The Supply and Demand Side Impacts of Credit Market Information

Alain de Janvry*, Craig McIntosh**, and Elisabeth Sadoulet*

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Abstract

We utilize a unique pair of experiments to isolate the various ways in which reductions in asymmetric information alter credit market outcomes. First, a Guatemalan microfinance lender began the use of a credit bureau without the knowledge of borrowers. One year later, we ran a large randomized credit information course which described the existence and workings of the bureau to the clients of this lender. This pairing of natural and randomized experiments allows us to separately identify how new information enters on the supply and the demand side of the market. Our results indicate that credit bureaus generate large efficiency gains for the lender, that these gains are augmented when borrowers understand the rules of the game, and that economic mobility both upwards and downwards is likely to increase.

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* Department of Agricultural and Resource Economics, U.C. Berkeley. Alain@are.berkeley.edu, Sadoulet@are.berkeley.edu

** International Relations/Pacific Studies, U.C. San Diego, 9500 Gilman Drive, La Jolla, CA, 92093-0519. ctmcintosh@ucsd.edu

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1. INTRODUCTION

Credit bureaus provide a formal institutional solution to the problem of asymmetric information in lending markets. Many developing countries feature credit markets wherein multiple lenders make unsecured loans in the absence of a formal information-sharing mechanism. As the number of lenders in these markets increases, informal ways of preventing default and multiple borrowing are likely to become inadequate, and the repercussions for economic mobility and the health of the credit market are potentially severe. This paper presents a unique confluence of data and identification to analyze how lending outcomes have responded to the introduction of a credit bureau in Guatemala's microfinance market. We use the administrative data of a large microfinance lender as well as data from the bureau itself to assemble a picture of how borrowers react, not just with a single lender but with the credit system as a whole.

We take advantage of a rare opportunity to combine a natural and a randomized experiment in order to measure this transition. The natural experiment was created by the staggered entry of the administrative branches of a microfinance lender (Genesis Empresarial) into the bureau (Crediref) over the course of 18 months. The lender conducted no publicity accompanying the event, and we found in surveys conducted after the rollout that borrower knowledge about this new bureau was almost non-existent. In the absence of borrower knowledge, the impact of the bureau arises solely as a result of reduction in adverse selection by the suppliers of credit.

Subsequent to this, we conducted a randomized experiment wherein we gave borrowers in solidarity groups and communal banks a credit training course, telling them what information is recorded in the bureau, which lenders use the bureau, and what the positive and negative repercussions of the new system are. We expect the trained borrowers to adjust to this new understanding of the market in several ways. One is an incentive effect, through which a broadening of reputation leads to a reduction of moral hazard. Second, borrowers may change the lenders from whom they demand loans when they understand that the quality of their performance is generally observable. Because group loan repayment performance in the bureau is reported at the group level, we suggest that there should be a third effect: groups should become more aggressive in their selection behavior. The implementation structure of the natural and randomized experiments allows us to observe these 'demand-side' changes in behavior entering the market after the lender-driven effects have been realized. The resulting ability to disentangle the supply- and demand-side effects of information on credit market equilibria is, to our knowledge, unique to the literature.

We find significant impacts of informational changes on both sides of the market. The strongest effect comes from information in the hands of lenders when screening new clients, particularly individuals. The bureau allows them to give more loans, to improve repayment, and to increase average loan sizes. It also causes a dramatic increase in the expulsion of existing clients. On the demand side, informing group members about the implications of a credit bureau induced better repayment performance among members of solidarity groups, both through reduction in moral hazard and improved selection by the groups themselves. We observe that inexperienced communal bank borrowers took more and bigger outside loans immediately after the training. These borrowers saw some deterioration in outside repayment performance, while good Genesis borrowers took more loans from other lenders with no adverse effects. Our results demonstrate that additional improvements are realized when borrowers clearly understand the implications of information sharing arrangements.

The paper is organized as follows: Section 2 gives background on the development of credit bureaus and describes our paired experiments in more detail. Section 3 presents a simple model of the two-sided selection process that generates the pool of individuals who receive loans. Section 4 analyzes the impact of improved information on the supply side through the staggered rollout of Crediref, and Section 5 gives the corresponding changes when demand-side information improves. Section 6 concludes on the impact of credit bureau information on borrower behavior.

2. Reduction in Asymmetric Information

The creation of a credit bureau represents a formalization of the institutions that store and share information in a credit market. Prior to this, the informal enforcement of contracts was based on the repetition of exchange, with threat of loss of benefits from future transactions if a contract is not honored. This requires the building of reputation with providers, and the sharing of information among them. North (1990) and Greif (1994) showed that this could happen within social networks where information was shared, allowing the transfer of reputation and the multilateral punishment for default, thereby enabling a "localized honesty equilibrium" in spite of the individual pursuit of self-interest. Intra-group exchange was thus cheaper than inter-group exchange as the cost of enforcement was lower. The broadening of this equilibrium to a "generalized honesty equilibrium" supporting anonymous exchange in very large groups would require the emergence of institutional innovations for the transfer of reputation and the sharing of information (Platteau, 2000). In lending markets, the standardized, digital quality signal sent by the bureau augments (or replaces) the at-home interview, the joint-liability revelation mechanism (Besley & Coate, 1995; Ghatak & Guinnane, 1999), and informal exchanges of information among credit officers.

Microfinance markets provide a good environment in which to study the impact of this formalization. Lenders on these markets use a blend of formal and informal enforcement mechanisms, and so provide a continuous margin across which these tools are interchanged (Navajas et al. 2003; Morduch, 1999; Morduch and Armendariz de Aghion, 2005). Microfinance markets have also increased rapidly in sophistication, and so offer much starker changes in information-sharing agreements than developed credit markets which have typically been sharing information for many years. Early microfinance lenders operating in geographically monopolistic contexts could reduce asymmetric information through repeating exchanges, privately held reputation (or 'relationship banking'), and dynamic incentives. Rising competition among lenders without information sharing, however, increasingly undermined the power of dynamic incentives, and disrupted this equilibrium. The response to this change, in several developing countries, has been to introduce credit bureaus which share positive information (over borrowers' current indebtedness) as well as negative (over their history of defaults).

The decision for a lender to join a bureau involves a complex set of tradeoffs (Padilla & Pagano 1997). The benefits of doing so are a decrease in portfolio risk (Campion & Valenzuela, 2001), preventing multiple contracting by borrowers (McIntosh et al, 2005), and the preservation of reputation effects during long-term lending relationships with clients (Vercammen 1995). The incentives to share information are also closely related to the level of competition; even if we do not see the kind of collapse of repayment quality predicted in Hoff & Stiglitz (1998), not only is the need to screen clients likely to increase with competition (Villas-Boas & Schmidt-Mohr, 1999), but the dispersion of information that results from a larger number of lenders makes it more difficult to do so. The interesting strategic tension arises because the advantage conferred on incumbents by a lack of information sharing can be an effective method for preventing entry (Marquez, 2001). Hence we are likely to see information sharing emerge as a strategic equilibrium only where lenders face a large pool of mobile, heterogeneous borrowers, and when the incumbents are relatively unconcerned about new entry (Pagano & Japelli, 1993). Improved information sharing should be a major source of efficiency gains for lenders (Jappelli & Pagano, 1999; Campion & Valenzuela, 2001).

On the borrowers' side moral hazard should be held in check as lenders make punishments general to the entire credit system (Vercammen, 1995). Bureaus should also help to prevent clients from taking multiple loans and hide their true indebtedness (McIntosh & Wydick, 2005). The broadening of reputation will have strongly heterogeneous effects; those with bad histories may be pushed out of the credit market altogether, while good repayers will see new opportunities to access more and better loans from other lenders. This will allow good borrowers to shop for larger and cheaper loans, moving up the credit ladder on the basis of information about their past good behavior (Galindo & Miller, 2001).

A unique feature of credit bureaus in microfinance markets is the reporting of *group* indebtedness and repayment performance in relation to loans extended to solidarity groups.[1] If an individual who is taking a loan in a group of 5 is checked in the bureau, the check will reveal the size of the group, the characteristics of the loan (loan size, term), and the repayment performance for the whole group, but not for that specific person. Clearly, this noisy measure of individual quality makes the bureau less informative about people who have taken loans only in large groups. These large groups are supposedly governed by joint-liability lending rules, but often credit officers are too well informed to punish an entire group because of a single bad individual, as the rules dictate they should. We suggest that the blunt reporting of group information into the bureau will in fact strengthen the joint liability mechanism, and hence the incentives of groups to be choosy about their membership,

[1] While this structure appears odd, it may prove the norm in microfinance credit bureaus. The reasons are both data driven (most MFIs conduct all digital recordkeeping for group loans at the group level) and legal; Peru has mandated that if a loan was issued to a group and the group repaid it in full, lenders cannot report delinquency on individuals within the group who had problems.

reducing adverse selection at the group level. Good groups should try to expel bad members and good members of bad groups should be trying to leave them.

This increased mobility is a central feature of what we expect a bureau to accomplish. Without information-sharing, the best clients of lower-tier lenders should become 'informational captives'; the lender with whom one has a personalized relationship is monopolist over the knowledge of the borrower's high quality, and hence can extract information rents. Over the long term we expect that bureaus should lower interest rates for good clients, increase upward mobility on the credit ladder, and hasten the exit of bad borrowers from the credit market altogether.

2.1. MECHANICS OF THE IMPLEMENTATION

Guatemala's microfinance credit bureau, Crediref, was formed by five of the largest members of Redimif, the national association of MFIs. The impetus was concern over a rising level of default in the client base, and agreement by the three institutions that dominate microfinance lending in the capital city (Genesis, BanCafe, and Banrural) to all enter the credit bureau.[2] Concerns that the system would be used for client cherry-picking were alleviated through several simple mechanisms. First, only institutions that share information into Crediref are allowed to consult it, with the exception of a six-month trial period during which reduced-price checks can be run by prospective entrants. Secondly, the system does not allow users to identify the lender who issued the loan. To prevent lenders from using the act of receiving credit from a high-tier lender as a client quality signal, it was made institutionally anonymous. By restricting the information observable, then, Crediref was able to overcome the strategic obstacles to the formation of a bureau. Since its inception in 2002, the bureau has continued to grow and now contains data from eight different lenders, with eleven consulting it.

