The Threat of Exclusion and Implicit Contracting

Martin Brown, a Marta Serra-Garcia b

a Swiss Institute of Banking and Finance, University of St. Gallen, CH-9000 St. Gallen, Switzerland; b Rady School of Management, University of California, San Diego, La Jolla, California 92093

Contact: martin.brown@unisg.ch (MB); mserragarcia@ucsd.edu (MS-G)

1. Introduction

Economic theory suggests that self-enforcing agreements, i.e., implicit contracts, can mitigate moral hazard in labor, credit, and product markets (e.g., Bull 1987, Boot and Thakor 1994, Klein and Leffler 1981). Existing experimental evidence documents that self-enforcing agreements emerge and are efficiency enhancing in bilateral contracting settings as well as in market contexts with endogenous partner choice (Brown et al. 2004, Fehr et al. 2009). For managers, implicit contracting implies that conflicts of interest with employees, creditors, or suppliers may be resolved through repeated interaction rather than through monitoring, incentive contracts, or a reallocation of ownership.

The enforcement mechanism underlying an implicit contract is the threat of exclusion: the agent fears that he will lose future income if the principal breaks off the relationship. In this paper, we emphasize that in some markets and business relations, the threat of exclusion is inherently weak. In these environments, implicit contracting may not be an effective instrument to mitigate moral hazard, and managers may therefore need to resort to more formal and costly enforcement mechanisms.

The threat of exclusion is especially weak in environments where the agent can appropriate income-generating resources from the principal and can independently put these resources to productive use. For example, in credit environments with weak creditor protection, borrowers may appropriate and reinvest borrowed funds (Bond and Krishnamurthy 2004, Bulow and Rogoff 1989). In the context of foreign direct investment, local investment partners (e.g., the host government) may expropriate the physical and financial capital of foreign investors (Thomas and Worrall 1994). In professional services such as consulting, legal advice, or wealth management, advisors may persuade the firm’s clients to follow them when they leave the firm or set up their own business.1 In such environments, the agent (borrower, investee, employee) may no longer need to contract with the principal (lender, investor, employer) to maintain income generation.

We implement a laboratory experiment to investigate how implicit contracting is affected when the threat of exclusion is weak. Our main treatments study implicit contracting in the context of repeated, bilateral lending. In each period, the lender decides how much to lend to the borrower and what repayment to request. The borrower earns a nonstochastic investment return that depends on the loan size and can decide whether to strategically default or not. In our main treatment, the weak exclusion (WE) treatment, a borrower who defaults can continue to use the borrowed funds to invest in future periods. In our control treatment, the strong exclusion (SE) treatment, a defaulting borrower must liquidate his investment and consume the proceeds in the same period. If he wants to invest in subsequent periods, the borrower must go back to the lender for a new loan. This treatment closely resembles the design of existing experimental studies that study repeated investment games (for an overview, see Camerer 2003). By comparing the outcomes of the
WE and SE treatments, we examine how the ability of lenders to exclude defaulting borrowers from future income generation affects implicit contracting.

We derive predictions under the assumption that there is a share of nonidentifiable social borrowers who repay loans even in a one-shot situation (Kreps et al. 1982). In the SE treatment, an implicit contracting equilibrium with maximum loan sizes and full repayment until the penultimate period is feasible. Borrowers have a strong incentive to repay, since they will otherwise be cut off from future loans and thus investment. By contrast, in the WE treatment, the threat of discontinuing a credit relationship is a weaker disciplining device, since, upon default, borrowers can continue to invest the funds already borrowed. Two different types of equilibria can emerge: first, implicit contracting equilibria in which lenders “start small” (offer low initial loans and repayment-contingent loan increases to establish dynamic incentives for borrowers to repay), and second, screening equilibria in which lenders offer high initial loans to borrowers to screen out selfish agents (who will default). Hence, from a theoretical perspective, we show that weak exclusion can lead to either smaller loan sizes or an earlier breakdown of credit relationships.

Our results show both predicted effects of weak exclusion. First, we find that credit relationships are more likely to break down in the WE treatment. By the end of the first period, only 44% of all relationships are characterized by a loan offered, accepted, and repaid in the WE treatment, compared to 73% in the SE treatment. In accordance with screening equilibria, we find that in the initial period of the WE treatment (but not the SE treatment), default is more likely for large loan sizes. Second, we find that credit relationships in the WE treatment feature smaller loans than those in the SE treatment. In accordance with the implicit contracting equilibria, a larger fraction of lenders in the WE treatment offer small loan sizes in the initial period.

In robustness tests, we document that our main treatment effects are moderated in competitive market conditions. We replicate our experiment with lender competition; i.e., each borrower is matched with two lenders who can make competing loan offers in each period. Under lender competition, we find that self-enforcing credit relationships are also less frequent and loan sizes are smaller in the weak exclusion condition than in the strong exclusion condition. However, lender competition mitigates the negative treatment effect of weak exclusion on the early breakdown of credit relationships. This can be explained by the fact that screening equilibria are no longer feasible under lender competition. Moreover, competition lowers interest rates and thus reduces the incentives of borrowers to default in the weak exclusion condition.

The main contribution of this paper is to provide evidence that the threat of exclusion is crucial to the efficacy of self-enforcing agreements. The existing experimental literature has explored two conditions under which implicit contracting may not be a perfect substitute for third-party enforcement: lender competition and stochastic investment returns. Competition between lenders could potentially weaken borrowers’ incentives to repay a given lender as they may turn to other lenders after default. Similarly, stochastic investment returns may limit the scope for relational contracts as lenders cannot perfectly identify and punish strategic defaults. Brown and Zehnder (2007) show that, even in the presence of competition, relational contracts emerge and lead to large volumes of credit. Fehr and Zehnder (2009) find that even with stochastic investment returns, relational contracts emerge and sustain high credit volumes. In contrast to these studies, we document that when agents can appropriate income-generating resources from principals, implicit contracting may be seriously impaired.

The second contribution of our study is that we examine how the threat of exclusion impacts on the time structure of implicit contracts. Previous theoretical work has examined the time structure of self-enforcing agreements and suggested that asymmetric information about players’ types may explain “starting small” in investment contexts (Ghosh and Ray 1996, 2016; Rauch and Watson 2003; Sobel 1985) and in prisoners’ dilemmas (see Watson 1999, 2002; Andreoni and Samuelson 2006). Starting small—or “progressive lending,” as it is often termed in credit market contexts—is commonly observed in environments with weak creditor protection, for example, in microfinance (Morduch et al. 1999, Armendariz and Morduch 2006). However, there is no evidence to date documenting that weak creditor protection is a determinant of “progressive lending.” Our results suggest that weak creditor protection can lead to smaller loan offers. However, we find “progressive lending,” i.e., increasing loan sizes over time, independent of the threat of exclusion.

The rest of this paper is organized as follows. In Section 2, we describe the experimental design. In Section 3, we outline the predictions. We report our experimental results in Sections 4 and 5. Section 6 concludes.

2. Experimental Design

We study the consequences of the threat of exclusion for self-enforcing agreements by focusing on repeated, bilateral lending (and use a credit market framing henceforth). We implement two treatments, based on a repeated investment game (Berg et al. 1995). In robustness tests, we adopt our main treatments to allow for lender competition. We report on these competition treatments in Section 5.

2.1. Strong Exclusion Treatment

In the strong exclusion treatment, one lender and one borrower are paired for seven periods. We choose a
finite horizon game because it allows us to identify the emergence of self-enforcing implicit agreements.\(^5\) We choose seven periods rather than two or three to be able to clearly separate “starting small” in loan sizes from the potential end-game effect, i.e., a reduction of loan sizes in the last periods of the game.

