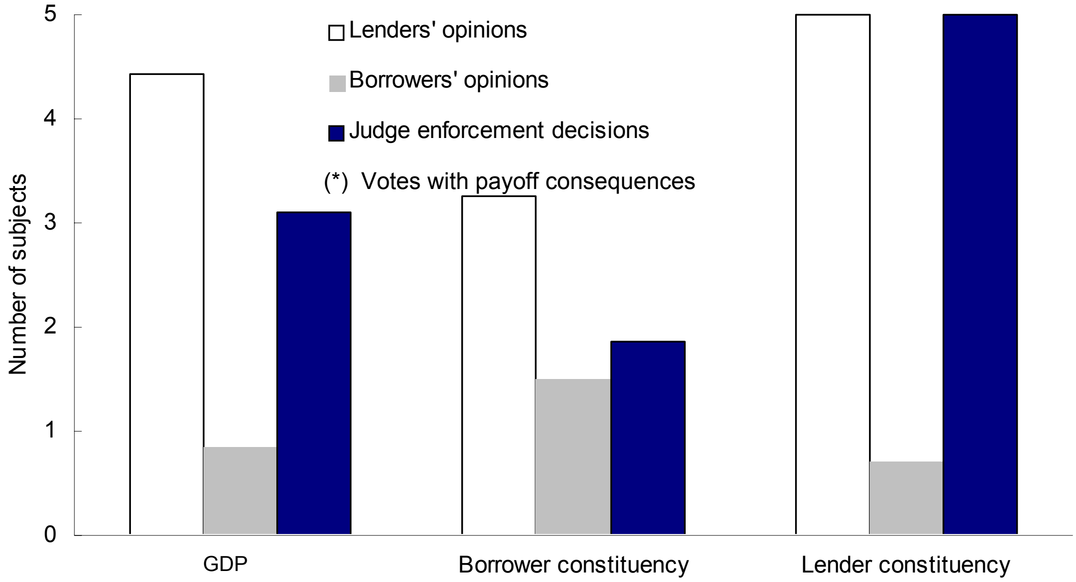


Figure 10. Voting and judges enforcement decisions



Notes: Periods 11-20 only

Figure 11. Time profile of borrower constituency sessions with human lenders