Genesis extends loans to individuals and to two different types of groups: solidarity groups (SG), which number 3-5 people and feature relatively large loans; and communal banks (CB), which feature larger groups and smaller loans. The logic of borrower and group behavior is quite different in the two types of groups. Accordingly, the response to

[2] BanCafe and Banrural are both national full-service banks which only share microlending information in Crediref, and not information from their commercial banking divisions.

information about the role of a credit bureau can also be expected to be quite different. In CBs, loans are completely uncollateralized and so MFIs commonly used dynamic incentives to keep clients credit constrained and hence holding a high future valuation for the relationship with the lender. Internal control of behavior is difficult due to the large size of the group, loans are very small, group members have few other borrowing options inside Genesis, and their low asset endowments also severely limit their access to loans from other lenders. Further, the bureau is virtually never used to check ongoing CB clients. The situation is quite different for SG borrowers, who are checked in the bureau just as frequently as individual borrowers. For them, internal control is made easier by the small size of the group, and the use of collateral and cosigning is common. While SG clients have access to much larger loans, they are also likely to be more informed about and attractive to outside lenders who will offer lower rates than an MFI on these high-volume loans. As the size of SGs decreases, the incentives become more similar to those under individual lending.

Genesis has 39 branches distributed over most of Guatemala. The institution staggered the entry of its branches into Crediref over the period between March 2002 and January 2003 because a team of computer experts had to be sent branch-by-branch to set up the hardware, software, and networks required to perform ongoing credit checks and the monthly sending of data into the bureau. While this rollout was not randomized, Luoto et al (2007) perform a variety of tests on the sequencing and find that it was not endogenous to outcomes, and hence can be used for identification.

The randomized training program was conducted over the period June-November 2004. For logistical reasons, we trained only SGs and CBs and not individual borrowers. The course was administered by the Genesis in-house training staff. The design of the materials presented a challenge because nearly 50% of the Genesis clients are illiterate. We drew on experience from the training office and from the faculty of Universidad Rafael Landivar to develop materials that were primarily pictographic. We used the logos of the different lending institutions in combination with diagrams showing the flow of money and information in the lending process to illustrate when Genesis shares information on the clients and when it checks them in the bureau. The key focus of the information was to reinforce the fact that repayment performance with any one lender now has greater repercussions than previously. This point was made both in a negative fashion (meaning that repayment problems with any participating lender will decrease options with other lenders) and in a positive fashion (emphasizing the greater opportunities now available for climbing the 'credit ladder' for those who repay well).[3]

3. THEORY

We now construct a simple timeline that illustrates how the information sets held by borrowers and a lender were changed by our two experiments. We consider the information set held by the borrower, the set held by the lender over current borrowers, and the set held by the lender over potential borrowers. A borrower who has not yet taken a loan is characterized by a set of observable variables *X*, as well as a variable a observable only to the borrower, which are good predictors of the outcome $f(.)$ that will obtain if a loan is issued (this may be repayment performance, loan size, etc). The bureau reveals an additional informational signal which helps to predict a (and hence $f(.)$) and we denote the information set revealed by the bureau as α. For a borrower that has already been taking loans for *k* periods with this lender, the information set used to determine future lending also contains lagged outcomes $f_{t-1}, f_{t-2, \ldots,} f_{t-k}$, denoted by F_k.

The borrower knows her own value of a but may not be perfectly informed over what the lender knows. We denote the information that the borrower possesses over α by α^B. The information in α^B includes knowing which other lenders use the bureau, what kinds of outcomes are reported into the bureau by lenders, and under what circumstances they use it. Behavior is reported into the bureau at the level at which the loan was issued, so borrowers taking group loans learn that the loan outcome $f(.)$ is reported at the group, not the individual, level.

Given this structure, we can use the shocks to information caused by the rollout of the bureau and the credit course to divide our timeline into three distinct parts. In period (1) there is no bureau. The rollout reveals information only to the lender, and so in period (2) α is observed and we have a 'lender-only bureau'. The training reveals α^B and so in period (3)

[3] As a cautionary tale of the unpredictable consequences of training programs, Schreiner (1999) finds that the randomized Unemployment Insurance Self-Employment Demonstration actually *discouraged* the most disadvantaged from entering self-employment.

the existence of the information in the bureau is common knowledge to both lender and borrower.

Timeline of the Introduction of the Credit Bureau:

Events:		**Rollout:** Lender begins using bureau		**Training:** Borrowers learn of bureau	
	(1) No bureau	↓	**(2) Lender bureau only**	↓	**(3) Borrower & lender bureau**
Information Sets:					
Lender with potential borrowers:	$\{X\}$		$\{X, \alpha\}$		$\{X, \alpha\}$
Lender with current borrowers:	$\{X, F_k\}$		$\{X, F_k, \alpha\}$		$\{X, F_k, \alpha\}$
Borrower:	$\{X, a\}$		$\{X, a\}$		$\{X, a, \alpha^B\}$

In each period borrowers decide whether to apply for loans, the lender decides whether to extend them a loan, and for individuals who take loans the outcome $f(.)$ is realized.

The lender will offer a loan if the expected return on that loan is positive given the information set held at the time of screening. We write the latent variable indicating expected lender profits on a future loan as a function of current information as $\pi^L(.)$, and the lender offer a loan only if $\pi^L(.) > 0$. Similarly, a borrower will only choose to seek a loan if the latent variable $\pi^B(.) > 0$. For a borrower to have received a loan prior to the bureau, then, we know that both $\pi^B(X, a)$ and $\pi^L(X)$ are positive (for them to have received a (k+1)-st loan prior to the bureau then $\pi^L(X, F_k)$ must also be positive). If a borrower did not receive a loan prior to the bureau, at least one of these terms must have been negative.

This simple setup demonstrates why our pair of experiments gives us a unique ability to separate supply and demand side impacts of credit market information. In a normal setting where a bureau is introduced with full knowledge of lenders and borrowers, α and α^B would be revealed simultaneously. Hence we would switch directly from setting (1) to setting (3) in the timeline. Imagine that we observed new individuals taking loans from the lender after this transition. This could have occurred either because $\pi^B(X,a)<0$ and $\pi^B(X,a,\alpha^B)>0$ or because $\pi^L(X)<0$ and $\pi^L(X,\alpha)>0$. Since we cannot directly estimate these latent variables from data only on borrowers, we would be unable to determine whether the individuals starts taking credit because it is now demanded (α^B) or because it is now supplied (α). Individuals who had been taking credit prior to the bureau and leave when it is introduced would present a similar ambiguity between the case $\pi^B(X,a)>0$, $\pi^B(X,a,\alpha^B)<0$ and $\pi^L(X,F_k)>0$, $\pi^L(X,F_{k+1},\alpha)<0$. Because our dual experiments allow us to observe the information set α entering before α^B, however, we can cleanly ascribe causality: shifts between (1) and (2) must come from the supply side of the market, and shifts between (2) and (3) must come from the demand side.

Access to individual-level data additionally allows us to separate impacts on the extensive margin from those on the intensive margin. Luoto et al (2006) used branch-level average outcomes to study the impact of the bureau rollout, but the aggregated data used in that paper measures only the joint effect of improved borrower selection and changes in contracts for ongoing borrowers. With individual-level data we can isolate the extensive margin impacts of α and α^B on screening in and out of borrowers. The impacts on the intensive margin can be cleanly isolated for the revelation of α^B, but we illustrate that certain error structures lead to mean reversion in estimates of the intensive impacts of α.

3.1. IMPACTS FROM LENDER ROLLOUT

Screening Borrowers In

The top panel of Figure 1 shows the pdf $\phi(.)$ of expected returns to the lender π^L for individuals who had not previously taken loans. Subsequent to the revelation of α, for some subset of borrowers $\pi^L(X)<0$ but $\pi^L(X,\alpha)>0$; the lender will try to acquire those borrowers that are expected to be profitable given the information in the bureau. If there are fixed costs to acquiring new borrowers then only those for whom expected profits exceed the fixed costs will be acquired.

One implication of the efficient use of information by the lender is that there may be no correlation between the change in the selection process induced by α and the set of observable covariates X. The intuition for this is the familiar case of omitted variable bias; the lender prior to the bureau uses X to predict $f(.)$ while omitting α. The coefficients on X in a pre-bureau scoring model will therefore contain the projection of $E(f(.)|\alpha)$ into X. This means that the mean improvements in scoring achieved through observation of α would be orthogonal to X. Figure 1 illustrates that the presence of fixed costs is likely to push lenders out into the tails of the distributions of expected profits, however, and so if there is a correlation between the *variance* of α and elements of X then we may still see correlations in the data between the process of screening and characteristics that were previously observable.[4]

Screening Borrowers Out

The lender possesses a richer information set over current borrowers than potential borrowers, since the history of borrowing outcomes is also observable. The bottom panel of Figure 1 illustrates the process of expulsion of clients; some borrowers for whom $\pi^L(X, F_k) > 0$ will have $\pi^L(X, F_k, \alpha) < 0$, and so the lender will kick out borrowers in the left tail of the distribution of expected profits subsequent to the use of the bureau. Again, fixed costs in expulsion reduce the set of borrowers who are kicked out. If we believe that it is more expensive to enroll a potential borrower than it is to expel a current borrower, then we should expect participation to be more sensitive to negative information in the hands of the lender than positive information. The bureau should result in fewer expulsions of borrowers with high k because a long internal credit history allows for better approximation of a and hence the bureau contains less novel information.

Contracts for Ongoing Borrowers

Ongoing borrowers are those who were *not* screened out when the bureau entered into use, and so are those for whom $\pi^L(X, F_k, \alpha) > 0$. Thus the simplest way of thinking about the discrete changes observed in $f(.)$ in this group is that it represents re-adjustment of contracts by the lender as a result of the revelation of α. Correlation between the repayment performance between lenders will, however, undermine this simple interpretation.