In each period \(t = \{1, \ldots, 7\}\), the borrower has an investment opportunity: he can invest the amount \(I_t \in \{0, 1, 2, 3, \ldots, 10\}\), which yields a certain gross return of \(vI_t\), with \(v = 3\) in our experiment. We restrict the maximum investment size to 10 in each period. The investment amount of the borrower in each period \(I_t = C_t + S_t\) is equal to his capital \(C_t\) and the loan size \(S_t\) he receives from the lender. In all periods of the SE treatment, the borrower has zero capital \(C_t = 0\). Thus, in accordance with previous experiments on repeated investment games (e.g., Cochar et al. 2004), borrowers in the SE treatment start from scratch in each period and can only invest if they obtain a loan.

The decision structure in each period is as follows:

- **Loan offer:** The lender receives an endowment of 10 units at the beginning of each period. The lender can offer a loan size of \(S_t \in [0, 10]\) to the borrower. The lender also chooses her requested repayment \(R_t\), where \(R_t \in [0, vS_t]\).

- **Loan acceptance:** If the lender chooses an offer with a strictly positive loan \(S_t > 0\), the borrower must decide whether to accept \((A_t = 1)\) or reject the offer \((A_t = 0)\).

- **Repayment decision:** If the borrower accepts a loan offer \((S_t, R_t)\), he decides whether to make the repayment requested by the lender \((D_t = 0)\) or default \((D_t = 1)\).

Both the lender and the borrower receive a symmetric “reservation” income of 10 points per period, if they decide not to trade. This design choice was made so that asymmetric reservation payoffs would not affect the decisions of lenders to offer credit. The income of the lender in each period is equal to her reservation payoff plus her net income from lending as shown below:

\[
\pi_t = \begin{cases} 
10 & \text{if no loan } (S_t = 0 \text{ or } A_t = 0), \\
10 - S_t + R_t & \text{if loan repaid } (S_t > 0, A_t = 1, D_t = 0), \\
10 - S_t & \text{if loan default } (S_t > 0, A_t = 1, D_t = 1).
\end{cases}
\]

The income of the borrower is equal to his reservation payoff plus his investment income \(v \cdot I_t = v \cdot S_t\) minus the repayment he makes to the lender \(R_t\):

\[
u_t = \begin{cases} 
10 & \text{if no loan } (S_t = 0 \text{ or } A_t = 0), \\
10 + v \cdot S_t - R_t & \text{if loan repaid } (S_t > 0, A_t = 1, D_t = 0), \\
10 + v \cdot S_t & \text{if loan default } (S_t > 0, A_t = 1, D_t = 1).
\end{cases}
\]

At the end of each period, the lender is informed about the borrower’s repayment decision. The lender and the borrower are informed of their own and their partner’s payoffs for the period. They also see an overview of the history of their bilateral interactions in previous periods, showing past loan sizes and requested repayments of the lender, and acceptance and repayments by the borrower.

### 2.2. Weak Exclusion Treatment

In the weak exclusion treatment a lender and a borrower are also paired for seven periods and make the same decisions as in the SE treatment in each period. The WE treatment differs from the SE treatment only with respect to the consequences of a loan default for the borrower’s current period payoff and his capital. In the WE treatment, the borrower has zero capital in period 1. However, if the borrower receives a loan and does not repay it, he can keep the lender’s funds for future investment. We assume that borrowers who default in period \(t\) automatically have the loan principal \(S_t\) added to their capital for all subsequent periods. The borrower liquidates his capital (and consumes the proceeds) in the final period. The capital of a borrower in periods \(t = \{2, \ldots, 7\}\) thus equals the sum of the borrowed funds which he did not repay:

\[
C_t = \sum_{k=1}^{t-1} D_k \cdot S_k.
\]

The fact that we force borrowers to reinvest funds that they keep after default, rather than allowing them to decide whether to consume or reinvest them, seems restrictive. We chose this design for two reasons. First, we wanted to simplify the game as much as possible by abstracting from endogenous consumption/saving decisions. Second, reinvestment of loaned funds is the optimal strategy of a borrower who has defaulted.

The income of the borrower in periods \(t = \{1, \ldots, 6\}\) is equal to his reservation payoff plus his investment income \(v \cdot I_t\), minus the repayment he makes to the lender \((R_t)\) and minus the capital that he keeps for the subsequent period, \(C_{t+1} = C_t + D_t \cdot S_t\). In periods \(t = \{1, \ldots, 6\}\), the capital is thus deducted from the investment income and transferred to the subsequent period, leading to the following payoffs:

\[
u_{1, \ldots, 6} = \begin{cases} 
10 + (v - 1) \cdot C_t & \text{if no loan } (S_t = 0 \text{ or } A_t = 0), \\
10 + (v - 1) \cdot C_t + v \cdot S_t - R_t & \text{if loan repaid } (S_t > 0, A_t = 1, D_t = 0), \\
10 + (v - 1) \cdot (C_t + S_t) & \text{if loan default } (S_t > 0, A_t = 1, D_t = 1).
\end{cases}
\]

At the end of period 7, the borrower liquidates his capital and consumes it. We make this assumption to ensure that repayment behavior in the final period of
the WE treatment has the same payoff implications as in the SE treatment. His payoff in this period is thus

\[ u_{t=7} = \begin{cases} 
10 + v \cdot C_t & \text{if no loan} \; (S_t = 0 \text{ or } A_t = 0), \\
10 + v \cdot (C_t + S_t) - R_t & \text{if loan repaid} \; (S_t > 0, A_t = 1, D_t = 0), \\
10 + v \cdot (C_t + S_t) & \text{if loan default} \; (S_t > 0, A_t = 1, D_t = 1).
\end{cases} \]

As in the SE treatment, both the lender and the borrower are provided with an overview of the history of the relationship, including any capital accumulated by the borrower.

2.3. Discussion

Some features of our experimental design warrant detailed discussion. First, it is important to clarify the underlying assumptions about the debt-enforcement environment in our two treatments. In both the WE and the SE treatment, borrowers can strategically default on their loans. If the borrower defaults, the lender cannot recover any part of the loaned funds. This design feature is common to previous experimental studies of repeated investment games (e.g., Cochard et al. 2004) and relational contracting in credit markets (e.g., Brown and Zehnder 2007). The assumption of a zero recovery rate implies either a very costly process of enforcing debt or legal impediments to seizing assets of borrowers.

The World Bank Doing Business database documents that in many countries the judicial procedures to enforce debt are indeed very costly, even when lenders have secured claims. In 91 of 198 countries, the estimated recovery rate on a secured loan amounts to less than one-third of the claim. Costly debt enforcement is particularly prevalent in low-income and emerging economies and negatively impacts on credit market development (Djankov et al. 2008). Recovery rates may be low in high-income countries as well. For example, in many U.S. states, a broad range of household assets are exempt from the bankruptcy process (see, e.g., White 1998), with negative consequences for the supply of consumer credit (Gropp et al. 1997) and small business finance (Cerqueiro and Penas 2017). Moreover, corporate borrowers may be able to tunnel assets out of their current business (Johnson et al. 2000, Jiang et al. 2010). Thus, in both low-income and high-income countries, the assumption of a low recovery rate on defaulted loans seems reasonable for many credit markets.

The difference between the SE and WE treatments lies in what a borrower can do with the funds when he defaults. In the SE treatment, if the borrower defaults, he must liquidate his investment and consume the proceeds within the same period. This treatment inherently implies that although the recovery rate for lenders is low, the debt enforcement process is very fast. Borrowers cannot hold on to and reinvest appropriated funds. By contrast, in the WE treatment, a defaulting borrower can retain borrowed funds and reinvest these in future periods. This treatment assumes that the debt enforcement process is not only very costly, but also very slow. The Doing Business database documents that, especially in low-income countries, the debt enforcement process is very slow. For example, in 109 of 198 countries, the judicial enforcement process for secured claims takes more than two years in the case of a bankruptcy. And unsurprisingly, those countries with low recovery rates on secured debt are typically characterized by lengthy enforcement processes. Our WE treatment captures this environment by allowing a defaulting borrower to continue to use borrowed funds for future investment without having to surrender either his assets or profits to the lender. Anecdotal evidence suggests that in countries with weak creditor protection, borrowers do appropriate and continue to use borrowed funds, for example, by tunneling them out of existing businesses into new ones.

