Show Me Yours and I'll Show You Mine: Sharing Borrower Information in a Competitive Credit Market*

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Abstract

We exploit contract-level data on approved and rejected small-business loans to assess the impact of a new credit registry in Bosnia and Herzegovina. Our findings are threefold. First, mandatory information sharing tightens lending at the extensive margin as loan officers reject more applications. These rejections are based increasingly on hard information—especially registry information on applicants' outstanding debt—and less on soft information. Second, lending standards also tighten at the intensive margin: information sharing leads to smaller, shorter and more expensive loans for first-time borrowers. Yet, in line with lower switching costs, repeat borrowers gain from information sharing. Third, the tightening of lending standards results in fewer defaults, in particular among first-time borrowers, and higher returns on loans. This suggests that a reduction in adverse selection is an important channel through which information sharing affects loan quality.

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1. Introduction

Agency problems in banking remain rife, especially in emerging markets where information asymmetries tend to be high, screening and monitoring costly, and creditor rights weak. Various countries have recently tried to improve the functioning of credit markets by introducing public credit registries that require lenders to share borrower information. So far the empirical evidence on the effectiveness of such registries—in terms of access to credit and loan quality—remains scarce and is mainly based on cross-country comparisons.

This paper presents more direct evidence of what happens when a new credit registry obliges lenders to start sharing borrower information. Evaluating the impact of such a regime change is challenging for at least two reasons. First, borrower information is typically only publicly available *after* a registry is introduced. Second, even if pre-registry data exist it remains difficult to identify the impact of information sharing if all lenders and borrowers are similarly affected by the new regime.

To surmount these challenges, we use unique contract-level data on the complete loan portfolio of a large lender in Bosnia and Herzegovina. Two features make our data particularly well suited to study the question at hand. First, we can exploit detailed information on the terms—amount, maturity, interest rate, collateral and performance—of all small-business loans that this lender granted through its branch network. Importantly, we have data from before and after the introduction of the credit registry and hence observe lending decisions by the same loan officers under different information-sharing regimes. Second, we also have information on all loan applications that this lender rejected and we know *why* they were rejected. We again have these data for the period before and after the introduction of the registry.

Credit market competition varies significantly across Bosnia and Herzegovina. We capture this variation both through an objective competition measure and a subjective one based on loan-officer perceptions. Based on existing theoretical work, we expect the impact of the credit registry to be stronger in geographical areas with more intense credit market competition. We therefore use a difference-in-differences framework that combines time variation in mandatory information sharing with geographical variation in competition to identify impacts on rejection rates, lending conditions and loan quality. Likewise, we also exploit borrower variation in lending history.

We find that information sharing tightens lending at the extensive margin as more applications are rejected, in particular in competitive areas. These rejections are based increasingly on *hard* information—especially positive borrower information from the new registry. In contrast, the probability that a loan gets rejected due to soft information declines. Using loan-officer fixed effects, we then show that lending standards also tighten at the intensive margin: first-time borrowers receive smaller, shorter and more expensive loans that require more collateral. Interestingly, with the registry in place, repeat borrowers can now signal their quality to competing lenders. This forces the incumbent lender to offer better terms. Using borrower fixed effects, we show that—in line with a decline in switching costs—repeat borrowers receive progressively larger, longer and cheaper loans once the registry is in place. Lastly, the tightening of lending standards also results in higher loan quality, in particular in high-competition areas and for first-time borrowers. This suggests that a reduction in adverse selection is an important channel through which information sharing affects loan quality.

Various robustness and placebo tests confirm that our results reflect the introduction of the credit registry—and the associated improvement in available borrower information—rather than different economic conditions across branches, secular trends, the impact of the global financial crisis, or model-specification choices.

Our paper contributes to the nascent literature on mandatory information sharing. This literature builds on theories that explore how asymmetric information in credit markets—the fact that borrower information is private rather than public—causes lenders to provide either too little or too much credit. The seminal contribution by Stiglitz and Weiss (1981) shows that lenders ration credit when they fear that a market-clearing interest rate will attract riskier borrowers. Some entrepreneurs with ex ante profitable projects are consequently denied credit. Making borrower information public may reduce such rationing and increase lending.

In contrast, de Meza and Webb (1987) and de Meza (2002) show that when information about the ability of entrepreneurs is private, too many individuals apply for a loan and some negative NPV projects receive credit. If entrepreneurial ability would instead be publicly observable, then lenders could better tailor their interest rates, marginal entrepreneurs would no longer apply for credit, and overall lending would decline.

Against the background of this earlier literature, various theoretical contributions explore how information sharing can reduce moral hazard, adverse selection and over-borrowing. First, moral hazard may decline as borrowers no longer fear that their bank will extract rents by exploiting proprietary information (Padilla and Pagano, 1997). Hold-up problems due to informational lock-in (Sharpe, 1990; Rajan, 1992; von Thadden, 2004) diminish in particular for repeat borrowers. With a registry in place, defaulting borrowers lose their reputation in the whole credit market and not just with their current lender (Hoff and Stiglitz, 1997). This further reduces moral hazard, in particular if banks only exchange negative information (Padilla and Pagano, 2000). Theory suggests that both mechanisms increase borrower discipline, improve loan quality and lead to more lending at lower interest rates.

Second, the availability of centralized credit data can reduce adverse selection and bring safe borrowers back into the market (Pagano and Jappelli, 1993). While such improved screening boosts loan quality, the effect on the quantity of lending is ambiguous as more lending to safe borrowers may be offset by less lending to riskier clients.

Third, a credit registry can also prevent borrowers from taking loans from multiple banks ("double dipping") instead of applying for one single loan.[1] When borrowers can hide outstanding debt, each loan will be under-priced as new lenders ignore that their loan increases the default risk of existing debt. Sharing (positive) information about other loans rules out such negative externalities and makes lenders more careful.[2] This may lead to fewer, smaller and more expensive loans with a better repayment record.

To sum up, the extant theoretical literature predicts an unambiguous positive effect of information sharing on loan quality while the impact on the quantity of lending is less clear-cut. Models that stress initial over-indebtedness predict a decline in lending, theories that focus on moral hazard suggest that lending increases and the effect of reduced adverse selection remains theoretically ambiguous.

Importantly, all these contributions suggest a stronger impact of information sharing in more competitive credit markets. When competition is high, moral hazard may be more salient because defaulting borrowers can easily move to another lender. Lender competition can also exacerbate adverse selection as investments in information acquisition fall (Hauswald and Marquez, 2006) and banks reallocate credit to captured borrowers of lower quality (Dell'Ariccia and Marquez, 2004). Over-borrowing is more likely to occur in high-competition markets too (Parlour and Rajan, 2001). For these reasons, the introduction of mandatory information sharing can be expected to "bite" more in competitive credit markets.[3]

[1] See Hoff and Stiglitz (1997), McIntosh and Wydick (2005) and Bennardo, Pagano and Piccolo (2015) for theory and McIntosh, de Janvry and Sadoulet (2005) for evidence from a Ugandan microfinance institution.

[2] Degryse, Ioannidou and von Schedvin (2012) use data from a Swedish bank to show that when a previously exclusive firm obtains a loan from another bank, the initial bank decreases its internal limit, suggesting that information sharing allows lenders to condition their terms on loans from others.