[4] Also, of course, the influence exerted by groups on the screening of their own member consitutes a set of variables unobserved in this process.

Since the bureau in effect reveals borrowing outcomes from different lenders for the previous period, it may be the case that borrower-specific shocks cause a co-movement between $f(.)$ and α within a time period. Hence α and F_k are positively correlated, and if these shocks are i.i.d. then we will have the typical case of mean reversion moving from $f_{t-1}(.)$ to $f_t(.)$ because borrowers with positive shocks are disproportionately likely to be retained in the period in which the bureau is introduced.

3.2. IMPACTS FROM BORROWER TRAINING

New Borrower Entry

In the training exercise, we trained only individuals who were currently taking group loans from Genesis. The lender had already implemented the bureau and did not change its use around the training, leaving only one reason why incoming borrower composition could shift: changes in the process by which groups select new members. Borrowers do not see the information in the bureau (although a standard model of joint liability would assume that they already know it), and so these changes stem from alterations to the incentives over group composition induced by the bureau. We suggested in Section 2 the fact that, while joint liability rules ostensibly provide incentives for groups to be very careful about their composition, in reality the credit officers may hold too much information to make this threat credible: This suggests that shifts in new client enrollment from the training would arise from an increased incentive placed on the group quality signal. This might strengthen the push towards assortative matching, or it might induce groups towards greater heterogeneity in order to improve group insurance.[5]

Borrower Departure

Similarly, groups that had been willing to carry along under-performing members will wish to be more selective when they have understood the way the bureau works. Thus the pattern of expulsions from groups would indicate the characteristics of those members which the groups believed to be the weakest. However, borrowers may also wish to quit Genesis as a result of the training, because α^B informs borrowers what information is observable on

[5] This tension is illustrated by Hirshleifer (1971) who shows that asymmetric information can actually *improve* insurance if the use of improved information is to eject individuals from an insurance pool and thereby decrease the ability to spread risk among the remaining members.

them and what lenders can observe it. Thus any borrower who would have preferred to take a loan from a different lender and had only been constrained by the absence of an observable reputation will 'graduate' from Genesis when they receive the training.

Behavior Among Ongoing Borrowers

Unlike the analysis of the bureau rollout, we can cleanly isolate the intensive margin effects for the training. This is because while the bureau is only used to *check* clients at the beginning of a new loan, we trained groups mid-cycle and so are able to observe the before-after changes in borrower behavior with group composition held constant. This isolates the moral hazard impacts of the training on borrowers.

4. THE LENDER BEGINS USING THE BUREAU

Table 1 presents basic summary statistics on the loans and individual characteristics of borrowers for all three kind of loans Genesis offers. The data show that communal banks are very different from individual clients and solidarity groups. They are larger groups (on average 7.7 members taking loans vs. 4.3 in SG) composed mainly of women (99.7% vs. 50% in SG), with low education (42% uneducated vs. 24% in SG), and with much smaller loans (US$324 per client vs. $711 in SG). We begin the empirical analysis by using t-tests to draw some simple comparisons, and then proceed to a causal analysis using the staggered entry of the Genesis branches to gain identification.

A natural starting question is the difference between the kinds of loans that Genesis chooses to screen with the bureau and those they don't. We form this comparison using every client who was given a loan by Genesis between June 2000 & June 2003. Table 2a illustrates that repayment performance is better for those who were checked than for those who were not, suggesting that the treatment effect of the bureau on decreasing default is strong enough to overwhelm the selection effect which would make them more likely to check unreliable borrowers. The fixed cost of a check in Crediref starts at $1.60 (decreasing to $0.67 per check when over 6,400 checks per month are made), thus making Genesis less likely to investigate small borrowers. We see that SGs loans (where the average loan is $711) are small enough to make loan size a determinant of screening, but Individual loans (where the average is $1,164) are not.

The only opportunity we have in these data to look at the characteristics of applicants to Genesis who were *not* given loans is through a database that records every check made in Crediref. We can then compare the people who appear in this database and do not appear in the Genesis database (and hence did not receive loans) to those who subsequently appear as clients. This answers the following: among those who are screened before ever receiving a loan, what kind of person is given a loan? This is a group in which only X and not F_k is observable, and so we would expect the bureau to have the strongest effect here. In Table 2b we indeed see a night-and-day difference between these Crediref records: those rejected for loans have default rates and credit at risk which are an order of magnitude higher than those who eventually get loans.[6] So in the environment in which the lender knows least, the information in the bureau seems to play a driving role in determining who gets access to credit.

Another illuminating comparison is looking at the Crediref records of ongoing borrowers who were checked and expelled from Genesis versus those who were checked and not expelled. Table 2c draws this comparison using those who entered Genesis without having been screened. Because F_k is a part of the information set in this case, we expect the influence of α to be more muted; and indeed while the differences are strongly significant, those who are ejected are only twice as likely to have defaulted, instead of the factor of ten seen in comparisons of those who had never received loans.

4.1. Extensive margin impacts of the Bureau

We now move to utilizing the staggered entry of the branches of the bureau of Genesis into the bureau to estimate causal impacts.[7] Using loan-level data, we can measure several interesting effects that are not visible using branch-level data. Firstly, because we can observe whether each loan is issued to a new or to an ongoing borrower, we are able to disentangle the screening effects of the bureau on the extensive margin from changes in contracts on the intensive margin. Secondly, we can track the differences over time between borrowers who entered Genesis before and after the bureau was being used, and so measure

[6] Note that because we do not know what kind of loan the person would have gotten, we cannot segregate this comparison by Individual and SG loans.

[7] See Luoto et al (2007) for a series of tests that verify that the rollout is a valid source of identification.

the longer-term effects of improved information. Finally, because we also observe the credit officer who issues each loan, we can examine changes of behavior at the level of the individual who actually makes loan screening decisions.

We run a regression at the loan level to explain the difference in differences in $f(.)$ that results from the staggered treatment dummy R_{lbt} on loan *l* issued by branch *b* in month *t* (observed at the group level for group loans and at the individual level for individual loans). R_{lbt} is one for loans that could have been checked by the bureau and zero for loans given before bureau was introduced in each branch, and hence measures the Intention to Treat Effect of a branch *having* the bureau, not the effect of *using* it.[8] Our basic regression specification is then:

(1) $f_{lbt} = \delta_t + \delta_b + \beta R_{lbt} + u_{lbt}$

where δ_t and δ_b are month- and branch-specific fixed effects, and u_{lbt} is an error term clustered at the branch level.

Table 3a demonstrates the substantial changes in selection in and selection out induced by the use of the bureau. For individual loans, we see that the bureau causes a symmetric change in the percentage of all borrowers who are kicked out and who leave; both figures increase by roughly 17 percentage points. In other words, there is a period of upheaval in the client base triggered by the use of the bureau. Figure 2 shows the large increase in new individual clients that occurs for roughly six months after the bureau is implemented. For solidarity groups, the picture is somewhat more nuanced; individuals within these groups are much more likely to be expelled, but the groups themselves become more durable as a result of the bureau. The net effect of decreased enrolment by existing groups, expulsions, and the creation of new smaller groups is the dramatic decrease in average Solidarity Group size, illustrated in Figure 3. In other words, the bureau causes the lender to rely less on joint liability as a screening tool.

[8] We measure the ITE because the decision to check an individual is non-random, and so estimating the Treatment-on-the-Treated would require that we try to predict who in the branches without the bureau would have been checked. Any unobservables in this equation would create bias.

Table 3b uses the staggered entry into the bureau to measure the impact of the bureau on the characteristics of those entering & leaving Genesis. We run the regression on new and departing borrowers separately, estimating

$$(2) \quad X_{ibt} = \delta_t + \delta_b + \beta R_{ibt} + u_{ibt}$$

for individuals *i*. We put borrower characteristics such as gender, age, and education on the LHS and explain these with the ITE rollout dummy, so the coefficients should be interpreted as the effect of the bureau on the average characteristics of new and departing clients. For departing clients, we define R_{ibt} as the first time that a client could have been screened with the bureau to focus identification on those who were likely ejected rather than choosing to leave. This analysis has strong statistical results that would require more analysis to understand their origin. The bureau pushes individual lending to be more female (less dropouts), and SG lending to be more male (less new clients). It decreases the share of clients with no education and increases the share with primary education. It decreases the share of clients with secondary education among individual borrowers. On client education, the impact of the bureau is similar for individual and SG loans.

In Table 3c we use loan-level data and the specification in (1) to measure the impact of the bureau on client repayment performance on their first loan. The large cohorts screened in under the bureau take somewhat larger first loans, and see improvements in repayment performance relative to first loans issued before the bureau was in use. For loans given to individual clients, where we would expect the effects of new information to be strongest, we see a drop in the share of loans that were charged late fees, despite the fact that the average loan size to individuals increased weakly. Loans more than 2 months delinquent, which would be technically under default, are not changed. For group borrowers, on the other hand, the primary effect is an increase of more than 40% in mean loan size. So the decrease in the lenders' adverse selection causes loan sizes to go up, while repayment on these new loans improves slightly. Given that lenders' profits are made on margin, loan size increases of this magnitude with no corresponding increases in repayment problems translate into substantial earnings for the lender. Put together these results demonstrate a large increase in the supply of credit from Genesis to people that had not previously taken loans.

Table 4 shows the results of comparing performance on subsequent loans for those selected before vs. after the bureau. The bureau causes significant improvements in the ability to pick good ongoing clients, but only for people who take individual loans. Individual borrowers selected with the bureau are half again as likely as those selected before the bureau to go on to take subsequent loans: the mean probability is .45 and the increase in this probability for those selected with the bureau is .24, with a t-statistic of 8.4. These subsequent loans are taken somewhat sooner, and the size of these loans is roughly 12% larger. Group borrowers, on the other hand, display no impacts on subsequent performance. This is consistent with the joint liability mechanism providing a richer information set when group borrowers are screened.