A further assumption that warrants discussion is the investment limit for borrowers. We hold the investment opportunity of the borrower constant over time to examine credit rationing over the course of a relationship. If, for example, we observe that a lender offers a small loan in period 1 and she increases it over time, we know that the borrower was credit constrained in period 1. By contrast, when field studies observe rising loan schedules over time (Ioannidou and Ongena 2010, Kirschenmann 2016), they typically cannot distinguish whether this is due to improved investment opportunities of the borrower or a relaxation of credit constraints.

2.4. Procedures

We implement our two main treatments in a laboratory experiment. A total 186 students participated in the WE and SE treatments. Our first set of sessions was implemented at Tilburg University (90 participants). Our second set of sessions was implemented at the University of California, San Diego (96 participants). Each subject could only participate in one session, and in each session, only one of the treatments was implemented. As shown in Online Appendix D, we do not observe a significant difference in behavior in the lending game across locations, except for a small difference in acceptance rates in the SE treatment. We thus pool the observations from Tilburg University and UC San Diego. Additionally, we include a location fixed effect in all regressions.

At the beginning of each session, participants were randomly assigned to the role of either a borrower or a lender. These roles were fixed for the whole session. Each player formed part of a matching group,
composed of three lenders and three borrowers. Each player played three rounds of our lending game: each lender (borrower) repeated the lending game with the three different borrowers (lenders) in her or his matching group. As a consequence, we observe nine lender–borrower relationships for each matching group. In the WE treatment there were 15 matching groups, and in the SE treatment there were 16 matching groups. This implies that we observe 135 lender–borrower relationships in the WE treatment and 144 relationships in the SE treatment.

The experiment was programmed and conducted with the experimental software z-Tree (Fischbacher 2007). All sessions lasted approximately 90 minutes. Subjects at Tilburg University received a show-up fee of €5 and an additional €1 for every 25 points earned during the experiment. Subjects at UC San Diego received a show-up fee of US$10 and an additional US$1 for every 20 points earned during the experiment. On average, subjects earned €20.9 or US$30.0 for their participation.11

Before starting our lending experiment, each subject read a detailed set of instructions. The instructions can be found in Online Appendix A. The experimental instructions were framed in a credit context.12 After reading the instructions, participants had to pass a test with control questions. The lending game did not start until all subjects had correctly answered all control questions.

Behavior in our lending game might be affected by individual behavioral traits. First, individual risk preferences affect decisions in investment games (Eckel and Wilson 2004, Schechter 2007, Houser et al. 2010). Second, the level of strategic reasoning, i.e., the anticipation of what other subjects in the matching group might do, can affect behavior significantly (Nagel 1995). Third, social preferences, i.e., reciprocal motives and fairness preferences of the borrower, as well as the anticipation of these preferences, i.e., trust by the lender, should affect behavior in our experiment (see, e.g., Roe and Wu 2009).

Participants took part in three “games” aimed at measuring their levels of risk aversion (using a multiple choice list with a fixed lottery and increasing fixed payments), strategic reasoning (using a guessing game), and trust and trustworthiness (using a one-shot strategy-method trust game). In our first set of sessions (Tilburg), these games took place before the main experiment (we label them “preexperiment”). In our second set of sessions (San Diego), the games took place after the main experiment (we label them “postexperiment”).13 Online Appendix B describes these pre- and postexperiment games and provides summary statistics for their outcomes in our two treatments. We find no significant differences in behavior in these games across treatments in either location.14

3. Predictions

Under the assumption of common knowledge of rationality and selfishness of all market participants, the predictions for all three treatments are straightforward. Since repayments are not enforceable, a borrower’s best response is to never repay a loan in a one-period game. As it is public knowledge that the WE treatment and the SE treatment last for a finite number of periods, a backward induction argument ensures that there is no lending in any period of either treatment.

A broad body of experimental evidence suggests, however, that not all people will maximize monetary payoffs in our experiment. Social preferences based on reciprocity (Dufwenberg and Kirchsteiger 2004) or distributional concerns (Fehr and Schmidt 1999) can induce borrowers to repay loans even in one-shot interactions. Evidence from one-period investment games in the lab (Berg et al. 1995) and in the field (Karlan 2005) suggests that a substantial share of second movers, i.e., borrowers in our context, exhibit such social preferences and repay.

In the following we establish predictions for our treatments under the assumption that some (nonidentifiable) borrowers are conditionally reciprocal: they are willing to repay a loan in a one-shot situation, as long as the repayment requested by the lender does not exceed a threshold value. We assume that this threshold ℱ can be characterized by the maximum (gross) interest rate \( i_r = R_s / S_r \leq \bar{ℱ} \) that a social borrower is willing to pay. We assume that the remaining borrowers are selfish in the sense that they never repay loans in a one-shot situation. The share of social borrowers \( p \) is assumed to be \( 1/\bar{ℱ}^T \leq p < 1/\bar{ℱ} \). The assumption that \( p < 1/\bar{ℱ} \) implies that it is not profitable for risk-neutral lenders to lend in a one-shot game. The assumption \( p \geq 1/\bar{ℱ}^T \) implies that a repeated game equilibrium in which selfish borrowers repay with positive probability in initial periods exists.15

The outcomes of our preexperimental and postexperimental games, documented in Online Appendix B, suggest that there is a substantial share of social borrowers in both treatments. In particular, behavior in the one-shot, strategy-method trust game suggests that less than 15% of the subjects in the roles of borrowers can be characterized as pure money maximizers, who always default in a one-shot situation. By comparison, more than 25% of the subjects behave as social borrowers who are willing to repay a loan as long as the desired repayment implies that they earn at least one-half of the surplus. Applied to our experiment, this finding would imply that the above conditions on the share of social borrowers is satisfied in our sample.16

In the following we outline the main predictions for both treatments. All proofs are presented in Online Appendix C.17 Since borrower types are a priori indistinguishable, the WE and SE treatments can be
characterized as finitely repeated games of incomplete information. Such games have multiple equilibria (Kreps et al. 1982). We distinguish between two types of equilibria and, within each type, concentrate on the profit-maximizing equilibria for the lender (as in Thomas and Worrall 1994). In implicit contracting equilibria, selfish borrowers imitate the behavior of social borrowers during the first periods but default toward the end of the game. In screening equilibria, selfish borrowers default in the first period, and from period 2 onward the lender only extends credit to (now identified) social borrowers. It is important to note that in both treatments, the one-shot equilibrium of no lending is also feasible.

In the SE treatment, the profit-maximizing (and surplus-maximizing) implicit contracting equilibrium features loans of maximum size 10 in periods 1–6 and a smaller loan of $10 \cdot \frac{f}{v}$ in period 7. The interest rate is $i_t = \bar{r}$ in each period. Loan offers in periods $t = \{2, \ldots, 7\}$ are contingent on the borrower repaying all past loans. The incentive constraint of a selfish borrower in period $t$ is as follows:

$$\sum_{k=t}^{T-1} (v - i_k) \cdot S_k + v \cdot S_T \geq v \cdot S_t. \quad \text{(IC}_{\text{SE}})$$

Since loans are of size 10 for periods 1–6, $\text{IC}_{\text{SE}}$ is satisfied with inequality in these periods. The smaller loan size in period 7 implies that the constraint is satisfied with equality in period 6. Thus, in this period the selfish borrower is indifferent between repaying and defaulting, and defaults with a strictly positive probability. This allows the lender to learn about the borrower’s type in period 6 and to break even in period 7.

A screening equilibrium is not feasible in the SE treatment. By definition, in such an equilibrium, selfish borrowers would default with certainty in the first period of the game. In the following periods, the lender would offer maximum loans of 10 to the borrowers who did not default, i.e., social borrowers. However, given that the lender offers maximum loans in subsequent periods, a selfish borrower has no incentive to default in the first period. It is impossible for the lender to offer a contract that does not meet $\text{IC}_{\text{SE}}$ in the initial period if $i_1 \leq \bar{r}$. We summarize these results in Proposition 1.