[3] This is also because *voluntary* information sharing is unlikely to emerge in competitive markets. See Pagano and Jappelli (1993) and Bouckaert and Degryse (2006) for theory.

On the empirical side, cross-country evidence suggests that information sharing is associated with less risk taking by banks (Houston et al., 2010; Büyükkarabacak and Valev, 2012); more lending to the private sector, fewer defaults and lower interest rates (Jappelli and Pagano, 1993; 2002); and less tax evasion as accounting fraud is easier to detect (Beck, Lin and Ma, 2014). These effects appear to be stronger in developing countries (Djankov, McLiesh and Shleifer, 2007) and for opaque firms (Brown, Jappelli and Pagano, 2009). However, most cross-country studies only imperfectly control for confounding factors that might lead to a spurious correlation between information sharing and credit outcomes. These studies typically also remain silent about the mechanisms through which information sharing affects credit markets. Moreover, some recent anecdotal evidence appears at odds with the positive impacts suggested by cross-country analysis. For instance, while a new credit registry in the United Arab Emirates was widely expected to increase banks' appetite to lend, its introduction instead coincided with a sharp increase in loan rejections.[4]

A small literature exploits contract-level information to more cleanly identify the impact of information sharing. These papers typically study changes in the coverage (borrowers) or participation (lenders) of existing credit registries. Luoto, McIntosh and Wydick (2007) and de Janvry, McIntosh and Sadoulet (2010) analyze the staggered use of a registry by the branches of a Guatemalan microfinance institution. They find an increase in loan performance, especially for borrowers that are aware of the existence of the registry. Doblas-Madrid and Minetti (2013) focus on the staggered entry of lenders into a credit registry for the US equipment-financing industry. Entry improved repayment for opaque firms but reduced loan size. In a similar vein, Hertzberg, Liberti and Paravisini (2011) show how lowering the reporting threshold of the Argentinian credit registry resulted in less lending to firms with multiple lending relationships. Liberti, Seru and Vig (2015) find that this registry expansion also led to more delegation of tasks to loan officers at one of the participating banks. Borrowers that were revealed to be of low quality, experienced a reduction in loan size and a worsening of lending terms. Lastly, González-Uribe and Osorio (2014) explore the impact of *erasing* negative borrower information from a Columbian credit bureau. Wiping out information allowed borrowers to attract larger and longer loans from new lenders. The quality of these loans was significantly below that of similar borrowers whose credit history had not been reset. In a similar vein, Ioannidou and Ongena (2010) find that Bolivian firms

[4] See http://www.thenational.ae/business/banking/adib-consumer-loan-rejections-soar-10-after-bank-adopts-credit-bureau-data.

switch banks once information about prior defaults is erased and the incumbent lender no longer holds them up.[5]

We contribute to this recent literature in four ways. First, to the best of our knowledge, this paper is the first to study the impact of a new credit registry on the basis of contract-level data from before and after the introduction. Earlier loan-level studies instead focus on existing registries that expanded their coverage by lowering participation thresholds for borrowers or by including new lenders in a staggered fashion. Second, we have access to unique data on *why* individual loan applications are rejected. This allows us to observe directly to what extent lenders use negative and positive borrower information once this information becomes publicly available. Third, we exploit within-borrower and within-loan officer variation under different information-sharing regimes. This helps us identify some of the mechanisms through which information sharing affects access to credit (something which is difficult to do in cross-country studies). Fourth, we use information on local variation in lender competition to test whether mandatory information sharing is particularly effective in more competitive credit markets.

We proceed as follows. Section 2 provides background on our empirical setting, after which Sections 3 and 4 describe our data and identification strategy, respectively. Section 5 then presents our empirical results and Section 6 concludes.

2. Empirical setting

2.1. Bosnia and Herzegovina

Bosnia and Herzegovina is a middle-income country with a relatively entrepreneurial middle class (Demirgüç-Kunt, Klapper and Panos, 2010). In 2009, 17 banks and 12 microfinance institutions were lending to Bosnian small businesses. This competitive financial sector helped domestic credit expand from 23.4 percent of GDP in 2001 to 67.7 percent of GDP in 2013.[6] Some entrepreneurs even took out several loans at the same time, often collateralized through personal guarantees by friends or family members (Maurer and Pytkowska, 2011).

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[5] Elul and Gottardi (2015) derive theoretical conditions under which erasing negative borrower information (‘forgetting’) may improve welfare as the resulting strengthening of borrowers’ ex post incentives outweighs the weakening of their ex ante incentives.

[6] Source: World Bank (http://data.worldbank.org/country/bosnia-and-herzegovina).

While a private data-collection agency had been active in Bosnia and Herzegovina since 2000, most banks and microfinance institutions neither used it nor contributed information to it. Participation was voluntary and expensive and coverage therefore incomplete. As a result, lenders had no way to check whether loan applicants had already borrowed from one or more competitors. As one manager of a large Bosnian financial institution succinctly put it: "*Before the introduction of the credit registry, we were basically blind*." Loan officers of competing lenders sometimes even disseminated false information about their borrowers. Coordination failures thus prevented the emergence of voluntary information sharing among lenders.[7]

In response to this institutional deficiency, the Bosnian central bank started to set up a public credit registry (Centralni Registrar Kredita, CRK) in 2006. Yet, it was only in July 2009 that participation became mandatory for all lenders, including microfinance institutions. This is also the month in which EKI, the lender whose loan portfolio we analyze, started to provide information to the registry and began to use it. Interviews with loan officers suggest that the July 2009 registry introduction marked a sudden improvement in the available information about loan applicants. No other financial regulation was introduced in 2009.

The Bosnian credit registry requires lenders to submit a report for each loan to a firm or private individual that is disbursed, repaid in full, late or written off. The registry contains both negative information on past loan defaults and positive information on any other loans that a loan applicant has outstanding. The registry also includes information on whether applicants have a guarantor or are a guarantor themselves. Each loan applicant receives a credit score that reflects current debt as well as past repayment performance and that is calculated on the basis of uniform regulatory guidelines for credit-risk assessment. The central bank checks whether reporting follows the appropriate formatting and undertakes random checks on data validity. Registry information is therefore regarded as both comprehensive and dependable.[8]

Lenders are required to include a clause in each loan contract in which the borrower agrees to a credit check via the registry. Borrowers are therefore aware that their repayment performance will be recorded and shared with other lenders.

[7] *Ibid.* footnote 3.

[8] We cannot rule out that some information manipulation may occur. For instance, Giannetti, Liberti and Sturgess (2015) find evidence for credit-rating manipulation in the Argentinean credit registry. We point out, however, that while submitting information to the Bosnian registry is mandatory, checking the data is voluntary and subject to a small fee. Our data show that the registry is actively used, suggesting that lenders attach value to the newly-available information. The registry receives on average over 240,000 requests a month.

2.2. The lender

We use data from EKI, one of the main providers of individual-liability small-business loans in Bosnia. Founded in 1996, EKI currently lends through a network of 15 branches across both parts of the country (the Republika Srpska and the Federation of Bosnia and Herzegovina). Borrowers are personally liable for their loans and are typically small entrepreneurs that are not covered or vetted by the press, rating agencies or auditing firms.

EKI loan officers act as sales agents who collect all loan-applicant information, including from the credit registry, needed to make an initial lending decision. They fill out an electronic site-visit form with information on the borrower, his or her credit history and the available collateral. Initial lending decisions are discussed during a meeting of the branch-level loan committee on the basis of which loan applications are approved or rejected. Each branch employs on average 14 loan officers.