4.1. INTENSIVE MARGIN IMPACTS OF THE BUREAU

Having seen the supply-side changes that occur on the extensive margin, Table 5 carries out the analysis on the intensive margin by including only borrowers who took loans both before and after the bureau was being used in their respective branch. Here we can include borrower-level fixed effects, and so the treatment effect measures changes in contracts for ongoing clients. Since we have limited the sample to those for whom $\pi^L(X) > 0$, $\pi^L(X, F_k) > 0$, and $\pi^L(X, F_k, \alpha) > 0$, we follow a consistent cohort through the implementation of the bureau. We estimate

$$(3) \quad f_{ilt} = \delta_t + \delta_i + \beta R_{ilt} + u_{ilt},$$

for everyone whose first loan was before the bureau and last loan was after the bureau, continuing to cluster standard errors at the branch level. This test provides a measure of the intensive margin effects, but it may be biased by mean reversion in the manner suggested in Section 3.1.

Loan sizes for ongoing clients increase when the bureau is in place, but for individual loans this is accompanied by a sharp worsening of repayment. Such a worsening is not surprising if loan sizes have increased, and it is also possible that there is a multi-tasking externality through which credit officers are occupied in screening when the bureau comes in and neglect their ongoing clients. Perhaps a more likely scenario, however, is that this (the only negative impact of the bureau found for the lender) is caused by mean reversion.

Evidence is provided by the extremely low mean default rate among these ongoing clients; 2% versus an institutional average of over 4%. Thus the 'treatment effect' measured in the second column of Table 5 essentially shifts performance among borrowers retained when the bureau comes in back to the unconditional mean in the institution. In any case, repayment performance does not improve among ongoing clients in the way that it does among new clients. This indicates that F_k does a reasonable job of proxying for the information revealed through α among those who continue to be offered loans.

One way of summarizing the joint effects of lender information on the intensive and extensive margin is to use the monthly performance of credit officers as the unit of analysis. In this way we can measure efficiency effects of the bureau as well, by examining whether a given employee is able to increase the number of new borrowers whose applications they process in a given period of time. Table 6 runs specification (3) using data and fixed effects at the credit officer/month level rather than the borrower/month level. There is doubling in the number and size of new loans issued per month by credit officers. This increase arises from increases in individual clients and group clients in similar proportions. Lending to ongoing clients, however, is not significantly impacted by the bureau. The total effect among all clients is thus an increase in the number of new loans by 1.9 on a basis of 7.17 and an increase in the portfolio growth of 20%, although not precisely measured. The growth of loans to both individuals and groups in the whole institution increased sharply as a result of the use of the bureau.

We conducted a number of additional regressions (not shown) to test whether Genesis' entry to the bureau caused changes in their clients' behavior with *other* lenders. Given that borrowers knew little about this change, we do not expect to see shifts induced by borrowers seeking out new opportunities (for this, see the next section). However, it is possible that changes in the contracts offered by Genesis would have altered demand with other lenders. The data structure for this analysis is not ideal, because Guatemalan law stipulates that the bureau can only keep a two-year window of data on borrower behavior. For this reason we could only observe outside borrowing behavior for the latter third of the branches of Genesis entering the bureau, but in no case did we find any significant impacts.

Our results indicate substantial improvements in screening performance when the bureau was introduced, with changes among ongoing borrowers more subdued. In other words, the lender learns useful information about borrowers to whom they have not given loans before, and they learn useful negative information about ongoing borrowers. However, given that they decide to continue to lend to a borrower once they have looked in the bureau, there is little or no improvement in their ability to increase loan sizes without seeing a corresponding decrease in repayment performance. For solidarity group borrowers, the bureau induces a strong swing toward smaller groups and new clients, and also appears to allow lenders to increase loan sizes without causing problems. There is a huge increase in employee efficiency at the lender, with the average credit officer moving from screening six new borrowers to ten new borrowers per month.

5. Borrowers Learn that the Lender is Using the Bureau

We now turn to the impacts of the randomized credit training course which was implemented a year after the staggered rollout was complete. The sample used in this analysis consists of the seven branches randomly selected from the 39 branches of Genesis to represent the variety of Genesis clients.[9] Within each of these seven branches, we randomly selected a predetermined number of groups for treatment, the others forming the control groups. Table 7 gives the treatment/control structure, and presents relevant statistics at the branch level for the selected branches.[10]

Once selected, groups were notified that they were eligible to receive a free information session, and they were requested by their credit officer to appear at a specific time and place in order to receive the information. Attendance was entirely voluntary, and if a group did not show up the first time, two subsequent efforts were made to call it for the session. The percentage of chosen units that were in fact treated varies from 31% to 100% across branches, with an average response rate of 62%. The lowest compliance rate came from the branch of El Castaño in Guatemala City, a part of the capital which saw a great deal of instability and drug violence during the study. The information sessions took place over a

[9] This selection was done by randomly selecting one branch in each of seven groups of similar branches constituted by credit officers with intimate knowledge of the institution. However, despite the randomization, the average characteristics of the groups from these selected branches do not perfectly match those of the non-selected branches. We therefore limit the analysis to the groups from the selected branches.

[10] The experimental structure included a second layer wherein we randomized the saturation of training within each branch with an eye to measuring the spillover effects of the training. In every specification we find insignificant spillover effects (meaning that the untrained in high-saturation branches looked similar to the untrained in low-saturation branches), and these results are not reported.

period of four months, from July to November 2004, with the order in which groups were called randomly defined. The timing of the treatment is thus specific to each treated group and we assign the median of the treatment dates within each branch to the control groups.

The quality of the randomization can be gauged from Table 8. Comparing the mean values of group-average characteristics such as age, marital status, education, gender, and ethnicity, we find no evidence of significant differences between the selected and control groups, with the exception that the trained SGs have fewer women. Looking at Table 9 on repayment performance of the 1617 loans taken between January 2003 and June 2004, the situation is less ideal. The selected groups perform better than the control groups, and the groups that actually showed up for the training even more so. Hence, the *de facto* selection of groups in the field appears to have favored good groups that were experiencing fewer repayment problems. The selection effect present in the decision to attend the information sessions is strongly positive: groups that had lower default to begin with were the ones that chose to attend.

Because we use branch & month fixed effects in all of our regressions, the relevant question in terms of bias to our estimators is whether a significant difference remains in the context of this regression. We construct false treatment effects regressions by dividing the pre-treatment time period into two equal halves, and checking for differences between 'treatment' and control groups between these two periods using group fixed effects and month dummies:[11]

$$f_{lgt} = \delta_g + \delta_t + \beta FT_{lgt} + u_{lgt} \qquad (4)$$

The observations include all loans completed between January 16, 2003 and May 16, 2004. and the "false treatment" is set to take place in the middle of the pre-treatment period, such that $FT_{lgt} = 1$ if the group g has been selected for treatment, and $t \geq$ September 16, 2003. None of the false intention to treat effects featured in the first two columns of Table 10 are significant, suggesting that there are no serious biases when we use a double difference.

Because of the relatively high non-response rate and apparent selection in compliance, our analysis again focuses on the Intention to treat Effect rather than the Treatment effect on the Treated. Assuming that the non-experimental implementation of the

[11] Standard errors on all regressions are clustered at the branch level.

training program would have a similar non-response rate, the ITE is in fact the quantity of interest for the lender.

The interpretation of the ITE is complicated by the fact that some borrowers must certainly have understood at least some part of α^B before the training. As a general matter, knowledge of the workings or indeed the existence of Crediref was very low among clients; not one of 184 clients surveyed in 2003 was aware that information was being shared between MFIs. That said, certainly some clients would have possessed better information, or at least more realistic expectations, over the process of information sharing. Such clients will appear to have a lower impact (and hence a smaller moral hazard response) simply because they learned less from the sessions. A causal impact of the treatment, then, is composite of the amount that was learned and how what was learned effects behavior.

5.1. The Intensive Margin: Discontinuous Impact within a Loan Cycle

The instantaneous impact of the information program on inside repayment isolates the moral hazard effect that arises from the desire to use reputation to leverage credit from other sources (that is, borrowers who have no interest in ever taking a loan from another lender will not respond to knowledge of the bureau). Since group composition takes time to change, there should be only the moral hazard effect present in the discontinuity, and hence in the short run our experiment represents an instrument for the value that clients place on outside credit. Over time, the repercussions of changes in group membership undertaken due to the bureau begin to have their own effects upon inside repayment, adding adverse selection to moral hazard effects.

In isolating the moral hazard effect, we are aided by the fact that within a single loan cycle group composition is fixed. The trainings occurred mid-loan, and so we have the ability to see whether a given group of people change their behavior once α^B is revealed. This analysis is conducted at the loan *payment* level, partitioning Solidarity Groups and Communal Banks. The observations are the different intermediate payments made on the loans that were active at the time of the treatment. Because repayment problems tend to come only after a certain time is elapsed, we control for where in the loan cycle the repayment takes place. A complication occurs in that loans are of different length and require various numbers of intermediate repayments. To make these repayments comparable, we therefore divide the length of each loan cycle in 10 equal intervals of time, that we refer to as deciles, and we control for the deciles rather than the rank of the repayment. We thus estimate:

$$(5) \qquad f_{plt} = \delta_l + \delta_t + \beta_d D_{plt}^d + \beta T_{plt} + u_{plt}$$

where f_{plt} is an indicator of performance for payment *p* made at time *t* on loan *l* that was active at time of treatment. The deciles dummy variable D_{plt}^d is equal to 1 if the payment belongs to decile *d*. The treatment variable, defined at the payment level, T_{plt} is set equal to 1 if the payment *p* is in loan *l* taken by a group *g* that was selected for treatment and $t > \tau_g$, the treatment date for group *g*.