**Proposition 1.** In the SE treatment, the profit-maximizing implicit contracting equilibrium features the maximum credit volume in periods 1–6 and no defaults in periods 1–5. A screening equilibrium is not feasible in this treatment.

In the WE treatment, the potential to reinvest borrowed funds in future periods increases the borrower’s incentive to default. This can be seen from the selfish borrower’s incentive constraint for this treatment:

$$\sum_{k=t}^{T-1} (v - i_k) \cdot S_k + v \cdot S_T \geq v \cdot S_t + \sum_{k=t}^{T-1} (v - 1) \cdot S_t. \quad \text{(IC}_{\text{WE}})$$

Implicit contracting equilibria are also feasible in the WE treatment. However, these equilibria must be characterized by “progressive lending”: to meet the borrower’s incentive constraint, the lender must start with nonmaximum loans and increase the loan size offered to the borrower if he repays. The intuition for this result is simple: if the lender offers the maximum loan of 10 in period 1, a selfish borrower could default and reinvest these funds in all future periods without paying interest. The selfish borrower only stands to gain from repaying initial loans if future loans are higher.

In contrast to the SE treatment, a screening equilibrium exists in the WE treatment. If the lender offers a large enough loan in the first period, a selfish borrower prefers to default straight away. For example, from $\text{IC}_{\text{WE}}$ we see that a selfish borrower will not repay a loan offer with $S_1 = 10$ and $i_t = \bar{r}$, while a social borrower will repay such a loan. These results are brought together in Proposition 2.

**Proposition 2.** In the WE treatment, an implicit contracting equilibrium must feature “progressive lending.” Loan offers increase gradually over time and there are no defaults in periods 1–5. In this treatment, a screening equilibrium is also feasible in which selfish borrowers default with certainty in the first period.

Based on the above predictions, we establish the following empirical hypotheses. Our null hypothesis is that there is no difference in observed lending outcomes between the SE and WE treatments: The shares of self-enforcing credit relationships in which loans are offered, accepted, and repaid for at least the first five periods are similar. Moreover, the average loan sizes and interest rates are alike. Our alternative hypothesis is that we will observe a marked difference in lending outcomes between the WE and SE treatments. The nature of this difference will depend on the type of equilibria that dominate empirically in the WE treatment. We outline the expected impact of weak exclusion if mainly implicit contracting equilibria emerge in the WE treatment (H1) and if mainly screening equilibria emerge (H2). Empirically, a combination of these two types of equilibria is likely to occur.\(^8\)

**Hypothesis 1 (H1)** (Implicit Contracting Dominates in the WE Treatment). In both treatments there is a similar share of self-enforcing credit relationships, i.e., credit relationships that are characterized by loan offers and loan repayment up to at least period 5. The average loan size in the SE treatment is higher than that in the WE treatment, as the WE treatment is characterized by progressive lending. Loan sizes rise steadily in the WE treatment, while they exhibit a drop in the last period in the SE treatment. Interest rates do not differ between the treatments.

**Hypothesis 2 (H2)** (Screening Dominates in the WE Treatment). In the WE treatment there is a lower share of
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4. Main Results
4.1. Credit Relationships and Credit Terms

In this section, we test our main hypotheses by comparing three outcomes of interest across the WE and SE treatments: the share of self-enforcing credit relationships, i.e., relationships in which a loan is offered, accepted, and repaid for at least the first five periods, as well as the average Loan size and the average Interest rate (requested repayment/loan size) for accepted loan offers. Our unit of observation is matching-group averages. We hereby compare the outcomes of the 15 matching groups in the WE treatment to the 16 matching groups in the SE treatment using two-sided nonparametric and parametric tests. Since we are comparing three outcomes, we correct for multiple hypothesis testing using Bonferroni adjustments on nonparametric Mann–Whitney (MW) tests and the adjustment proposed by List et al. (2016) on parametric t-tests.

Figure 1 displays the share of credit relationships in which a loan was offered, accepted, and repaid in each period. Figure 1 reveals that credit relationships break down much more frequently in the WE treatment compared to the SE treatment. In the first period, only 44% of the 135 relationships in the WE treatment are characterized by a loan being offered, accepted, and repaid, compared to 73% of the 144 relationships in the SE treatment. This difference is driven by a lower number of loans offered (90% versus 99%), a lower acceptance ratio of offers made (76% versus 89%), and a lower repayment rate for offers that have been accepted (65% versus 84%) in the WE treatment.

The share of relationships featuring a loan offer in period 2 is 78% in the WE treatment and 92% in the SE treatment. Thus, lenders not only continue those relationships that survive the first period, but also attempt to restart many of the relationships that break down in period 1 (61% and 72% of the relationships that break down in period 1 in the WE and SE treatments, respectively, feature a loan offer in period 2). However, the share of relationships in which the period 2 offer is also accepted and repaid again declines to 44% in the WE treatment and 71% in the SE treatment. This pattern repeats itself through period 6.

Figure 1 suggests that the SE treatment is characterized by more self-enforcing credit relationships than the WE treatment. This conclusion is confirmed by the end-game behavior in period 7. In the SE treatment, the majority of credit relationships fall subject to the end-game effect: Borrowers default more frequently, and more lenders refrain from offering credit in the final period.

In line with Figure 1, we observe a substantial difference in the share of self-enforcing contracting
Table 1. Main Treatment Effects

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<th>Mean</th>
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Notes. This table presents the means of our three main outcome variables for each treatment. We also present the results of the Mann–Whitney test on the treatment differences, including the naive p-values and the Bonferroni adjusted p-values for multiple hypothesis testing. We also present the results of t-tests for the treatment differences, including the naive p-values and the p-values using the adjustment for multiple hypothesis testing proposed by List et al. (2016).

relationships, i.e., relationships in which a loan is offered, accepted, and repaid for at least the first five periods, between the SE treatment (48%) and WE treatment (12%). Table 1 displays the results of the non-parametric and parametric tests, which reveal that this treatment difference is statistically significant.

Figure 2 presents the credit terms for accepted loan offers, showing the average Loan size (panel A) and the average Interest rate (panel B) by period. The treatment difference between the average loan size increases from period 1 (5.20 versus 5.97) to period 7 (5.25 versus 7.89). Over all periods, the average loan size is significantly smaller in the WE treatment (5.00) than in the SE treatment (6.90). As shown in Table 1, this difference is statistically significant. Panel B of Figure 2 documents that the average interest rate is similar in all periods in both treatments. Average interest rates do not differ significantly between the WE treatment (1.94) and the SE treatment (1.95), as shown in Table 1. The observed interest implies (upon repayment) an equal sharing of surplus between the lender and borrower, in accordance with our behavioral assumptions.

The impact of the WE treatment on credit relationships and credit terms has implications for the distribution of payoffs between the lender and the borrower. Lenders earn less in the WE treatment than in the SE treatment (10.74 versus 13.04 per period). In contrast, borrowers earn more in the WE treatment than in the SE treatment (20.20 versus 17.98 per period).

Result 1 (Credit Relationships and Credit Terms). In the WE treatment there is a lower share of self-enforcing credit relationships than in the SE treatment. The average loan size is also smaller in the WE treatment than in the SE treatment. Interest rates do not differ across treatments.

To what extent are the findings presented in Result 1 noteworthy? Maniadis et al. (2014) argue that the judgment of novel empirical findings should not rely solely on an assessment of statistical significance. They emphasize that the probability that a significant treatment effect represents a true association depends on the level of significance (\( \alpha \)) and power of our empirical test (\( 1 - \beta \)) as well as our prior (\( \pi \)) about the probability with which our alternative hypothesis
Table 2. Poststudy Probability Calculations

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<td>0.5</td>
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<td>0.83</td>
<td>0.89</td>
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Notes. This table illustrates the significance of the treatment differences between the WE and SE treatments by calculating the PSP for one of our outcome variables: the share of self-enforcing relationships. The PSP is calculated in line with Maniadis et al. (2014) as $(1 − β) \cdot π/(1 − β) \cdot π + α \cdot (1 − π)$, where $α$ is the level of significance (0.05), $1 − β$ is the ex ante power of our empirical test, and $π$ is our prior about the probability of the alternative hypothesis occurring.