EKI did not introduce any changes to their lending policies around the time of the introduction of the credit registry other than obliging loan officers to pull reports on prospective clients from the registry. Throughout the period 2007-10 EKI had ample access to funding from both commercial lenders and international financial institutions. Funding costs did not change either over this period.

3. Data

3.1. Loan applications and granted loans

We have access to all loan applications received by EKI during the period January 2007-December 2012 and all loans granted during June 2002-December 2012. Figure A1 in the Appendix summarizes the loan applications (panel A) and approved loans (panel B) for the overlapping period January 2007-December 2012.[9] We also show the distribution of loans and applications across branches in high versus low-competition areas and, for approved loans, across new versus existing borrowers. For the loan applications, we know the age and gender of the applicant as well as the loan amount, loan purpose and maturity requested.

Table 1 shows that the rejection rate almost doubled, from 8 to 15 percent, after the introduction of the credit registry (the remainder of the loan applications was approved or, in a few cases, withdrawn by the applicant). A unique feature of our data is that we know *why* each loan was rejected, as loan officers are required to enter the main motivation for rejecting

[9] Tables A1 and A2 in the Appendix provide variable definitions and data sources.

a loan into the management information system. We split the various rejection reasons into those based on hard versus soft information or, alternatively, public versus private information. Rejections based on hard information are those where loan applicants were dismissed because of their age, a low credit score (negative registry information), too much outstanding debt elsewhere (positive registry information), a bad credit history with EKI itself, weak financials or insufficient collateral. Rejections based on soft information are those where the loan officer had doubts about the applicant's character, received a bad recommendation from someone else or where the loan purpose was unclear.

Rejections due to private information are based on information that EKI collected itself, either in the past or during the current screening. This includes information on the financial ratios of the borrower, the purpose of the loan, the character of the borrower and the available collateral. Rejections due to public information are those based on (positive) information about outstanding debt elsewhere or (negative) information about previous repayment problems. Both types of information became easily available with the introduction of the credit registry while they were unavailable before (as the voluntary exchange of borrower information among lenders was virtually absent).

Panel A of Table 1 shows a clear shift in the rejection reasons once the credit registry is introduced: more (less) loans are rejected due to hard (soft) information. Loan officers start to rely more on public information, in particular positive information about outstanding loans elsewhere. This suggests that mandatory information sharing led to a significant change in loan-officer behavior. According to documentation of one funder of EKI, "*EKI took increasing account of the client's overall indebtedness when providing loans*".

[Insert Table 1 here]

For the more than 200,000 loans approved between June 2002 and December 2012 we have information on their size, maturity, interest rate, collateral and purpose. We also have precise information on whether and when there was a late repayment, whether the loan was written off and, if so, how much principal and interest was recovered. We also know borrowers' income, education, gender, employment status and family size. Overall, we observe the complete borrowing history of over 130,000 unique borrowers and can therefore distinguish between new and returning borrowers. Lastly, we know the identity of the 458 different loan officers that granted the loans in our dataset. The average loan officer approved 21 (18) loans per month before (after) the introduction of the credit registry.

Panel B of Table 1 shows that the median granted loan amount is 2.5 times the average monthly household income of borrowers. The median maturity of granted loans is 19 months and the annual nominal interest rate is 21 per cent.[10] Borrowers use the loans mainly for business purposes, with about half of all loans used to buy movable assets such as equipment and vehicles. A vast majority of loans is collateralized, typically by some form of personal collateral and/or one or several guarantors.

Our measure of loan quality, *Problem loan*, is a dummy equal to one if a loan was written off. For each non-performing loan, we observe the date when the borrower first started to be in arrears (>30 days) and we take this as the default event in our hazard analysis (see Section 4). We do not use the write-off date as our default indicator because its timing depends more on the bank's discretion than on borrower behavior. It would therefore be a less clean signal of when repayment problems start. Before the introduction of the registry, 5.9 percent of all loans defaulted and this number went down to 1.7 percent after the introduction.

3.2. Local credit market competition

Our prior is that mandatory information sharing has a stronger impact in competitive credit markets. We construct both an objective and a subjective proxy for the intensity of lender competition in each of the 15 localities where EKI operates. First, following Canales and Nanda (2012), we calculate a Herfindahl-Hirschman index (HHI) where a lender's market share is the number of branches it operates in a locality. We collect time-varying data on the distribution of branches across Bosnia and Herzegovina by conducting a survey among loan officers where we ask them to list their local competitors. We cross-check this information with branch information from www.mixmarket.org, the second EBRD Banking Environment and Performance Survey (BEPS II) and lenders' annual reports. We then calculate an annual competition measure equal to $1\text{-}HHI_{bt}$ where *b* indicates the branch and *t* the year.[11]

Our second measure of local lender competition is based on loan officers' subjective perceptions. We use information from the aforementioned survey where loan officers in each branch were asked how much they agreed, on a scale from one to seven, with the following statement: "*In the last ten years, there has been an increase in competitive pressure in my*

[10] Consumer price inflation was 7 percent in 2008 (http://data.worldbank.org/country/bosnia-and-herzegovina).

[11] $\mathrm{HHI_{bt}} = \sum_{\mathrm{i=1}}^{\mathrm{N}} \mathrm{s_{bti}^2}$ where $\mathrm{s_{bti}^2} = \mathrm{Number\ branches_{bti}} / \mathrm{Total\ branches_{bt}}$ is the market share in terms of branches for lender *i* (where *N* is the number of lenders).

area of operation.” This competition measure is time invariant, averaged by branch and ranges between 3 and 6.5.

For both the objective and the subjective competition measure we construct a dummy equal to one (zero) for branches in areas with above (below) median competition levels. The ranking of areas according to the level of credit market competition is very similar before and after the introduction of the credit registry, indicating that the registry did not change relative competition levels across areas.

An interesting question is whether localities with high versus low credit-market competition also differ along other dimensions. Table A10 in the Appendix provides some insights in this respect. Panel A shows a number of socioeconomic outcomes taken from the 2nd Life in Transition Survey (LiTS), a representative household survey conducted in 2010. Overall, there are few statistically significant differences between households in low versus high-competition areas. Households in high-competition localities appear somewhat more affluent on average, as reflected in significantly higher computer ownership and somewhat higher employment levels and use of bank accounts.

Panel B compares the level and growth rate of economic activity in high versus low-competition localities. We use satellite data on luminosity (light intensity at night) as a proxy for local economic activity. This variable is measured on a scale ranging from 0 to 63, with a higher value indicating higher light intensity. The data show that before the introduction of the credit registry, both the level and the growth rate of economic activity were very similar in high versus low-competition localities. Moreover, also with the registry in place economic growth (that is, the trend in night-time luminosity) was independent of the level of lender competition. This indicates that any differential effects of the registry that we may find in high versus low-competition areas cannot simply be attributed to different growth patterns.

Interestingly and reassuringly, Panel C shows that both types of localities do differ along dimensions directly related to credit market competition. Before the registry, loans in high-competition areas were on average 2 percent larger and were granted to borrowers with a 4 percent lower income. The average loan-to-income ratio was therefore 7 percent higher in high-competition localities. Importantly, there is also a 10 percentage point difference in the proportion of borrowers with a stable income source. The average borrower in high-competition areas hence not only had a somewhat lower but also a less stable income. In response, lenders required more collateral in these high-competition localities.