We see in column 1 of Table 11 that there was no significant change in performance on intermediate payments as a result of the training. The fraction of loans which were late as of their last payment, however, is halved by the training, but only for SGs. The fraction that ultimately go into default is unchanged. While virtually all of these treatment effects show some improvement, then, the effect is muted and only in the case of last payments for SGs is it significant. This indicates that SGs, with a smaller number of members over which collective control can be exercised, may be in a better position than larger CBs in improving control over moral hazard among members. The immediate message taken away from the information session seems to have been the perils of loan delinquency, and not of missed intermediate payments.[12]

5.2. Impact across Loan Cycles

We have data on repayment behavior from Genesis for one year after the intervention. Over this intermediate time frame, we expect the moral hazard impacts to dominate although, in groups that take one or more loans after having received the information, repayment behavior is also plausibly being effected by the selection response of group members. These impacts are measured by estimating the repayment performance at

12 Although Crediref does in fact report on these intermediate payments, we encountered widespread confusion among credit officers as to how to interpret this data, and so the clients were probably correct in presuming that it was the final repayment status of the loan that mattered most.

the loan level over the long period 2002-2005. We used both OLS difference-in-difference and group fixed effects estimators:

(6) $$f_{lgt} = \delta_t + \delta S_g + \beta T_{lgt} + u_{lgt} \quad \text{(OLS)}$$

or

(7) $$f_{lgt} = \delta_t + \delta_g + \beta T_{lgt} + u_{lgt} \quad \text{(FE)}$$

where f_{lgt} is a measure of repayment performance of loan *l* of group *g* with last payment at time *t*, S_g a dummy variable indicating that the group *g* was selected for treatment, and T_{lgt} the treatment variable equal to 1 if the group *g* was selected for treatment and $t > \tau_g$, the treatment date for group *g*. We also do two TET estimations in which non-compliers are omitted.

Results are reported in Table 12. Again, despite the fact that the sign of the effects indicates an improvement in 9 out of 10 regressions, the magnitude of these effects is muted. The size of the impact of the ITE of the training on SGs is several times larger than for CBs, but only in the case of the decrease in the probability of paying high late fees is it almost significant at 95%. To the extent, then, that we find any moral hazard impact of the training on the behavior of borrowers with their inside lender (Genesis), they are confined to SGs, with no change in the repayment performance of CB.

5.3. The Extensive Margin: Impact on Group Composition

We argued in Section 3.1 that statistical profiling would tend to make observable characteristics orthogonal to the information in the bureau, but should this hold true for the group selection mechanism as well?. Groups contain a much larger information set than exist in the institutional data, and so we expect omitted variables to play a larger role in our screening estimation. More interestingly, it is standard in the literature to assume that group members hold perfect information on each other. Under this scenario 'bad' group members are retained with full knowledge of their quality, which suggests that additional reasons must have existed to retain them other than the quality metrics we observe (social capital, business ties, insurance, etc.). We would then expect a tradeoff in selection between these other attributes and 'quality' as defined by the bureau. There is no reason to expect these other attributes to be orthogonal to observable characteristics, and so no reason to expect the information in the bureau to be orthogonal to characteristics we observe.

In response to the understanding of the use of Crediref, new clients brought into groups may be subjected to increased selection. Departures from Genesis may be triggered by expulsion (where groups AS changes) or by the decision to leave (borrowers realize new outside options). Using institutional data we only see that they leave the database, and have no means to establish why they left. So analysis of 'dropout' contains an ambiguity between those ejected and the voluntary departures.[13]

We might expect the worst borrowers to be ejected, and the best to be able to take outside loans when the bureau is in use. So, before we move to the impact of the bureau on group composition it is worth asking what kinds of borrowers were repaying well before the bureau was in use. We estimate the correlations between group repayment performance and the average characteristics of groups during the pre-treatment period,

$$f_{lgt} = \delta_t + \bar{X}_{lg}\beta + u_{lgt}, \qquad (7)$$

where $\bar{X}_{lg}$ is a vector of group-average attributes for each loan. We use only SG borrowers because the group-level characteristics are not consistently entered for CB clients.

Table 13 shows that those with only a primary education are bad repayers, that middle-aged people are better than the young or the old, and that clients who conduct the day-to-day transactions on their loans through a Crediref-participating institution repay better.[14] The share of female borrowers has intriguing effects; women are more likely to have small repayment problems which result in minimal fines, but they are no more likely to be hit with substantial fines. This may be related to the fact that the large majority of these women are primary care-givers in the household, and so as businesspeople they must multitask in a manner that causes numerous small lapses. Groups with a high share of women are in fact less likely to have eventual default problems, suggesting that whatever these intermediate problem are, they are effectively overcome through the joint liability mechanism.

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[13] We assume that applicants to the groups did not 'receive' the training. If the informations set held by the applicant pool to a group is shifted by the training, then changes in new borrower composition contain a similar ambiguity between the decision to apply to a group and the decision to accept an applicant.

[14] In towns where Genesis does not have a branch office but other lenders do, they have agreements to allow payments on loans to be made through accounts in other banks.

With this basic sense of the determinants of repayment, we move to an analysis of the training's impact on group composition. We use the average characteristics of those entering and those leaving existing solidarity groups in the aftermath of the training.. New members are defined as members that join a group after the first loan of the group, and dropout members those that quit the group before the last group loan. Members that do not participate to a particular loan cycle but return to the group for a subsequent loan are counted as continuing borrowers. We used both OLS difference-in-difference and branch fixed effects estimators:

(8) $\bar{X}_{lgt} = \delta_t + \beta T_{lgt} + u_{lgt}$, (OLS)

and

(9) $\bar{X}_{lgbt} = \delta_t + \delta_b + \beta T_{lgt} + u_{lgbt}$, (FE)

with regressions weighted by the number of people represented by each group average.

Similar patterns emerge for OLS and FE estimators in Table 14: there are no sharp compositional impacts. We see a decrease in average group size of roughly one member, but this is marginally significant. This is consistent with the training causing groups to be more protective of their group reputation. Perhaps the most robust result is a decrease in the number of new members who have only a primary education, which emerged from Table 13 as a prime determinant of repayment problems.

There is some evidence here that group composition also swings to be more male as a result of the training. Given the ambiguity of the results for gender in Table 13, this is hard to interpret. It is plausible that the patterns for women repayers arise because they are in fact less reliable but have sufficient social capital that the group will consistently cover them when the loan comes due. Thus the sharpening of group incentives causes these women to be cut loose by their now more picky groups. Female groups have lower eventual default, however, and so this pattern could also arise because these successful female groups have the most improved access to outside credit as a result of the bureau, and so leave Genesis. If groups lost on average one member (relative to the counterfactual) as a result of the training, the difference between the coefficients on leaving and entering clients for women is about .45, suggesting that that lost member was half again as likely to be a woman as a man.

5.4. BEHAVIOR WITH OTHER CREDIREF LENDERS

One of the more intriguing possibilities opened up by our data is the ability to measure how Genesis' borrowers alter their behavior with *other* lenders when they understand that the bureau is in use. The placement of Genesis' client data into the bureau took place along with the staggered rollout of the use of the bureau, and hence as of the time of the training all 'inside' information had been observable to other lenders for a year or more. However, to the extent that clients did not know that the bureau existed, they possessed a form of reputational capital of which they were unaware. Treatment effects of the training thus arise from differences in the process by which borrowers *seek* outside loans, and not from a systematic shift in the *supply* of credit.

We characterize a client's outside borrowing by the number of loans taken from other lenders (the quantity is difficult to interpret for group loans). We calculate the repayment performance of each completed loan by whether there has been any late payment during its cycle. The analysis uses only Genesis clients who were members of a group at time of treatment, and their treatment status is that of the group to which they belonged.[15]

The date recorded for each loan in Crediref is the date of the last data entry, which corresponds to the closing date of the loan (except for the current loans which have their last transaction recorded in June 2005). In this analysis we consider as pre-treatment all loans completed before the treatment date. Using a DID method, we estimate the following equation:

$$(10) \qquad \Delta_{ig} = \delta + \beta S_g + u_{ig}$$

where Δ_{ig} characterizes the change in outside loans reported in Crediref from the pre-treatment to the post-treatment period of individual *i* from group *g*, δ represents the average change in outside borrowing for the members of the control groups, and S_g a dummy variable indicating that the group *g* was selected for treatment. The parameter β measures

[15] When clients belonged to two groups, they were considered treated if at least one of their groups was treated. About 3% of the control SG clients (20% of the control CB) changed group, joining a treated group after the treatment date. We also perform the analysis by attributing them the status of treated starting from the date they joined the treated group. Results are very similar and not reported here.

the ITE effect of the information sessions. We use change in number of loans and whether individual *i* started taking outside loans after the treatment date.

Results are reported in Table 15. Considering all 5419 clients together, there is no significant effect on the number of loans taken, but there is a significant 29% increase in the number of members that are reported taking an outside loan for the first time. When we look at the lower rows in this table, we see that these effects arise solely from CB clients; for SG borrowers there is no change in outside behavior. We searched for heterogeneity among these SG members, along their relationship with Genesis (contrasting old and new clients, with many or few loans) and their past performance in Genesis and with their recorded outside loans, and found no group that increased its engagement outside of Genesis.

By contrast, CB members increased the number of outside loans by 47% and increased by 31% the number of new entrants in outside borrowing. This impact starts from a lower base (18% of the SG members had records of outside borrowing prior to the treatment, while only 12% of the CB members had any) but still represents a substantial increase in credit received.

Who among the CB members responded to the training by taking on outside loans? The bottom rows of Table 15 report the contrast in ITE for good Genesis clients (never had a delinquent repayment) and bad clients (had at least a delinquent repayment) as well as for more experienced clients (had 4 or more loans with Genesis) and less experienced clients (had 3 or less loans with Genesis). Good CB clients respond to information about their public reputation by increasing the number of loans taken outside (+13%) and the number of them taking outside loans increases by 11%. By contrast, bad clients, with knowledge that their defaults in repayment is public information, are not able to increase their outside borrowing. The impact of information in inducing outside borrowing is stronger on the less experienced clients (who increase the number of loans by 12% while 15% start taking outside loans) than it is on their more experienced counterparts who do not change their outside borrowing.