4.2. Lender Behavior

Our above findings show more loan defaults in initial periods in the WE treatment than in the SE treatment, suggesting less implicit contracting and more screening in the WE treatment. However, we also find smaller loans in the WE treatment, suggesting that self-enforcing agreements were (at least) attempted in this treatment. If lenders attempt to initiate implicit contracts in both treatments, we should see lower initial loan sizes offered in the WE treatment than in the SE treatment. Panel A of Figure 3 displays the empirical cumulative distribution function of the Loan size offered by lenders to borrowers in the first period in the WE and SE treatments. The figure reveals that small loans are more frequent in the WE treatment, where close to 70% of lenders offer a loan smaller than 5. By contrast, in the SE treatment, only 49% of lenders offer a loan smaller than 5. Panel B of Figure 3 shows that the distribution of Interest rate offered in first-period loan offers is similar in the WE and SE treatments. In both treatments, the surplus sharing interest rate of 2 is most common.

Table 3 reports the results of a multivariate analysis relating first-period loan offers to the treatment (WE or SE) and characteristics of the lender. The estimated coefficient of the dummy variable WE Treatment in column (3) confirms that first-period loans in the WE
Table 3. First-Period Loan Offers

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Notes. This table reports ordinary least squares (OLS) estimates for the dependent variables Loan size (columns (1)–(4)) and Interest rate (columns (5)–(8)) using observations from first-period loan offers only. Loan size is the loan offered by lenders, taking values from 0 to 10. Interest rate is the repayment requested divided by loan size and takes values 0 to 3. WE is a dummy variable that is 1 for all observations from the WE treatment and 0 for those from the SE treatment. All regressions include round fixed effects, whereby round 1 is the omitted category, and location fixed effects, where Tilburg University is the omitted category. The variables Risk aversion, Strategic reasoning, and Trust are lender-specific measures elicited from pre- and postexperiment games. Standard errors are reported in brackets and are corrected for clustering at the matching group level.

∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% levels, respectively.

The treatment are on average 1.3 points lower than in the SE treatment. The coefficients of Round 2 and Round 3 in columns (1) and (2) and those of the interaction terms WE treatment × Round 2 and WE treatment × Round 3 in column (4) suggest that the treatment effect is strengthened as subjects become more experienced.

The variation in initial loan offers across lenders is related to individual risk attitudes of lenders as well as the degree to which they trust other participants. In Table 3 we control for three measures of lender characteristics—Risk aversion, Strategic reasoning, and Trust—using data from the pre- and postexperiment games from Section 2.4. We find that lenders with higher indicators of Risk aversion and lenders who exhibit less Trust in other participants offer smaller period 1 loans in both treatments, but that this is effect is larger and more precisely estimated in the WE treatment.

For periods 2–7, we predict that in both treatments the renewal of loan offers by lenders from one period to another should be strongly contingent on the repayment of past loans. Moreover, as implicit contracts must be characterized by progressive lending in the WE treatment, we expect that—conditional on past loan repayment—the offered loan size increases more strongly over time in the WE treatment than in the SE treatment.

We find that loan offers are strongly contingent on past repayment in both treatments. If the borrower repaid a loan in the previous period, lenders offer a loan in the next period in 98% of the cases in the WE treatment and 97% of the cases in the SE treatment. By contrast, if the borrower defaulted, lenders offer a loan in 61% of the cases in the WE treatment and 62% of the cases in the SE treatment.
higher than in period 1. The negative coefficient of WE treatment (column (3)) indicates that the loan size offered to borrowers who repaid in the past is significantly lower in the WE than in the SE treatment. The negative (but insignificant) interaction terms of WE \times Period 2–3 and WE \times Period 4–5 (column (4)) suggest that the loan sizes offered to borrowers do not increase faster in the WE treatment than in the SE treatment.

In Table 5, we compare the resulting time structure of loan sizes within credit relationships across the two treatments. We define the ultimate length of a relationship as the number of periods for which the relationship survived, i.e., loans were offered, accepted, and repaid in every previous period. A relationship in which a loan was offered and accepted in period 1 but not repaid is defined as having an ultimate length of zero periods. By contrast, a relationship that involved positive loan offers in all periods and in which the borrower always accepted and repaid the loan has an ultimate length of seven periods. We define a long-term relationship as a relationship with an ultimate duration of at least five periods. In line with the results presented in Figure 1, only 16 of 135 relationships in the WE treatment (12%) are long term, compared to 69 of 144 relationships in the SE treatment (48%).

Long-term relationships in the WE treatment start off with somewhat smaller loan sizes than long-term relationships in the SE treatment. The average first-period loan for these relationships is 5.8 in the WE treatment compared to 6.5 in the SE treatment. This difference is, however, not statistically significant. By period five, the average loan size in long-term relationships in the WE treatment increases by 1.8 points to 7.6. This confirms our prediction that long-term relations in the WE treatment should, on average, be characterized by increasing loan sizes. However, we find a similar pattern in the SE treatment. In this treatment, the average loan size in period 5 is 8.6, implying an even stronger increase of 2.3 points from period one.

Result 2 (Lender Behavior). Lenders in the WE treatment offer smaller loans in the first period of a relationship compared to those in the SE treatment. Long-term credit relationships in the WE treatment display an increase in loan sizes over time. However, “progressive lending” is observed to a similar degree in the SE treatment.

Result 2 supports the conjecture that a substantial fraction of lenders attempt to start implicit agreements with small loan sizes in the WE treatment. However, the long-term relationships that emerge do not exhibitsubstantially lower initial loan sizes or a stronger time trend in the WE compared to the SE treatment. Two central features in the data seem to explain this behavior. First, as we will see in the next subsection, borrowers in the WE treatment are significantly more likely to reject small loan offers in the initial period of a relationship. Second, loan sizes exhibit an increasing time trend in the SE treatment as well, a finding that has been documented previously in finitely repeated trust games (Anderhub et al. 2002, Cochard et al. 2004, King-Casas et al. 2005, Bornhorst et al. 2010). Our results are in line with these previous findings and suggest that a weak threat of exclusion is not a necessary condition for progressive lending in credit relationships plagued by moral hazard.

4.3. Borrower Behavior

Figure 4 displays the frequency with which borrowers reject loan offers (panel A) and default on accepted loans (panel B) in period 1 by treatment. Table 6 presents results of a corresponding multivariate analysis of borrower behavior in period 1.

In period 1, 25% of all loan offers are rejected in the WE treatment, while 11% of the loan offers in the SE treatment are rejected (see Figure 1). For both
treatments, Figure 4 displays the highest rejection rate among credit offers with Interest rate above 2 (58% in the WE treatment, 50% in the SE treatment). These offers propose an unequal split of surplus to the disadvantage of the borrower. This is in line with our predictions that “social” borrowers will not accept unfair loan offers. The regression results in Table 6 confirm that loan offers that propose Interest rate > 2 are significantly more likely to be rejected than loan offers that propose an equal share of surplus to the borrower. There is no difference in the rejection rate of “unfair” offers between the WE and SE treatments, as captured by the insignificant interaction term WE × Interest rate > 2 in column (4) of Table 6. By contrast, loan size is treated differently by borrowers in the WE and SE treatments. The estimate for WE × Loan < 5 in column (4) reveals that offers with small loan sizes are significantly more likely to be rejected in the WE treatment than in the SE treatment. This finding suggests that some lenders who attempted to start with small loans could not do so due to borrower rejections.