4. Identification and empirical methodology

4.1. Impact on the extensive and intensive lending margins

We set out to identify the effects of mandatory information sharing on the extensive and intensive lending margins and the subsequent performance of approved loans. To this end we apply a difference-in-differences framework in which we regard loan applications and approved loans in high-competition areas as the affected or treated group and those in low-competition areas as the control group.

A key identifying assumption is that outcomes would have developed in parallel in the treatment and control group in case no credit registry had been introduced. Any trend differences that appear once mandatory information sharing is introduced can then be attributed to the registry. Figure A2 in the Appendix shows trends, conditional on borrower and loan characteristics, for four outcome variables in the low versus high-competition areas around the July 2009 registry introduction. The grey areas indicate the quarter in which the credit registry was introduced. In panels A and B, we observe that average loan amounts and terms developed very similarly in high versus low-competition areas. However, once information sharing becomes mandatory there is a sharp drop in loan size and maturity in both types of areas. In the same month there is also a sudden jump in the interest rate charged as well as the required collateral (panels C and D). We test more formally for parallel trends in Section 5 by running the baseline regression for a number of fictitious placebo events. Moreover, we show that our results are robust to controlling for any divergence in trends between high and low-competition areas in our regression framework.

We first use our diff-in-diff framework to measure the impact of information sharing on the extensive margin—the probability that loan applications get rejected—and then on the intensive margin (loan amount, term, interest rate and collateral). We apply this framework to a pooled dataset of all loan applications and approved loans in the year before and after the introduction of the credit registry. Our baseline pooled OLS regression model is:

$$Y_{ibt} = \alpha_1 \cdot Credit\ registry_t + \alpha_2 \cdot Competition_b + \beta \cdot I_{bt} + \gamma \cdot X_{ibt} + \varepsilon_{ibt} \qquad (1)$$

where Y_{ibt} is one of our outcome variables for loan or loan application *i* in branch *b* in month *t*; $Credit\ registry_t$ is a dummy variable that is one for all observations after June 2009 (the period when the credit registry was in place); $Competition_b$ is a dummy variable that is one for all loans and loan applications in high-competition branches; I_{bt} is an interaction term between *Credit registry*$_t$ and *Competition*$_b$; X_{ibt} is a matrix of covariates and ε_{ibt} is the error term. We cluster standard errors conservatively at the loan-officer level. Results remain quantitatively and qualitatively unchanged when we do not cluster or cluster by branch.

Our standard battery of covariates X_{ibt} includes loan-level control variables, such as dummies for various loan types, key borrower characteristics (such as age and gender) and a proxy for local economic activity. Since reliable conventional measures of local economic activity across Bosnia and Herzegovina do not exist, we use local night-light data from 2003 to 2010 as proposed by Henderson, Storeygard and Weil (2012).

A key parameter of interest is β: the additional impact of mandatory information sharing on loan outcomes in high-competition areas. Based on the prior that mandatory information sharing has a larger impact in more competitive credit markets, the interaction between the credit-registry dummy and the measure of local competition should be positive. To identify this interaction coefficient more cleanly, we also estimate:

$$Y_{ibt} = A_b + B_t + \beta \cdot I_{bt} + \gamma \cdot X_{ibt} + \varepsilon_{ibt} \tag{2}$$

where A_b and B_t are branch and month fixed effects to control for omitted local variables and economy-wide shocks, respectively. If information sharing matters more in high-competition branches, even after controlling for branch fixed effects, this is strong evidence that our results are not driven by omitted local variables.

We also estimate this fixed-effects model with a separate time trend, *time*$_t$, for high and low-competition areas. This allows us to control for possibly diverging trends in outcomes prior to the registry introduction. Equation (3) in effect provides an in-model correction, under the assumption that the trends are linear, for the case where the parallel-trends assumption may not be fully satisfied (Angrist and Pischke, 2009):

$$Y_{ibt} = A_b + B_t + \beta \cdot I_{bt} + \gamma \cdot X_{ibt} + \delta_0 time_t + \delta_1 time_t * Competition_b + \varepsilon_{ibt} \tag{3}$$

To obtain unbiased diff-in-diff estimates we need to ensure that we attribute impacts to information sharing and not to differences between borrower groups due to non-random assignment across areas with different levels of lender competition. We therefore also present a variant of Equation (1) where we use propensity-score matching, based on borrower and loan characteristics, to assure that borrowers in high (treatment) and low (control) competition areas are comparable. By matching borrower and loan characteristics we also circumvent the issue of jointness of loan terms (Brick and Palia, 2007).

We match loans on loan, borrower and local characteristics and calculate propensity scores with nearest-neighbor matching with replacement. There is ample common support: less than one percent of observations fall outside the support area. We then use the propensity scores as weights in a linear regression model where we exclude variables that might be jointly determined with our dependent variables. We apply a double-robust estimator (Robins, 2000) which yields unbiased estimates of the average treatment effect when either the propensity-score matching model or the linear regression model is correctly specified.

A final note is warranted on timing. Like most countries, Bosnia and Herzegovina was not immune to the global financial crisis. One may therefore wonder whether any effects we may find should be partly attributed to the crisis rather than the introduction of the credit registry. We will provide three pieces of evidence to show that this is unlikely. First, and most importantly, our data show that immediately after (but not before) the introduction of the credit registry, loan officers started to reject more loan applications on the basis of registry information. This 'smoking gun' points directly to the registry causing the observed changes in lending behavior. Second, we provide an extensive set of placebo tests that show that our results quickly disappear if we let our registry treatment start just one or two quarters earlier (that is, when we move the treatment closer to the start of the crisis but further away from the actual credit-registry introduction). Third, the next section demonstrates that we find a strong *positive* effect of the new registry on loan quality. This is difficult to reconcile with the idea that our results would pick up a crisis effect, as the crisis would arguably have had a negative rather than a positive effect on borrower quality.

4.2. Impact on loan quality

The second part of our analysis focuses on the impact of mandatory information sharing on repayment performance and loan quality. We define the hazard rate as the probability that a borrower is late on their repayment at time *t* conditional on regular repayment up to that point. A hazard function allows us to model not only whether a borrower defaults but also how the default probability changes over time.[12] Our main variable of interest is the time between disbursement and the first instance of late (>30 days) repayment. We do not use the

[12] See also Ongena and Smith (2001), Ioannidou, Ongena and Peydró (2015) and Jiménez, Ongena, Peydró and Saurina (2014) for recent applications of duration analysis in the empirical banking literature.

write-off date as our default indicator because its timing depends more on the lender's discretion than on the borrower's default date. The hazard model allows us to compare the development of hazard rates before and after the introduction of mandatory information sharing and for first-time versus repeat EKI borrowers.