Table 16 reports the impact of the information sessions on the change in repayment performance on all outside loans:

(11) $$f_{libt} = \delta_b + \delta_t + \beta T_{lit} + u_{lit}$$

where y_{libt} is a measure of performance for the loan *l* taken by individual *i* from branch *b* last recorded in Crediref at time *t*. The treatment variable T_{lit} is equal to 1 if individual *i* was member of a group selected at time of treatment and $t \geq \tau_i$, the treatment date. We also report the average pre-treatment performance $\overline{f}_0 = \sum_{lib,t<\tau_i} f_{libt}$ and the average change in performance in the control group $\overline{f}_1^C - \overline{f}_0^C = \sum_{l,ib\notin S,t\geq\tau_i} f_{libt} - \sum_{l,ib\notin S,t<\tau_i} f_{libt}$.

We note first that there has been an important decline in repayment problems even for members of the control groups, from its occurrence in 16% of the reported loans to 6% on average in the post-treatment period. The absence of overall impact of information on performance hides an interesting heterogeneity by type of borrower, notably among CB members. Experienced clients in Genesis, who did not see substantial increases in number of loans, strongly improve their repayment performance on outside loans. Those inexperienced clients who rushed for the exits after the training, on the other hand, see some deterioration in repayment. This indicates that the expansion of access to outside credit which was created by the bureau came at no cost for Genesis, but that some Genesis clients who sought outside loans as a result of the training may have seen their outside performance suffer as a result.

Two explanations suggest themselves for this heterogeneous response across SG and CB. The first is the use of the bureau itself: SG borrowers are checked frequently in the bureau, and so it represents a more effective restraint on moral hazard than for CB borrowers, who are rarely checked. Thus SG borrowers suppress the desire to rush out and take larger loans due to the fear of losing access to Genesis credit if they default, while CBs worry less about Genesis & so undertake more risk. The second explanation for this pattern relates to the extensive use of dynamic incentives in microfinance, particularly in CBs. Loan sizes begin very small and grow slowly, holding clients credit-constrained and this putting a high value on the future relationship with the lender. In this case, it is the 'inexperienced' CB borrowers who will be the most credit constrained, which explains their increased use of outside credit. While we expect the bureau to reinforce dynamic incentives with the credit system as a whole, the rise in default in this group indicates that the bureau may have a perverse effect through expanding credit access to new borrowers too fast.

We ran additional regressions (not shown) which analyzed the likelihood of Genesis 'graduating' to other lenders after the training. We defined this as being someone who had been taking only a Genesis loan moving to take an individual loan from a new lender which was larger than any loan they received from Genesis. We found evidence that the training increases the likelihood of this event for SGs, but not for CBs. The number of 'graduates' on whom these estimates were based was however too small to give much credence to the results (50 SG members after the training). There has certainly been no large-scale graduation effect whereby trained clients left *en masse* to join higher-tier lenders.

6. CONCLUSION

We utilize an unusual pair of experiments which decompose the impacts of a new credit bureau into two parts: what happens when lenders observe new information about borrowers, and what happens when borrowers become aware that lenders can observe this new information. We find that the new information in the hands of the lenders has stark impacts, leading to a large increase in the turnover of the client base, particularly in the 6 months after the introduction of the bureau to a branch. Large number of clients are ejected, and there is a dramatic increase in the number and size of loans made to new borrowers. The impact of the bureau in terms of allowing improved lending to ongoing clients that are not ejected is limited or nonexistent.

When group borrowers learn of the bureau, we see more muted impacts. Solidarity groups show some immediate improvement in repayment and are likely to shrink still further in size, taking in fewer uneducated borrowers and shifting in composition to be more male. Composition shifts both due to changes in new borrower enrollment and the ejection/departure of current members. Communal bank borrowers, who are rarely screened by Genesis, show no significant changes in behavior on inside loans. Instead, they use their knowledge of the bureau to get access to credit from outside lenders. CB borrowers who have repaid Genesis well have good records, and so the bureau increases the aggregate supply of credit to them. When they become aware of the bureau, they take more and larger loans, and this effect is particularly pronounced for 'good' and for 'inexperienced' clients The 'inexperienced' Genesis borrowers run into repayment problems on their outside loans.

In several ways the impact of this credit bureau in fact demonstrates the success of joint liability in combating asymmetric information. First, the improvement in the lender's screening ability is substantially larger for individual borrowers than for Solidarity Group borrowers. This suggests that the joint liability in SGs was a relatively efficient screening mechanism even in the absence of the bureau. Second, we see no impact of the trainings on the inside performance for CBs, which should have the strongest joint liability mechanisms in place to control moral hazard. Thirdly, we see that new CB borrowers who increase their net indebtedness run into repayment problems, indicating that the credit system in the absence of the bureau was providing as much credit as these clients could manage without default. This indicates that the 'two birds' achievements of the lender using the bureau (where loan sizes increase while repayment problems decrease) cannot be matched by the borrower. Joint liability contracts and credit bureaus appear to have a complementary effect across a broad range of loan types.

We demonstrate that bureaus are effective in improving outcomes in a credit market. Since they are a relatively low-cost intervention, this implies that they should be made a part of efforts to achieve financial deepening in developing countries. Their use appears to be almost universally to the benefit of lenders, and in a competitive market, this should lead to lower interest rates for borrowers over time. The losers from the introduction of a bureau are those borrowers who are screened out as a result of the information, and ongoing borrowers who may lose insurance opportunities as a result of the winnowing of the borrower pool. We show that group reporting can in fact reinforce the group mechanisms that underlie microfinance lending. The ultimate outcome is efficiency gains for the innovating institutions, gains for the more capable economic agents, and increased social differentiation.

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Table 1. Summary statistics on the Genesis loan portfolio six months prior to entry in the credit bureau

	Individual clients	Solidarity Groups	Communal Banks
Number of clients	7740	9318	12761
Number of groups		2163	1597
Characteristics of clients			
Percent female	49.1	50.4	99.7
Percent married	78.2	75.8	83.8
Percent with no education	10.1	24.0	41.5
Percent with some primary educ.	68.1	71.2	55.2
Percent with more than primary	20.9	4.1	2.9
Average age	42.1	40.1	39.0
Percent ethnic	29.8	47.5	-[1]
Percent rural	22.0	26.3	-[1]
Average number of loans	2.2	4.6	2.5
New clients/month	309	458	645
Dropout clients/month	159	232	474
Characteristics of groups			
New groups/month		111	73
Dropout groups/month		38	47
Number of members taking loans		4.3	7.7
Loans			
Total current loans	9492	3554	2318
New loans / month	628	588	384
Loan size per client (US$)	1164	711	324

[1] Missing information

Table 2. Summary statistics on use of the Bureau

Table 2a — **Among all borrowers from Genesis, was this individual ever checked in Crediref?**

Individuals	No	Yes	Difference
% with defaults	10.1	6.5	3.6
in Genesis	(.316)	(.301)	(.462)
Av. loan size	1080.2	1057.5	22.7
in Genesis	(12.9)	(14.7)	(19.9)
Number	8276	5371	

Solidarity Groups	No	Yes	Difference
% with defaults	19.3	11.5	7.8
in Genesis	(.383)	(.338)	(.534)
Av. loan size	509.6	659.2	-149.6
in Genesis	(4.11)	(5.98)	(7.01)
Number	8679	10644	

Table 2b — **Among those who were screened before ever receiving a loan, did they receive a loan?**

All borrowers	No	Yes	Difference
% with defaults	18.7	1.7	16.9
in bureau	(.496)	(.155)	(.510)
Av. credit in default	32.1	2.4	29.7
in bureau	(3.15)	(.703)	(3.16)
Number	6083	6362	

Table 2c — **Among those who were *not* screened when they entered but *were* screened subsequently, were they 'kicked out'?***

Individuals	No	Yes	Difference
% with defaults	20.8	36.7	-15.8
in bureau	(.869)	(2.78)	(2.57)
Av. credit in default	48.9	77.4	-28.4
in bureau	(5.35)	(20.6)	(16.4)
Number	2152	291	

Solidarity Groups	No	Yes	Difference
% with defaults	16.8	33.7	-16.8
in bureau	(.733)	(2.98)	(2.55)
Av. credit in default	32.9	105.1	-72.2
in bureau	(4.84)	(30.3)	(18.2)
Number	2562	244	

*Last payment made to Genesis within 2 months of being checked
(Standard errors in parentheses)
Estimated using those who started borrowing from Genesis and/or were checked in Crediref by Genesis between June 2000 & June 2003.