The default rate on first-period loans that are accepted by borrowers is 35% in the WE treatment compared to 16% in the SE treatment. Panel B of Figure 3 shows that in the WE treatment, the default rate on first-period loans is especially high for larger loans and for loans that propose an “unfair” split of surplus. This result is confirmed by the column (5) estimates in Table 6. In the WE treatment, accepted offers with a loan size of less than 5 (Loan < 5) are 19 percentage points less likely to suffer a default, while offers with Interest rate > 2 are 25 percentage points more likely to suffer a default. While both of these coefficients are imprecisely estimated, they are jointly significant (F-test = 0.020), suggesting that large loans with high interest rates are 45 percentage points more likely to be defaulted on than small loans with low interest rates in the WE treatment. In the SE treatment, by contrast, we find only a weak impact of the loan size and the interest rate on first-period loan defaults. The estimated coefficients for Loan size < 5 and Interest rate > 2 are jointly insignificant (F-test = 0.764). These results suggest that dynamic incentives to repay are substantially stronger in the initial periods of the SE treatment.

Result 3 (Borrower Behavior in Period 1). In the initial period, borrowers in both treatments are more likely to reject loan offers that propose an unfair sharing of surplus. In the WE treatment, borrowers are also more likely to reject period 1 loan offers with small loan sizes. Borrowers in the WE treatment are much more likely to default on large loans with high interest rates than borrowers in the SE treatment.

The default behavior reported in Result 3 points to screening in the WE treatment. Borrowers who were offered larger loan sizes are more likely to default. At the same time, rejection behavior indicates that implicit contracts are more difficult to establish in the WE treatment. Our findings show that borrowers in the WE treatment often reject the low initial loan offers inherent to progressive lending. One explanation for the prevalence of this behavior is that in the WE treatment, social borrowers prefer screening equilibria (in which they receive maximum loan sizes in all periods) to implicit contracting equilibria, which feature progressive lending. Thus, they may have tried to signal a desire to be tested with larger loans—a test that is only meaningful in this treatment.

5. Lender Competition

Weak exclusion has important effects on implicit contracting in bilateral credit relationships, as shown thus
Figure 4. (Color online) Borrower Behavior in Period 1

Panel A. Rejection

Panel B. Default

Notes. Panel A displays the average Rejection rate in period 1 over Loan size (left) and over Interest rate (right), by treatment. Panel B displays the average Default rate over Loan size (left) and over Interest rate (right), by treatment. The average rejection and default rates are calculated considering each matching group average as one independent observation.
Table 6. Borrower Behavior in Period 1

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<td>93</td>
<td>43</td>
<td>48</td>
<td>93</td>
<td>91</td>
</tr>
<tr>
<td>Overall R²</td>
<td>0.197</td>
<td>0.282</td>
<td>0.231</td>
<td>0.254</td>
<td>0.088</td>
<td>0.013</td>
<td>0.072</td>
<td>0.095</td>
</tr>
</tbody>
</table>

Notes. This table reports panel estimates for Reject (columns (1)–(4)) and Default (columns (5)–(8)) in surviving relationships in period 1. Reject is a dummy variable that takes the value 1 if the borrower rejects the lender’s offer and 0 otherwise. Default is a dummy variable that takes the value 1 if the borrower does not repay an accepted loan offer. Loan size < 5 is a dummy variable that takes the value 1 if the loan size is 1–4 and 0 otherwise. Interest rate > 2 is a dummy variable that takes the value 1 if Interest rate (requested repayment/loan size) exceeds the surplus sharing rate of 2 and 0 otherwise. WE is a dummy variable that is 1 for all observations from the WE treatment and zero for those from the SE treatment. All regressions include round fixed effects and the interaction of round fixed effects with the treatment dummy (columns (3), (4), (7), and (8)). Standard errors are clustered at the matching group level and reported in brackets. OLS, ordinary least squares.

‘∗’, ‘∗∗’, ‘∗∗∗’ indicate significance at the 10%, 5%, and 1% levels, respectively.

In this section, we examine whether weak exclusion has a similar effect on credit relationships in a competitive environment. Specifically, we examine the effects of weak exclusion when lenders compete for borrowers. We study lender competition (rather than, e.g., borrower competition) as theory suggests that competition between lenders may strongly influence the emergence and time structure of credit relationships. For example, Sharpe (1990) and Petersen and Rajan (1995) show that competition between banks may undermine the ability of lenders to earn quasi rents in ongoing lending relationships, and therefore may reduce their incentive to engage in such relationships from the outset.

We implement lender competition by pairing two lenders with one borrower. The lending game is identical to that without competition, except for the following changes: First, both lenders make a credit offer to the borrower simultaneously at the beginning of each period. Second, the borrower can accept one offer (or none). Third, at the beginning of each period, each lender is informed about the past credit volume accepted by the borrower and his repayment behavior, but not the interest rate. We implement a treatment with lender competition for our weak exclusion condition (WE Competition treatment) and our strong exclusion condition (SE Competition treatment).

5.1. Predictions

In this section, we discuss the main differences in predictions for the WE Competition and SE Competition treatments compared to our main treatments with bilateral interaction. Detailed predictions are provided in Online Appendix E.

Implicit contracting in the SE Competition treatment exhibits a similar pattern of loan sizes compared to our main SE treatment with bilateral interaction. Implicit contracting equilibria are characterized by maximal loan offers throughout the first six periods and a drop in period 7. However, interest rates are lower with lender competition: they are at the break even rate ($i_t = 1$), rather than the surplus sharing rate ($\bar{r} = 2$), since lenders compete to attract the borrower. Note that since there are two symmetrical lenders with identical information about past repayment, a relationship can emerge with one of the lenders or there can be switching between lenders. We refer to an active credit market as an environment in which at least one offer is made and one offer is accepted and repaid in each period.

The set of equilibria in the WE Competition treatment changes compared to our WE treatment with bilateral trading. In particular, screening equilibria do not exist under weak exclusion with lender competition. The reason is that, as both lenders are equally
informed about past repayment behavior, borrowers are not informationally captured by their incumbent lender. In expectation, therefore, lenders cannot recoup losses from defaulting selfish borrowers in the first period by earning quasi rents on social borrowers in subsequent periods. In the WE Competition treatment, implicit contracting equilibria exist, but exhibit a different time pattern of loan volumes. Instead of progressively increasing loan volumes, lenders offer a constant loan volume from periods one to six, which is strictly smaller than the maximal loan offer. In the last period, they increase their loan offer, offering the maximal loan size, to induce indifference between repayment and default among borrowers in period 6. The constant loan size in nonfinal periods is self-enforcing, as lenders demand the break-even interest rate ($i_t = 1$).

Our null hypothesis for the comparison of the WE Competition and SE Competition treatments is again that we observe no differences in lending outcomes. Our alternative hypothesis suggests that we observe differences in loan sizes, but not in the frequency of self-enforcing credit relationships:

**Hypothesis 3** (H3) (Lender Competition). Self-enforcing credit relationships, i.e., credit relationships in which loans are offered, accepted, and repaid for at least five periods, are equally likely in the WE Competition and SE Competition treatments. The average loan size is, however, lower in the WE Competition treatment than in the SE Competition treatment. Interest rates are similar in both treatments.

### 5.2. Procedures

The treatments with lender competition were designed and implemented following the same procedures as our bilateral trading treatments. The only major difference was that two lenders were matched with one borrower in each round. To keep the perfect stranger matching used in the main experiment, each matching group consisted of 12 (instead of 6) subjects.

We ran 12 sessions with lender competition at UC San Diego with one matching group per session. A total of 144 subjects participated in these sessions, whereby each subject participated in one treatment only. Six sessions, i.e., matching groups, were run for the WE Competition treatment and six sessions were run for the SE Competition treatment. After the subjects completed three seven-period rounds of the lending game, they played the postexperiment games as outlined above.