An important advantage of hazard models is their ability to deal with censoring, which occurs when a loan is repaid or when the life of a loan extends beyond the sample period. Such right censoring may yield biased and inconsistent estimates in static probability models (Ongena and Smith, 2001). A semi-parametric model (Cox and Lewis, 1966; Cox, 1972), which makes no assumption about the form of the hazard function, can deal with right censoring as the log-likelihood function accounts for the ratio of completed versus non-completed loans.[13]

Let *T* measure the amount of time before the first late repayment of the loan. The hazard function *h(t)* is the probability of repayment being late at time *t* conditional on regular repayments until then:

$$h(t) = \lim_{\Delta t \to 0} \left\{ \frac{P(t \leq T < t + \Delta t | T \geq t)}{\Delta t} \right\} \tag{4}$$

Alternatively, we can model the distribution of time until first late repayment as a survivor function:

$$S(t) = P(T \geq t) \tag{5}$$

The relationship between the survivor function and the hazard function is then:

$$h(t) = \frac{-d \log S(t)}{dt} \tag{6}$$

We can now estimate the effect of a set of time-varying covariates *Xt* and the distribution of time to default with the proportional hazard model:

$$h(t) = \lim_{\Delta t \to 0} \left\{ \frac{P(t \leq T < t + \Delta t | T \geq t, X_t, \beta)}{\Delta t} \right\} = h_0(t) exp(\beta' X_t) \tag{7}$$

[13] Left censoring can bias estimates as well, but it is not an issue in our case as we only observe new loans.

where h_0 represents the baseline hazard when covariates are set to zero: $X=0$. Covariates shift the baseline hazard without affecting the underlying shape of the hazard function. In the Cox (1972) semi-parametric approach the functional form of h_0 is not specified. The model uses the ranking of duration times to estimate the β parameters via maximum likelihood methods.

The Cox proportional hazard model relies on two assumptions. First, it assumes continuous time, as the presence of tied events in discrete time makes ranking impossible. Since late repayments are only observed at intervals, we deal with tied events with the approximation by Breslow (1974). Second, it assumes proportionality, which implies time fixed β coefficients. We relax this assumption by estimating a model where the effect of covariates X_t can change over the life of the loan.

We check the robustness of our results to the functional form of the hazard rate by estimating two parametric specifications using a Weibull and an exponential distribution. The Weibull distribution is expressed as:

$$h(t) = h\alpha t^{\alpha-1} \tag{8}$$

where α measures duration dependence. If $\alpha>1$ the hazard rate increases with time (positive duration dependence). The exponential distribution is a special case of the Weibull distribution characterized by a constant hazard rate ($\alpha=1$): the probability of late repayment is constant over time (Kiefer, 1988).

5. Results

5.1. Information sharing and loan rejections

Table 2 provides estimation results, based on our difference-in-differences framework, to explain the probability that a loan application was rejected. In addition to the variables *Credit registry*, *Competition* and their interaction term, all specifications include our standard applicant and loan covariates. Columns 1 and 2 show the baseline specification, estimated with a logit and linear probability model, respectively. We provide the logit model as a benchmark but focus primarily on the linear model so that we can estimate the model using time and fixed effects; can directly interpret the coefficients as marginal effects; and prevent the well-known problems associated with interaction effects in nonlinear models (Ai and Norton, 2003). A possible disadvantage of linear probability models is that fitted values might fall outside the 0,1 bounds. However, in our case more than 99 percent of the linear predictions have a value that lies between zero and one.

[Insert Table 2 here]

We find that the introduction of the credit registry is associated with a large and statistically significant increase in the probability that a loan application gets rejected, all else equal. In the logit model in column 1, the marginal probability of rejection increases by 4.8 percentage points in low-competition areas once the credit registry is introduced. This increase is consistent with the magnitude of the linear probability effects reported in the subsequent columns which range between 4.8 and 5.2 percentage points. In all cases the coefficients are significantly different from zero at the one percent level.

In line with the theoretical literature outlined in Section 1, we expect stronger effects in high-competition areas. This is indeed the case as the interaction term of *Competition* and *Credit registry* has a positive and significant effect in all models. After the introduction of the credit registry, the rejection probability increases by an additional 4 percentage points in high compared with low-competition areas (9 versus 5 percentage points). Mandatory information sharing appears to be more effective in competitive credit markets. The statistical and economic significance of this result survives when we add branch fixed effects (column 3), month fixed effects (column 4) or both (column 5). Including separate time trends for high and low-competition branches (column 6) does not alter the results either.

We also observe a significantly higher base probability of rejection in high-competition areas, with a marginal effect close to 2 percentage points for the probit model and 1.8 percentage points for the linear-probability models. These level effects of course disappear once we control for cross-sectional differences in competition with branch fixed effects in columns 3, 5 and 6.

In Table A3 in the Appendix we subject the baseline interaction effect between *Competition* and *Credit registry*, based on the linear probability model, to a number of robustness (columns 1-3) and placebo (columns 4-6) tests. In the first three columns, we vary the time window over which we estimate the effect of the registry introduction. Our regular window is one year before and one year after the introduction. In column 1 we use a narrower symmetric window of just one year in total (February 2009-February 2010). In column 2 we then use a wider window which comprises the period May 2008-December 2010 while in column 3 we use the widest window possible given the available data: January 2007-

December 2012. In all cases the statistical and economic significance of the impact of the credit registry is very similar to our base result in Table 2.

We provide placebo tests in columns 4 to 6 to confirm that hitherto undetected secular trends do not drive our results. This is also a more formal way to test for the parallel-trends assumption: since at the placebo dates no credit registry was introduced, we should not detect any impact. In column 4 we move our two-year window one year forward. We thus take the true treatment period as the control period and let the treatment only start in July 2010 (basically assuming that the credit registry was introduced a year later than in reality). In column 5 we move our two-year window one year backwards. We now take the true control period as the treatment period and assume that the credit registry was already introduced in July 2008. This placebo test is especially useful because it checks whether we are not picking up any impact of the global financial crisis. Finally, in column 6 we randomly allocate branches to high or low-competition status. We repeat this random allocation a thousand times and show the average result (here the treatment starts in July 2009, the actual date of the credit-registry introduction). In all three placebo tests our results disappear completely. This gives us additional confidence that the results in Table 2 indeed reflect a change in lending behavior due to the introduction of the credit registry in July 2009.

The finding that information sharing increases the probability that a loan application is rejected, in particular in competitive areas, suggests that the newly available information makes loan officers more conservative. This is in line with theories that stress over-borrowing in competitive areas in the absence of information sharing (Parlour and Rajan, 2001). In Table 3 we assess which information is responsible for the increased conservatism among loan officers after the introduction of the registry. We present multinomial logit regressions to link the probability of loan rejection to the use of various types of borrower information. The dependent variable is categorical and indicates whether a loan application was accepted (which we take as the base probability) or rejected on the basis of different types of information. We then estimate the effect of the introduction of the credit registry on rejections due to hard versus soft information (columns 1 and 2) or, in a separate multinomial set-up, due to private information, negative public information or positive public information (columns 3-5).

[Insert Table 3 here]

The results in columns 1 and 2 show, in line with Table 2, that the introduction of mandatory information sharing led to a higher rejection probability and that this is especially so in high-competition areas. We now also observe directly that it is *hard* information that is responsible for this stricter screening by loan officers. In contrast, the probability that a loan gets rejected due to soft information goes down after the introduction of the registry, especially in low-competition areas. Note that there is a positive base effect of lender competition on the rejection probability due to hard information (column 1) but not soft information (column 2). This is in line with theories that stress that lending competition reduces banks' investments in generating and using soft information (Hauswald and Marquez, 2006).