Table 3a. Impact of bureau on screening borrowers in and out

	Individual Borrowers	Borrowers within ongoing Solidarity Groups	Entire Solidarity Groups
Leaving	Fraction leaving	Fraction leaving	Fraction leaving
First time screened	0.170	0.134	-0.396
	(8.74)**	(7.49)**	(6.36)**
Subsequent screenings	-0.066	-0.060	-0.158
	(1.75)	(4.50)**	(2.33)*
Observations	31,350	42,532	19,101
R-squared	0.17	0.03	0.17
Number of branches	36	35	35
Mean of dependent variable	0.51	0.08	0.30
Entering	Fraction entering	Fraction entering	Fraction entering
ITE	0.165	-0.013	0.249
	(5.08)**	(1.86)	(5.12)**
Observations	31,350	43,120	19,101
R-squared	0.04	0.01	0.07
Number of branches	36	35	35
Mean of dependent variable	0.42	0.04	0.28

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%
All regressions run with branch & month fixed effects, robust standard errors clustered at branch

Table 3b. Impact of screening on client composition

	Individual borrowers		Solidarity group clients	
	Entering ITE	Leaving ITE	Entering ITE	Leaving ITE
Female	-0.033	-0.059	-0.129	0.026
	(1.52)	(4.85)**	(4.22)**	(0.81)
Married	-0.009	-0.019	-0.061	-0.001
	(0.47)	(1.76)	(2.62)*	(0.04)
No education	-0.045	-0.027	-0.111	-0.035
	(3.82)**	(1.64)	(5.40)**	(1.43)
Primary education	0.155	-0.020	0.147	0.036
	(7.33)**	(1.02)	(6.79)**	(1.50)
Some secondary education	-0.110	0.042	-0.032	0.039
	(4.89)**	(3.58)**	(3.61)**	(0.09)
Age	0.425	0.072	0.867	0.036
	(0.90)	(0.31)	(1.73)	(0.44)
Ethnic	0.012	0.027		
	(0.30)	(0.70)		
Observations	13017	16104		

Absolute value of t-statistics in parentheses, standard errors clustered at the branch level in brackets

Table 3c. Extensive margin of staggered rollout: Performance of first loans

	Individual Borrowers	Borrowers within ongoing Solidarity Groups	Entire Solidarity Groups
Leaving	Fraction leaving	Fraction leaving	Fraction leaving
First time screened	0.170	0.134	-0.396
	(8.74)**	(7.49)**	(6.36)**
Subsequent screenings	-0.066	-0.060	-0.158
	(1.75)	(4.50)**	(2.33)*
Observations	31,350	42,532	19,101
R-squared	0.17	0.03	0.17
Number of branches	36	35	35
Entering	Fraction entering	Fraction entering	Fraction entering
ITE	0.165	-0.013	0.249
	(5.08)**	(1.86)	(5.12)**
Observations	31,350	43,120	19,101
R-squared	0.04	0.01	0.07
Number of branches	36	35	35

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%
All regressions run with branch & month fixed effects, robust standard errors clustered at branch level.

Table 4. Impact of the bureau on the future behavior of newly selected borrowers

	Probability of taking subsequent loan	Months until subsequent loan taken	Growth in size of subsequent individual loan
Individual loans			
Treatment effect	0.240	-0.127	0.122
	(8.40)**	(1.97)	(2.35)*
Observations	13040	5815	5814
R-squared	0.11	0.04	0.05
Mean of dependent variable	0.45	1.98	1.37
Number of branches	36	36	36
Solidarity Group loans			
Treatment effect	0.069	0.029	0.039
	(1.14)	(0.18)	(0.54)
Observations	5443	2881	2881
R-squared	0.14	0.04	0.07
Mean of dependent variable	0.53	1.6	1.35
Number of branches	35	35	35

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%
All regressions run with branch & month fixed effects, robust standard errors clustered at branch level.

Table 5. Intensive margin: Performance of ongoing borrowers

	Borrower pays late fees > 1% of principal	Loan more than 2 months delinquent	Average loan size per borrower
Individual loans			
Treatment Effect	0.036	0.023	640
	(2.29)*	(3.74)**	(2.15)*
Observations	11203	11203	11203
R-squared	0.04	0.02	0.19
Number of borrower	3256	3256	3256
Mean of dep. variable	0.120	0.020	8220
Solidarity Group loans			
Treatment Effect	-0.009	0.009	1618
	(0.67)	(1.39)	(6.60)**
Observations	8796	8796	8796
R-squared	0.03	0.02	0.31
Number of borrower	1149	1149	1149
Mean of dep. variable	0.040	0.020	6775

Absolute value of t statistics in parentheses

* significant at 5%; ** significant at 1%

All regressions run with individual and month fixed effects, dummies for loan cycle, robust standard errors clustered at branch level.

Table 6. Impact of the bureau on the efficiency of credit officers

			Individual loans		Group loans	
	Number of new loans	Total new lending (US$)	Number of borrowers	Average loan size (US$)	Number of borrowers	Average loan size per capita (US$)
New borrowers						
Treatment effect	2.498	2,919	0.991	215	2.843	259
	(8.53)**	(6.92)**	(5.08)**	(4.55)**	(4.02)**	(11.42)**
R-squared	0.11	0.07	0.09	0.04	0.02	0.07
Mean of dep. variable	2.65	3,178	1.56	533	5.51	182
Pre-existing borrowers						
Treatment effect	-0.571	-576				
	(1.32)	(0.52)				
R-squared	0.09	0.06				
Mean of dep. variable	4.52	8,162				
All borrowers						
Treatment effect	1.927	2,343				
	(3.31)**	(1.80)				
R-squared	0.10	0.07				
Mean of dep. variable	7.17	11,340				

Absolute value of t statistics in parentheses

* significant at 5%; ** significant at 1%

All regressions include credit officer and time fixed effects, robust standard errors clustered at branch level.

Table 7. Implementation of the randomization

Branch Name	Number of Solidarity Groups	Number of Communal Banks	% selected for treatment	% actually treated	% of treated in selected
Chimaltenango	141	209	25	18	71
Cuilapa	104	28	49	27	54
Santa Lucia	122	37	71	41	58
Salama	175	128	60	37	62
Poptun	95	69	38	38	99
El Estor	22	0	82	50	61
El Castano	77	0	93	29	31

Table 8. Comparison of pre-treatment covariates

	Solidarity Groups			**Communal Banks**		
Group characteristic:	Control groups mean	Selected groups mean	Selected - control difference	Control groups mean	Selected groups mean	Selected - control difference
Loan amount per capita	886	854	-32	297	296	-0.1
(in US$)	[87]	[67]	(0.47)	[8]	[5]	(0.02)
Percent divorced	1.61	1.46	-0.15	1.00	0.48	-0.52
	[0.57]	[0.43]	(0.20)	[0.27]	[0.28]	(1.04)
Percent widowed	4.10	4.51	0.41	3.69	4.15	0.46
	[0.90]	[1.29]	(0.24)	[1.63]	[1.70]	(0.96)
Percent with no education	3.34	4.38	1.04	33.95	35.88	1.93
	[1.62]	[0.98]	(0.99)	[2.77]	[3.46]	(0.81)
Percent with some primary edu	89.51	90.41	0.90	61.85	60.50	-1.35
	[3.39]	[2.05]	(0.57)	[3.01]	[3.26]	(0.76)
Percent with more than primary	6.80	5.02	-1.79	4.11	3.44	-0.67
	[2.96]	[1.37]	(1.03)	[0.77]	[0.40]	(0.67)
Percent female	55.27	50.50	-4.76	100.00	99.94	0.06
	[8.18]	[4.94]	(1.01)	[0.00]	[0.06]	(0.97)
Average age	38.84	37.69	-1.15	36.04	35.61	-0.42
	[1.32]	[0.88]	(1.26)	[0.58]	[0.33]	(1.29)
Number of observations	354	680		299	285	

Standard errors in brackets, absolute value of t-statistics in parentheses; t-tests compare mean values of selected groups and control groups.

Table 9. Comparison of pre-treatment outcomes

Outcome	Control groups mean	Selected groups mean	Selected - control difference	Treated groups mean	Treated - control difference
Borrowers pays late fees > 1% of princip	7.08	4.27	-2.81	1.61	-5.47
	[1.39]	[0.80]	(1.77)	[0.41]	(3.91)
Loan more than 2 months delinquent	3.96	3.05	-0.92	0.57	-3.39
	[1.54]	[0.75]	(0.47)	[0.32]	(2.27)
Observations	653	964		670	

Standard errors in brackets; absolute value of t statistics in parentheses; t-tests compare group outcomes for selected or treated groups to outcomes for control groups.
All outcomes in percent.

Table 10. Counterfactual tests

	False Treatment Effects (Two pre-treatment periods)		Selection Effects
Outcome	SGs	CBs	(Non-compliers versus control groups)
Borrowers pays late fees > 1% of princip	-5.21	-0.41	1.50
	(1.45)	(0.08)	(0.29)
Loan more than 2 months delinquent	-0.67	-3.23	-5.89
	(0.34)	(1.48)	(1.76)
Observations	819	368	1490

Absolute value of t-statistics in parentheses;
Analysis conducted at the loan level, with group and time fixed effects.
All coefficients multiplied by 100.

Table 11. Discontinuous impacts of information within a loan cycle

Outcome:	**Intermediate payments** ITE	**Final payment only** Mean value Control	Mean value Selected	Difference ITE	**Default** Difference ITE
Solidarity Groups:					
Delinquent payment[1]	-5.51	20.62	10.41	-10.22	-6.87
	(1.76)	[4.97]	[2.22]	(2.30)	(0.79)
Amount of late fees (US$)	0.42	1.57	1.22	-0.35	-0.09
	(1.46)	[0.68]	[0.72]	(0.31)	(1.12)
Number of observations	4015	138	258		
Communal Banks:					
Delinquent payment[1]	0.88	9.27	7.62	-1.65	-3.09
	(1.76)	[3.27]	[1.22]	(0.44)	(1.30)
Amount of late fees (US$)	-0.15	1.23	0.75	-0.48	-0.03
	(1.25)	[0.87]	[0.25]	(0.50)	(1.29)
Number of observations	3420	176	163		

Absolute value of t-statistics in parentheses, standard errors clustered at the branch level in brackets;
Analysis conducted at the loan payment level, using loans that were active at the time of the treatment.
Regressions on intermediate payment performance include loan and time fixed effects, and dummy variables for
[1] Coefficients multiplied by 100.

Table 12. Impact of information across loan cycles

	OLS DID		Group fixed effects			
	All	All	All	All	SG	CB
Outcome	ITE	TET	ITE	TET	ITE	ITE
Borrowers pays late fees > 1% of princip	-3.68	-0.80	-7.18	0.75	-9.86	-2.54
	(1.53)	(0.28)	(1.58)	(0.19)	(1.86)	(0.49)
Loan more than 2 months delinquent	-2.83	-1.98	-5.68	-0.39	-7.50	-1.12
	(1.08)	(1.00)	(1.49)	(0.20)	(1.15)	(0.28)
Observations	2582	2582	2582	2582	1670	912

Absolute value of t-statistics in parentheses, standard errors clustered at the branch level.
Analysis conducted at the loan level, with time fixed effects.
[1] Coefficients multiplied by 100.