### 5.3. Results

Our aim in this section is to examine whether the main treatment differences documented for our main WE and SE treatments also hold with lender competition. Our discussion of the results for the SE Competition and WE Competition treatments is therefore limited...
Table 7. Treatment Effects with Lender Competition

<table>
<thead>
<tr>
<th>Outcome variable</th>
<th>WE competition treatment</th>
<th>SE competition treatment</th>
<th>Mann–Whitney test</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Naïve p-value</td>
<td>Bonferroni adjusted p-value</td>
<td></td>
</tr>
<tr>
<td>Share of self-enforcing relationships</td>
<td>0.35</td>
<td>0.50</td>
<td>0.0609</td>
</tr>
<tr>
<td>Loan size</td>
<td>7.40</td>
<td>8.52</td>
<td>0.0250</td>
</tr>
<tr>
<td>Interest rate</td>
<td>1.27</td>
<td>1.31</td>
<td>0.7488</td>
</tr>
</tbody>
</table>

Notes. This table presents the means of our three main outcome variables for each lender competition treatment. We also present the results of the Mann–Whitney test on the treatment differences, including the naïve p-values and the Bonferroni adjusted p-values for multiple hypothesis testing.

...to replicating the analyses presented in Section 4.1. As we did there, we compare three outcomes: the share of self-enforcing credit relationships, i.e., relationships in which a loan is offered, accepted, and repaid for at least the first five periods, and the average Loan size and Interest rate for accepted loan offers. We measure our outcome variables again at the matching group level and thus compare six observations for the WE Competition treatment against six observations for the SE Competition treatment. Because of the lower number of observations for this robustness test, we confine our tests to nonparametric methods (Mann–Whitney test) and account for multiple hypothesis testing by adjusting p-values using the Bonferroni method.

Figure 5 displays the share of credit markets in which a loan is offered, accepted, and repaid by period in the WE Competition and SE Competition treatments. In the WE Competition treatment, 72% of the credit markets survive period 1 (i.e., a loan is offered, accepted, and repaid), while this is the case for 82% of the markets in the SE Competition treatment. By period 3, the gap in credit market performance widens. In the WE Competition treatment, 53% of the markets feature a repaid loan, while 75% of the markets do so in the SE Competition treatment. This gap remains until the end of period 5. By periods 6 and 7, the gap between the WE and SE Competition treatments closes again due to the end-game effect in the SE Competition treatment.

Table 7 summarizes our treatment effects under lender competition. In line with Figure 5, we find a smaller share of self-enforcing relationships in the WE treatment (35%) compared to the SE treatment (50%). However, the nonparametric tests presented in Table 7 reveal that this treatment difference is not statistically significant after adjusting for multiple hypothesis testing (MW test, adjusted p-value = 0.183).

Figure 6 displays the credit terms of accepted loan offers in the competition treatments. In line with our predictions, accepted loan offers are smaller in all periods in the WE Competition compared to the SE Competition treatment. By contrast, interest rates are similar in both treatments. Table 7 shows that the average Loan size of accepted loan offers over all periods is 7.40 in the WE Competition treatment compared to...
8.52 in SE Competition treatment (MW test, adjusted p-value = 0.075). Table 7 reveals no difference in average interest rates in the WE Competition treatment (1.27) compared to the SE Competition treatment (1.31; MW test, adjusted p-value = 1.00).

Comparing the treatment effect of weak exclusion on lending in competitive credit markets (Table 7) relative to bilateral trading environments (Table 1), we find in both cases a negative effect on the share of self-enforcing credit relationships and on average loan sizes. The magnitude of the effect of weak exclusion on the average loan size is similar in the both trading environments. However, in line with Hypothesis 3, the magnitude of the effect of weak exclusion on the share of self-enforcing contracts is substantially smaller under competition (15 percentage points) than under bilateral trading (36 percentage points) and has much weaker statistical significance.

**Result 4 (Lender Competition).** Under lender competition, weak exclusion is associated with smaller loan sizes and a small, nonsignificant decrease in the share of self-enforcing relationships. Compared to a bilateral trading environment, the magnitude of the effect of weak exclusion on the share of self-enforcing credit relationships is weaker under lender competition.

### 6. Conclusion

In this paper, we examine how the threat of exclusion impacts on self-enforcing agreements in contracting environments plagued by moral hazard. The results of our lending experiment suggest that weak exclusion leads to a more frequent breakdown of lending relationships as well as to smaller loan sizes. These effects are substantial with bilateral trading, and are moderated with lender competition. Our findings provide a new perspective on the conditions under which self-enforcing agreements may—or may not—be a viable instrument for managers to mitigate conflicts of interest with employees, creditors, and clients. Implicit contracting is an effective enforcement strategy when agents value the relationship with their current trading partner. This may not be the case when agents can appropriate income-generating resources provided to them by principals. In this case, the threat of exclusion is weak, and our experiment suggests that implicit contracting is impaired.

Which markets and business relations are most likely to be subject to a weak threat of exclusion? This threat is particularly weak when (i) agents (employees, debtors, suppliers) use income-generating resources provided to them by the principal, (ii) a weak legal environment enables agents to appropriate these resources, and (iii) agents do not rely on the continued cooperation of the principal to generate future income with these resources. Bank lending in environments with weak creditor protection—the context of interest in our experiment—is a prime example of these conditions. Joint ventures in foreign direct investment are another—especially when the foreign partner has the technical know-how to use common assets independently. Likewise, investment relations with upstream firms may be subject to a weak exclusion threat if the supplier can appropriate technology and supply competing firms. Professional services, such as wealth management or consulting, in which employees may poach clients is another environment where a weak threat of exclusion may undermine incentives. In such markets and business relations, managers may need to resort to formal and costly enforcement mechanisms, e.g., monitoring, incentive contracts, or a reallocation of ownership, to mitigate conflicts of interest.

### Acknowledgments

The authors thank Eric van Damme, Hans Degryse, John Duffy, Karolin Kirschenmann, Ola Kvaloy, Sera Linardi, Jan Potters, Joel Sobel, and Stefan Trautmann, as well as seminar participants at Maastricht University, Simon Fraser University, Tilburg University, the University of East Anglia, the University of Nottingham, the University of Osnabrueck, Universitat Autonoma de Barcelona, Universitat Pompeu Fabra, the University of Zurich, the XIX Finance Forum, CESifo Munich, the International Meeting on Experimental and Behavioral Economics 2011, and the First LeeX International Conference on Theoretical and Experimental Macroeconomics at Universitat Pompeu Fabra for comments and suggestions. The authors also thank John List (department editor) and three anonymous referees for very helpful comments.

### Endnotes

1. It is, for example, estimated that 60%–90% of wealth management clients follow their advisor when they change firms (Gudz 2013).
2. Such an assumption has been shown to hold in a wide range of experiments. Furthermore, evidence of such types has been found in trust games and has been linked to actual repayment decisions in the field (Karlan 2005).
3. Falk et al. (2013) show that dismissal barriers can undermine relational contracting in labor markets. Thus similar to our paper, they examine how institutions affect the emergence of implicit agreements. In contrast to their paper, we examine institutional features inherent to the credit market (debt enforcement) and not only examine whether these institutions affect the emergence of implicit agreements, but also how these agreements are structured over time.
4. Starting small can also be rationalized by profit maximization of the principal who increases the stakes toward the end of the relationship such that he can extract a greater surplus in the beginning (e.g., Ray 2002), or by borrowing constraints that are endogenous to the dynamics of debt and thus make increasing loan sizes optimal (Albuquerque and Hopenhayn 2004).
5. While reputation concerns are constant in an infinite horizon, they are strong at the beginning and very weak at the end with a finite horizon. As shown, for example, by Brown and Zehnder (2007), self-enforcing credit agreements can be identified as relationships in which the borrower repays in nonfinal periods and then defaults in the final period.
Partial repayments are not possible. In reality, some borrowers obviously become delinquent without fully defaulting. However, because of the deterministic nature of investment earnings in our design, we exclude partial repayments.

Relative to existing experimental studies, we introduce only one change, i.e., that a defaulting borrower can reinvest borrowed funds, and hence there are no endogenous savings/consumption decisions in any of the treatments.

The Doing Business indicators of “Resolving Insolvency” compare the costs, time, and processes involved in a bank enforcing a secured claim on a nonfinancial firm that enters bankruptcy procedures. For details of the methodology and data, see http://www.doingbusiness.org/data/exploretopics/resolving-insolvency (accessed October 13, 2016).