In columns 3 to 5, we cut the data in a different way and compare rejections due to private versus public information. The latter is split up in positive versus negative information, both of which became more easily available due to the registry. We find that after the registry introduction loan officers reject more loans on the basis of both private and public information although the impact of public information is much stronger. In particular, column 5 shows a very strong increase in rejections due to positive information about applicants' debt elsewhere and this holds independent of local competition. The use of negative information (credit scores that contain information about applicants' past defaults) increases too. This is especially the case in high-competition areas where adverse selection can be expected to be more severe.

5.2. Information sharing and loan terms

We proceed by analyzing the change in lending conditions on the intensive margin around the time of the credit-registry introduction. The loan terms we consider are *Loan amount*, *Loan maturity*, *Interest rate* and *Collateral* (the sum of personal, social and third-party collateral). We again assess both the direct effect of the introduction of the registry and its interaction with credit market competition.

Table 4 reports the difference-in-differences results. Mandatory information sharing was accompanied by a *reduction* in both loan amounts and maturities and an increase in the interest rate charged and collateral required. All of these effects are statistically significant, stronger in competitive areas and hold when including our standard set of borrower and other covariates. The unreported covariate coefficients show that older, highly educated, higher-income and urban borrowers receive larger loans at lower interest rates.

These results also hold when we use propensity-score matching to assure that borrowers in high (treatment) and low (control) competition areas are comparable (column 3).[14] The same holds when adding month and loan-officer fixed effects in column 4, which comes at the cost of not being able to estimate the level effects of *Credit registry* and *Competition*, and when adding time trends for high and low-competition branches (column 5). Finally, column 6 shows that our results do not change when we sort branches by competition level based on the subjective rather than the objective *Competition* measure.

[Insert Table 4 here]

After the introduction of the registry, the loan size drops by 19 percent. In high-competition areas, the reduction is even more pronounced, averaging 25 percent. The same pattern can be found when looking at loan maturity, with loans shortening by 13 percent in low-competition areas (equivalent to 90 days) and by 16.3 percent in high-competition areas (almost 120 days). These smaller and shorter loans also become more expensive, with interest rates increasing by 0.7 and 0.8 percentage points in low and high-competition areas, respectively.[15] Gehrig and Stenbacka (2007) argue that information sharing generates a flatter inter-temporal structure of interest rates as banks see fewer benefits to establishing long-term lending relationships. In line with our results, their model suggests that information sharing increases the interest rates paid by new borrowers.

In a similar vein, collateral requirements go up after the introduction of the credit registry by 0.68 extra items pledged per loan. In high-competition areas the number of required collateral items increases by 0.83. The increased reliance on collateral is in line with US evidence presented by Doblas-Madrid and Minetti (2013) and theoretical work by Karapetyan and Stacescu (2014) who show that information sharing and collateral may be complements as borrowers with a bad credit history are now more likely to face collateral

[14] The same holds when we match to correct for possible longitudinal changes in the borrower pool (not shown). When we compare new borrowers before and after the registry introduction along various observable characteristics, we find they have not changed much. This suggests that EKI did not react to the registry by shifting its lending to different types of borrowers.

[15] The positive impact of information sharing on interest rates holds whether we control for loan amount or not.

requirements.[16] In all, our results indicate clearly that the introduction of the credit registry led loan officers to significantly tighten their lending conditions on the intensive margin.

In Appendix Tables A4 and A5 we report similar placebo and robustness tests as the ones we performed for the extensive lending margin. The same covariates as in Table 4 are included but not shown for reasons of brevity. We again find that our results disappear once we move the start of the treatment to a fictitious date one year earlier or later (Table A4). And, as before, our coefficient of interest is robust to broadening or widening the window around the correct starting date of the registry (Table A5).

Since the timing of our treatment is a crucial part of our identification strategy, we further investigate the impact of varying the treatment timing (Figure 1). In effect we undertake a set of placebo tests to confirm that we detect the introduction of the credit registry and not a secular trend or a crisis effect. The graph shows the coefficient estimates and a 95 percent confidence interval for the interaction terms *Credit registry*Competition* as used in column 5 of Table 4. The value at time *t* shows the coefficients when using the actual timing of the registry introduction. The values at *t-1*, *t-2*, and so on, indicate the estimates when introducing the registry one quarter, two quarters, and so on, earlier than the real date. Figure 1 shows that when we artificially bring the credit-registry introduction forward, the placebo impact quickly reduces in size and essentially becomes zero just one or two quarters before the actual introduction date. We conclude that our findings indeed capture the shift in information-sharing regime and not a longer-term trend.

In additional unreported placebo tests we let the treatment period start in October 2010 for *Loan amount*, September 2006 for *Loan maturity* and February 2009 for *Interest rate*. These placebo start times are chosen on the basis of a Clemente-Montañés-Reyes unit-root test, which indicates a possible break point in that month for each dependent variable. We also perform a test where the placebo treatment starts in September 2008—the collapse of Lehman Brothers—and ends with the introduction of the registry in July 2009. If we simply picked up a crisis effect, it should show up here. Throughout all these placebo tests as well, our original results disappear. This suggests that we indeed pick up the true registry effect in our baseline regressions and not another trend or break-point.

[16] This fits with a broader empirical literature (Roszbach, 2004; Rice and Strahan, 2010; Berger, Scott Frame and Ioannidou, 2011) and theoretical work (Boot, Thakor, and Udell, 1991; Inderst and Mueller, 2007) highlighting that observably riskier borrowers are more likely to be required to pledge collateral.

[Insert Figure 1 here]

Finally, in Table A6 we replace our local competition variable with a variable equal to one minus the local market share of EKI.[17] A higher value thus indicates a smaller market share of the lender whose data we analyze. We calculate EKI's local market share as the number of branches and offices it operates in a locality as a percentage of all branches and offices operated by financial institutions in that locality. We expect that a stronger market position of EKI translates into a smaller impact of the credit registry, as the new registry information is less important. The results in Table A6 are in line with this prediction. At the extensive margin, the registry increased the probability of loan rejection in particular in localities where EKI did not have a strong market position (column 1). In these branches, loan officers benefited the most from the newly available information. At the intensive margin lending became stricter too: loan amounts went down more while interest rates and collateral requirements increased more (columns 2 to 5). Finally, there is some evidence that loan quality improved more in localities where EKI had a smaller market share, although this differentiated impact is imprecisely estimated (column 6).

5.3. Information sharing and loan terms: First-time versus repeat loans

Table 5 compares the impact of information sharing on first-time loans with that on repeat loans.[18] We assess the evolution of subsequent loans for borrowers who successfully repaid their first loan. Using borrower-fixed effects, we find that subsequent loans become progressively larger, longer and cheaper.[19] As the lender gathers information about the borrower, loan terms are gradually relaxed to reward timely repayment.

[Insert Table 5 here]

It is particularly interesting that this effect becomes stronger for all of the loan terms after the introduction of the credit registry, suggesting a decline in switching costs once mandatory information sharing is introduced. This is reflected in the statistically significant coefficients for the interactions between the loan numbers (2^{nd}, 3^{rd} and 4^{th}) and the *Credit registry*

[17] The correlation coefficient between both variables is 0.38.

[18] First-time clients are new to EKI but may have borrowed from other lenders in the past.