Table 13. Correlations between group repayment and individual characteristics in Solidarity Groups

	Borrower pays late fees	Borrower pays late fees > 1% of principal	Loan more than 2 months delinquent
Number of members	-1.72	3.20	1.71
	(0.48)	(1.26)	(1.01)
Divorced ratio	-8.91	-4.66	-4.39
	(1.04)	(0.83)	(1.83)
Widowed ratio	-16.06	-9.16	-5.99
	(1.54)	(3.77)**	(2.13)
With some primary education ratio	2.03	21.69	11.73
	(0.25)	(4.13)**	(3.44)*
With some secondary education ratio	15.55	21.77	7.42
	(1.33)	(1.83)	(1.49)
Female ratio	14.20	-0.35	-4.00
	(2.68)*	(0.19)	(3.87)**
Average age	-3.74	-1.65	-1.23
	(4.15)**	(8.29)**	(2.14)
Average age squared	0.05	0.02	0.02
	(3.79)**	(10.88)**	(2.88)*
Banking w/ Crediref institution ratio	-9.29	-1.52	-2.68
	(3.14)*	(1.28)	(2.04)
Observations	730	730	730

Analysis conducted at the loan level. Absolute value of t-statistics in parentheses.
All coefficients are multiplied by 100.

Table 14. Compositional impacts of information

	OLS DID		Branch-level fixed effects	
	New clients ITE	Dropouts ITE	New clients ITE	Dropouts ITE
Number of members	-0.13	0.93	-0.079	1.242
	(0.79)	(1.44)	(0.73)	(1.93)
Divorced ratio	-0.03	0.04	-0.006	0.028
	(1.40)	(1.69)	(1.07)	(1.27)
Widowed ratio	-0.04	-0.12	0.015	-0.13
	(1.18)	(0.93)	(0.89)	(1.38)
No education ratio	0.03	0.07	0.046	0.082
	(0.56)	(1.00)	(0.85)	(1.15)
Primary educ ratio	-0.14	-0.03	-0.155	-0.105
	(2.09)	(0.35)	(2.02)	(1.45)
Some secondary educ ratio	-0.03	-0.04	-0.006	0.023
	(1.65)	(1.11)	(1.02)	(1.73)
Female ratio	-0.46	0.04	-0.252	0.167
	(2.74)	(0.46)	(1.45)	(2.05)
Average age	-8.13	-4.50	-7.096	-1.49
	(1.42)	(1.44)	(1.88)	(0.84)
Observations (all groups / groups with new clients or dropouts)	1738 / 143	1738 / 216	1738 / 143	1738 / 216

Absolute value of t-statistics in parentheses, standard errors clustered at the branch level

Table 15. Impact of information on change in outside borrowing

	Number of clients	Change in number of loans	Start taking outside loan
All clients			
Control groups		0.261	0.361
		(1.65)	(6.37)
ITE	5419	0.071	0.104
		(1.47)	(2.43)
By type of clients			
Solidarity Group member			
Control groups		0.368	0.366
		(7.05)	(11.52)
ITE	1247	-0.049	0.085
		(0.31)	(1.02)
Communal Bank member			
Control groups		0.229	0.360
		(1.15)	(5.03)
ITE	4172	0.106	0.109
		(2.09)	(3.05)
Heterogeneity among Communal Bank members			
Less experienced clients - ITE	2717	0.117	0.154
		(2.58)	(4.02)
More experienced clients - ITE	1455	0.138	0.023
		(0.95)	(0.81)
Good client - ITE	3572	0.127	0.112
		(2.21)	(4.59)
Bad client - ITE	600	0.037	0.130
		(0.37)	(1.55)

Analysis at the client level; OLS of change in outside borrowing, with standard errors clustered at the branch level. Experienced (less experienced) clients are clients having had at least 4 (less than 4) loans with Genesis. Good (bad) clients had no (at least one) delinquent repayment before.
Absolute value of t-statistics in parentheses.

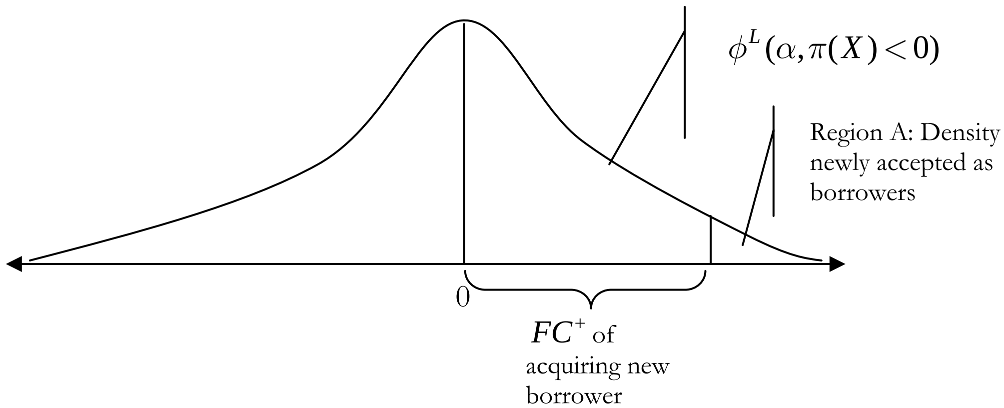
Table 16. Impact of information on the performance of outside loans

	Number of loans	Ever missed a payment: Pre-treatment average	Pre-post change in control groups	ITE
All	4811	0.161	-0.100	0.002
				(0.11)
By type of clients				
Solidarity Group members	1314	0.190	-0.120	-0.063
				(1.27)
Communal Bank members	3497	0.150	-0.092	0.021
				(1.77)
Heterogeneity among Communal Bank members				
Experienced clients (4 or more loans inside)	1658	0.152	-0.074	-0.020
				(2.81)
Less experienced clients (3 or less loans inside)	1839	0.148	-0.103	0.043
				(1.79)

Analysis at the loan level, with time and branch fixed effects and standard errors clustered at the branch level.
Absolute value of t-statistics in parentheses

Figure 1. Densities of expected profitability when bureau is used.

Density among Current Non-borrowers



Density among Current Borrowers

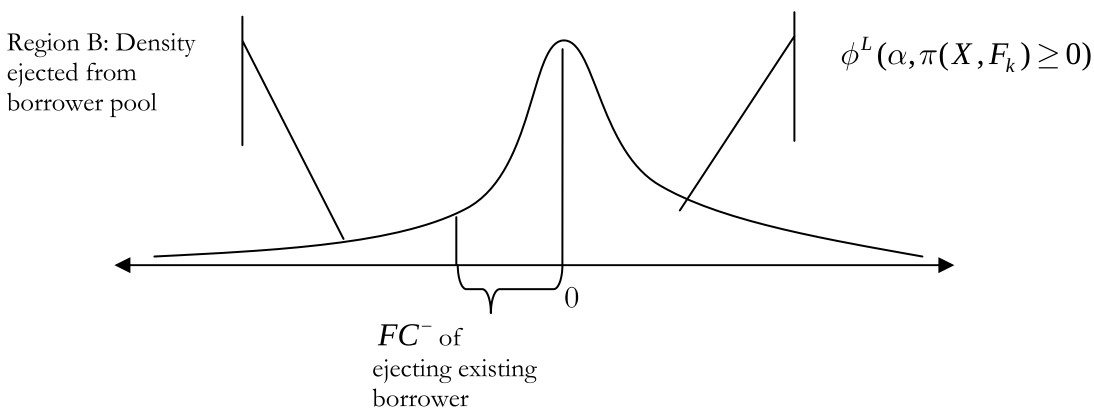


Figure 2. The increase in new individual loans when the bureau is used.

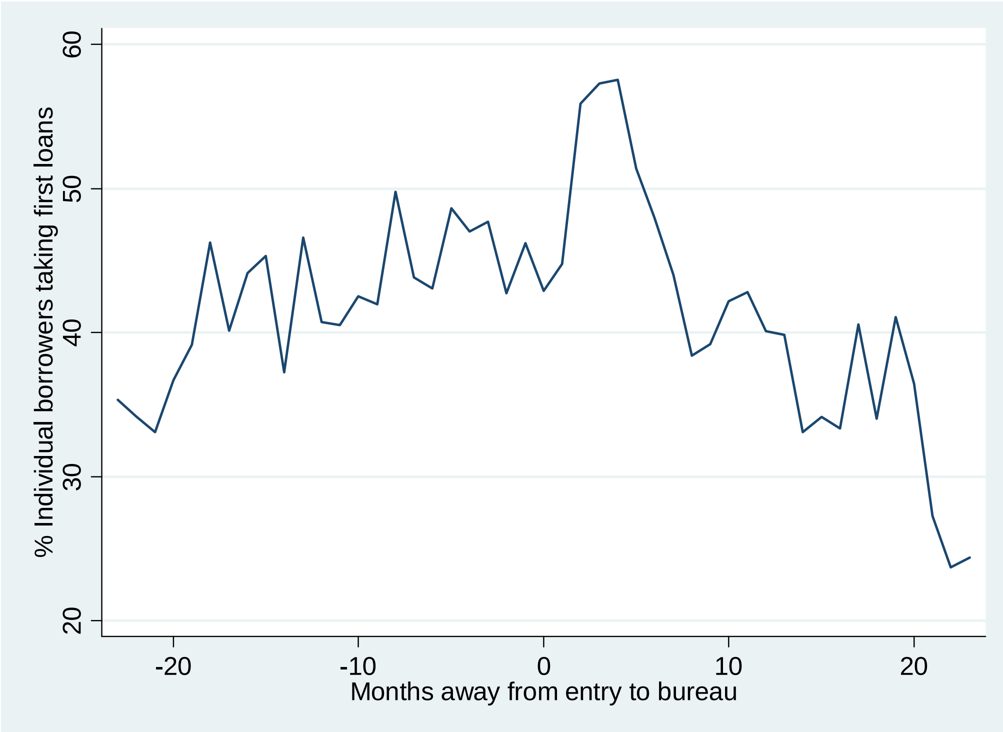


Figure 3. The decrease in the size of solidarity groups when the bureau is used.