For example, in the United States, when individuals file for personal bankruptcy under Chapters 7 or 13, several personal assets can be excluded from seizure during bankruptcy (e.g., owner-occupied residences, personal property, and retirement accounts). White (1998, p. 689) argues that “because few debtors have nonexempt assets, more than 90 percent of Chapter 7 cases involve no repayment at all, and the average repayment rate in Chapter 7 cases is less than 3 percent.”

A recent Economist article on bankruptcy proceedings in India reports that “At the moment, creditors cannot take any legal action against a defaulter until a restructuring plan is in place, which usually takes between three and ten years. In the meantime, the owners of sick firms retain day-to-day management control; and while they drag out court proceedings, creditors see their assets dwindle. In many cases, says a lawyer who works on such cases, the owners start up a fresh company in the name of a relative, and siphon off business from the old one” (The Economist 2015).

We chose to pay a higher show-up fee to subjects and decrease the exchange rate for two reasons. First, because of the euro to U.S. dollar exchange rate, which was 0.78 USD/EUR on October 29, 2014, shortly before we ran the sessions at UC San Diego. Second, we intended to add sessions with lender competition at UC San Diego (as will be detailed in Section 5), in which participants would make significantly less money for a similar amount of time in the laboratory. To avoid differential selection into sessions, we set the show-up fee to US$10 for all sessions conducted at UC San Diego.

The reason why we chose a context-specific and not a neutral framing was that the experiment was relatively complex. In complex experiments, a completely neutral language bears the danger that subjects will create their own (potentially misleading) interpretation of the decision environment. Thus, the context-specific framing gives us control over what our participants have in mind.

Because of logistical constraints, the location (Tilburg or San Diego) varied simultaneously with the order of the additional games (pre-versus postexperiment). We thus acknowledge that we can only measure the joint effect of location and order and cannot rule out that countervailing order and location effects partly offset each other. Our results suggest that location and order do not exert a significant joint effect on behavior in the additional games (as shown in Online Appendix B) or the lending game (as shown in Online Appendix D).

Throughout the pre- and postexperimental games, subjects received no feedback. They were not informed about other subjects’ decisions or their own payoffs until the end of the experiment. Subjects also knew that the decisions in each preexperimental/postexperimental game had no effect on the lending game (and vice versa). The instructions for these games are available from the authors upon request. We find that choices in the preexperimental versus postexperimental games are not significantly different in the risk elicitation task and guessing game. We find differences in the trust game in the SE but not the WE treatment. The higher trust and trustworthiness in the postexperimental games in the SE treatment suggests there was a spillover from successful interaction in the lending game onto the trust game.

As we outline in Online Appendix C, lending relationships with a positive repayment probability for selfish borrowers in nonfinal periods and certain default by selfish borrowers in the final period are feasible if $T \geq 2$ and $p \geq 1/R^2$. The higher the share of social borrowers $p$ in the population and the larger the number of periods $T$, the more periods with full repayment by all borrowers can be sustained.

In our experiment, $i = 2$ is the interest rate that generates the same payoffs for the lender and borrower if the borrower repays a loan. With $v = 3, i = R_i/S_i = 2$ implies that $R_i - S_i = v - S_i = R_i$. If we assume that $r = 2$ and $p = 0.25$, then as $T = 7$, the condition $1/R^2 \leq p < 1/r$ is met.

In deriving these predictions, we assume a finite horizon and risk neutrality of the lenders to simplify the analysis. These assumptions do not change the qualitative effects of weak exclusion, as we discuss in Online Appendix C.

Whether the profit-maximizing implicit contracting equilibrium or the profit-maximizing screening equilibrium yields higher profits to the lender depends on the gross return from investment ($v$) and the share of social borrowers. As shown in Online Appendix C.4, given the choice of $v = 3$ in our experiment and the assumption that the share of social borrowers is $p < 1/r$, where $r = 2$, the implicit contracting equilibrium yields higher profits to the lender than the screening equilibrium.

The strength of implicit contracting can also be measured by the average length of relationships in which there offers have been continuously made, accepted, and repaid, or, more generally, the average number of periods in which a loan has been offered, accepted, and repaid. Results remain the same if we use these alternative definitions.

In Sections 4.2 and 4.3, where we analyze individual lender and borrower behavior, we do not adjust $p$-values for multiple hypothesis testing. We follow this approach because the purpose of studying individual behavior is to understand the mechanisms underlying the treatment effects on our main outcome variables, rather than to examine additional hypotheses and outcomes.

Out of those relationships that break down in a given period $t$, 27% and 33% are restarted in the next period ($t + 1$) in the WE and SE treatments, respectively. Most restarts occur after a rejection in the previous period (45% and 37% of the restarts observed in the WE and SE treatments, respectively); 35% and 36% of the restarts occur after a default, while 20% and 26% of the restarts occur after no offer was made in the previous period in the WE and SE treatments, respectively.

Our statistical inference is based on comparing the outcomes of 15 matching groups in the WE treatment to 16 matching groups in the SE treatment using two-sided nonparametric (Mann–Whitney) tests. The power $(1 - \beta)$ of these tests is a function of the level of significance ($\alpha = 0.05$) and the assumed distribution of (matching-group-level) outcomes under the null and alternative hypotheses. In each matching group, there are nine relationships. We thus model the outcome of each matching group as drawn from a binomial distribution where $n = 9$ is the number of draws and $q$ is the probability that a loan is offered, accepted, and repaid in at least five periods. To estimate the power of our test, we need to specify $q$ for the null hypotheses (SE = WE) and alternative hypotheses (SE ≠ WE). Based on evidence provided by Brown and Zehnder (2007), we assume that in the SE treatment, two-thirds of the relationships feature implicit contracting so that $q_{SE} = 0.66$. The predicted probability for the WE treatment under the alternative hypothesis depends on the share of relationships that feature implicit contracting $q_I$, the share that feature screening $q_S$, and the share of social types $p$ (as only they repay in a screening equilibrium): $q_{alternative} = q_I + \ldots
contracting, relationships in the SE and WE treatments that feature repeated contracting. Brown and Serra-Garcia: 24 ships in our experiment as well.

contracting can be robust to institutional changes (e.g., competition, ment. However, existing experimental studies indicate that implicit [23] responding normal distribution.

Our test using the software PASS. When simulating, we approximate [24] separately (Spearman's rho significantly different from zero, we analyze these two variables sep-

arately). We chose this type of design because the presence of two lenders is enough to generate the effects of competition. Our focus is on the effect of weak exclusion on credit offers and repayment behavior, rather than the endogenous formation of credit relationships.

This information setting mirrors the functioning of public credit registries and private credit bureaus in many developed and developing countries (Jappelli and Pagano 2002, Miller 2003), where lenders have access to the volume of credit upon which the borrower defaulted, while not knowing the interest that was required for that loan.

We term the two lender and one borrower environment a market, following terminology used by Roth et al. (1991), among others, to describe environments in which multiple buyers (lenders, in our case) make offers to a single seller (borrower, in our case).

We chose this type of design because the presence of two lenders is enough to generate the effects of competition. Our focus is on the effect of weak exclusion on credit offers and repayment behavior, rather than the endogenous formation of credit relationships.

Given the smaller number of observations in the competition treatments (six matching groups per treatment) compared to our main bilateral trading treatments, one may worry that the lack of significance of an effect of weak exclusion is due to the low number of observations. We performed ex post power calculations to assess this concern. First, if the magnitude of the treatment effect of weak exclusion were the same as in the main treatments (12 versus 48 percentage points), we would be powered to detect a significant effect (the power would be 93% with six matching groups). Second, given the observed magnitude of the treatment effect in the competition treatments (35 versus 50 percentage points), our power level would be merely 62% even if we increase the number of observations to 15 per treatment (as in our main treatments). Given the observed magnitude of the treatment effect in the competition treatments, 27 matching groups per treatment would be needed in each treatment to obtain a power of 80% given a significance level of 0.05.

References