[19] The use of borrower fixed effects means that all one-time borrowers drop out of these regressions so that we compare first-time and repeat loans among a set of repeat borrowers.

dummies. The implication is that while information sharing results in tighter terms for first-time loans, it improves these terms for repeat loans. We also find that collateral requirements for repeat loans go down with the registry in place (while this was not the case before the registry). In the absence of information sharing, repeat borrowers that try to switch to a competing lender get pooled with low-quality firms and may be offered an unattractive interest rate (Sharpe, 1990). With information sharing, outside lenders can observe good borrower performance. This reduces the market power of the incumbent lender and boosts the bargaining power of reputable borrowers (Padilla and Pagano, 1997). This leads to better loan terms over the course of the lending relationship, in line with Petersen and Rajan (1995) and the aforementioned theoretical work of Gehrig and Stenbacka (2007).

Interestingly, additional regressions (reported in Appendix Table A7) indicate that the extra increase in loan amount and maturity and decline in interest rates and collateral for repeat loans after the introduction of the registry was mostly driven by high-competition areas. In areas where more lenders compete, information sharing opens up more outside options to formerly captive borrowers and, as a result, the impact of information sharing on repeat borrowers is higher in such competitive credit markets.

Finally, in unreported regressions we test whether the credit registry had a different effect on high versus low-income clients. We expect that access to borrower information is more important for riskier clients which we proxy by monthly borrower income. When we interact *Credit registry* with borrower income, we continue to find a negative base effect on loan amount and a positive effect on the interest rate and collateral. As expected, however, the effect of the credit registry is significantly smaller for high-income borrowers.

5.4. Information sharing and loan quality: Non-parametric results

Figure 2 provides a first non-parametric look at our data on loan quality in the form of a Kaplan and Meier (1958) survival analysis over the period June 2002 to December 2012. The graphs show how the probability that a borrower has not (yet) defaulted on her loan changes over time (horizontal axis, in quarters). At the time of disbursement (*t=0*) the probability of survival is by definition 1 but then gradually erodes over time. The graphs thus show the inverse of the cumulative default probability.

Panel A compares, for the whole sample period, the survival probability of borrowers in the branches that face below-median competition with those with an above-median level of competition. The key point to take away from this panel is the minimal difference in the survival behavior among borrowers in high versus low-competition areas. The difference between both curves is statistically insignificant as shown by a logrank test (p-value=0.60).

[Insert Figure 2 here]

In panel B, we start to compare the survival behavior of loans granted before and after the introduction of the credit registry. In this context, right censoring will affect disproportionately the more recent loans. The correct hazard rate is then calculated as the ratio of loans that have defaulted at time *t* over the remaining loans (Ongena and Smith, 2001). Panel B reveals a substantial difference in repayment behavior. Loans granted with the credit registry in place have a significantly higher survival probability compared with loans approved without mandatory information sharing. This is the first piece of evidence that points to a positive impact of information sharing on loan quality.

A striking aspect of panel B is that the difference between both loan types already emerges during the first quarters after loan disbursement. Indeed, the probability of a loan not being late in the first six months after disbursement increases from 94.6 percentage points before the credit registry introduction to 98.6 percentage points afterwards. Over time this difference declines but stays statistically and economically significant.

Panels C and D look at the interaction of mandatory information sharing and local credit market competition. Panel C shows that without information sharing repayment rates are significantly worse in high-competition areas (the p-value of a logrank test is 0.00). However, we observe the opposite after the registry introduction (panel D, p-value=0.00). Repayment behavior now becomes even slightly better in high-competition areas (and this is what drove the lack of an overall difference over the whole sample period in panel A). The difference is one (two) percentage points after 12 (24) months and remains significant throughout the sample period. This effect is economically meaningful as it amounts to a third of the average default rate in the period before mandatory information sharing.

In Figure 3 we take this analysis one step further and distinguish between first-time borrowers (clients that had never borrowed from EKI) and repeat borrowers. On the one hand, we expect the impact of the credit registry to be concentrated among new borrowers as the information asymmetry between lender and loan applicant is largest. On the other hand, to the extent that the registry (also) had an impact on borrower behavior, we expect an improvement in repayment behavior among repeat borrowers as well as these now realize that a default will "cost" them more in terms of foregone future borrowing opportunities. As before, we also slice our data by competition level, leading to the four panels in Figure 3.

[Insert Figure 3 here]

In panels A and B we first focus on new borrowers. There is a striking difference compared with the two top panels in Figure 2. The impact of the credit registry is much larger for new borrowers, suggesting that the registry mainly "worked" through the lender side. Comparing the low-competition areas (panel A) with the high-competition areas (panel B) we see clearly that the difference between both survival functions is widest and most persistent in the high-competition areas. It is in these highly competitive areas, where adverse selection problems are likely to be more important, that the registry has the most bite and loan officers put the hitherto unavailable borrower information to good use. In these areas the survival probability for new borrowers after 12 months increased from 92.5 to 97.5 percent.

In panels C and D we present a similar comparison but now for repeat borrowers. Independent of the level of competition, we see that the registry introduction is accompanied by an upward shift of the survival function: at each point in time repeat borrowers are less likely to default, suggesting that mandatory information sharing also increased borrower discipline. However, while in both graphs the differences between the "before" and "after" graphs are statistically significant (p-value is 0.00 in both cases), the difference is relatively small and declines over time. The main impact of the introduction of the credit registry therefore appears to come from a better selection of borrowers.

5.5. Information sharing and loan quality: (Semi-)parametric results

In Table 6 we proceed by providing semi-parametric and parametric evidence on the impact of mandatory information sharing on loan quality. As discussed in Section 4.2, an important advantage of hazard models—where the hazard rate is the probability of a borrower defaulting at time t conditional on having repaid regularly up to that point—is that they deal properly with right censoring. A second advantage is that the specifications in Table 6 allow us to control for a battery of borrower and loan covariates. We stratify by branch so that the form of the underlying hazard function varies across branches (the coefficients of the remaining covariates are assumed constant across strata). Hence we do not need to assume a particular form of interaction between the stratifying covariates and time.

[Insert Table 6 here]

In columns 1-4 we present the results of a semi-parametric Cox proportional hazard model while columns 5 and 6 show equivalent specifications using a parametric exponential and Weibull model, respectively. In the first column we limit our sample to loans to first-time borrowers, whereas in the following columns we use all loans and include a *First loan* dummy. We then interact this dummy with *Credit registry* to test whether the impact of mandatory information sharing was larger for first-time borrowers (as Figure 3 suggests).

The results in the first three rows of Table 6 show that the registry introduction is associated with a statistically significant reduction in the hazard rate. Importantly, this effect is almost twice as high in high-competition areas, in line with Figure 2 and the literature that we discussed before. The second line shows that the level of bank competition as such does not have an impact on the hazard rate, analogous to panel C of Figure 2.

In the lower part of the table we show the estimated coefficients for our control variables. These have the expected sign and in most cases display a statistically significant relationship with the hazard rate. For instance, we find that older and more educated borrowers pose less risk while longer and larger loans tend to have higher repayment risk, all else equal.

As expected, columns 2-6 show that the interaction term between *First loan* and the *Credit registry* dummy is significantly negative, indicating that the registry reduced defaults in particular among first-time borrowers. The coefficient for *First loan* itself is negative but not significantly different from zero.

In column 4, we relax the proportionality assumption of the Cox model and allow the effect of the covariates to change over time. This yields practically identical estimates. The same holds for the parametric exponential model in column 5 and the parametric Weibull model in column 6. The latter produces an *Ln(alpha)* of -0.645, indicating that the hazard rate decreases over time as a substantial part of the borrower risk is front loaded.

In Figure 4 we undertake a further placebo analysis to check that we pick up the introduction of the credit registry and not a secular trend. Similar to Figure 1, the graph shows the coefficient estimates and a 95 percent confidence interval for the interaction term *Credit registry*Competition* as used in column 2 of Table 6. The value at time *t* shows the coefficient when using the actual timing of the registry introduction. The values at *t-1*, *t-2*, and so on reflect estimates when introducing the registry one quarter, two quarters, and so on, earlier than the real date. When we bring the registry introduction forward, the placebo impact is quickly reduced in size and becomes zero two quarters before the actual introduction date. We conclude that our measurement of the impact of the registry indeed captures the shift in information-sharing regime.

[Insert Figure 4 here]

5.6. Robustness

In Appendix Table A8, we provide further evidence on the robustness of these findings by estimating similar models while allowing covariates to change over the life of the loan. In order to include time-varying covariates we modify the structure of our dataset so that the number of observations on each loan equals the number of periods between disbursement and either repayment or default (Singer and Willett, 1993). The hazard rate now not only depends on the loan and borrower characteristics at the time of disbursement, but also on a set of other variables—including the introduction of the credit registry—that may change during the life of the loan. The results in Table A8 are fully in line with those in Table 6: default risk is lower once the registry is introduced and this holds in particular in more competitive areas and for first-time borrowers.

Table A9 shows that our results are also robust to adding interaction terms between *Credit registry* and other locality covariates. We perform this exercise to confirm that our interaction term picks up local competition rather than other locality characteristics. We construct these new locality variables using the second wave of the EBRD-World Bank Life in Transition Survey (LiTS II), a nationally representative household survey administered in 2010. We calculate the mean monthly food spending of households in a locality; the percentage of households that own a computer; the percentage of households that have a bank account; the percentage of households that can be classified as risk takers based on LiTS II; the percentage of household heads that are employed; the percentage of orthodox Christian households; the unemployment rate in the canton and the cantonal GDP. Overall, there are few significant differences between high and low-competition localities along these dimensions (Appendix Table A10).

If the introduction of the credit registry affected lending outcomes more in highly competitive areas, then the coefficient of *Credit registry*Competition* should remain negative and significant while the coefficient for the interaction term with each LiTS variable should be insignificant. The first line of Table A9 shows that our baseline interaction result is indeed robust to the inclusion of these various additional interaction terms.

5.7. The impact of information sharing on lender profitability

The introduction of mandatory information sharing affected lending along several margins: more applications were rejected while granted loans became smaller, shorter and more expensive. At the same time, loan quality increased as repayment went up. What has been the combined impact of these adjustments on the lender's profitability?

To answer this question we first evaluate the profitability of EKI in the year before (June 2008–June 2009) and the year after (July 2009–July 2010) the introduction of the credit registry. We calculate the present value of all loans disbursed in each of those years. For the first year all values are discounted to June 2008 and for the second one to July 2009. We use a weighted average of the interest rate on all loans granted to EKI as the discount rate.

For each year we then calculate the present value of total loan disbursements, the probability of loan default, the net present value of the loans, and the net present value per dollar lent. The light-grey bars in Figure 5 show in the year after the introduction of the credit registry a substantial decline in the present value of lending (measured as the total amount of new lending, net of fees, discounted back to the beginning of each period using the lender's average funding cost). The present value of total lending goes down by 49.7 percent due to the combined effect of more loan rejections and smaller loans.

At the same time, however, we know from our previous analysis that the credit registry led to a substantial decline in the probability of default (loans that were at least 30 days late and were subsequently written off): from 10 to 4 percent (right axis). As a result of this strong increase in repayment performance once the registry is in place, the *net* present value of all loans (disbursements minus repayments) declined by only 31.2 per cent. Indeed, the net present value per US$ lent increased from 0.11 to 0.14 (right axis) and the internal rate of return (IRR) on lending increased from 17.6 to 21.8 per cent (an increase of 23.9 per cent, not shown). Given that the cost of capital was roughly the same during both periods, and under the assumption that operational costs did not change substantially, these numbers indicate that mandatory information sharing significantly increased the profitability of EKI.

[Insert Figure 5 here]

In Table 7 we analyze the impact of information sharing on lender profitability at the loan level. We calculate the realized return (cf. Haselmann, Schoenherr and Vig, 2013) on loans made in the year before and the year after the introduction of the credit registry. For loans that were fully repaid, this return is simply the interest rate charged. For loans that were defaulted on, the realized return is the weighted average of the return before the moment of default and the return after default took place. Before default, the return is again simply the interest rate charged over the (gradually declining) outstanding amount. After the default, the return is negative and reflects the amount of the loan outstanding at the time of default as well as the portion of that amount that the lender managed to recover (if any).

The results in Table 7 show a significant increase in the average return on loans of about 3 percentage points, reflecting the better repayment behavior due to the registry introduction. This is an economically meaningful improvement equal to 18 percent of the pre-registry average return on loans. The interaction terms in columns 3 to 6 indicate that this positive effect is particularly prominent in high-competition areas.

[Insert Table 7 here]

6. Conclusions

Finding novel ways to overcome credit-market frictions remains of first-order importance. An increasing number of emerging markets regard public credit registries that collect, consolidate and distribute reliable borrower information as a potentially effective tool to counterbalance weak creditor protection and inadequate bankruptcy laws. At the same time, many advanced countries are considering new or improved credit registries as part of their response to the global financial crisis. In Europe, for instance, these discussions focus on efforts to consolidate national data within one European central credit registry (IIF, 2013).

Are credit registries a useful component of a country's financial infrastructure? To help answer this question we present direct evidence of what happens when lenders are required to start sharing borrower information. Our analysis exploits unique data of a large small-business lender in a middle-income country. We have access to detailed information on the terms—amount, maturity, interest rate, collateral—and performance of all approved loans as well as on all rejected loan applications. We also know *why* loan applications were rejected.

Using these data, we document how mandatory information sharing allows loan officers to lend more conservatively at both the extensive and intensive margins. This increased conservatism reflects in particular the availability of positive credit-registry information, which provides loan officers with a complete picture of the indebtedness of loan applicants. The use of negative information about past defaults increases too, in particular in high-competition areas where adverse selection is likely to be more severe. The resulting improved credit allocation increases loan quality considerably and this applies in particular to more competitive areas and to first-time borrowers.

At first sight, the increase in rejection rates and associated reduction in lending appears at odds with cross-country evidence that shows a positive correlation between information sharing and banking sector depth. Our view is that both observations are not inconsistent. In particular, our identification strategy exploits data on the change in lending behavior during a narrow time window around the change in information-sharing regime. This identification allows us to precisely estimate whether and how mandatory information sharing affects lending behavior. In line with comparable loan-level evidence presented by Doblas-Madrid and Minetti (2013) we find no immediate loosening of lending standards. Indeed, the short-term impact is to tighten standards as the newly available information leads to a reassessment of borrowers' total indebtedness. This is also in line with recent theoretical work by Gehrig and Stenbacka (2007) who predict that information sharing may reduce lending and increase interest rates for first-time borrowers without a credit history.

In the longer term, however, the improved functioning of the credit market (and the associated higher profitability of lenders) can be expected to contribute to credit expansion. Indeed, our data already show how the increased transparency in the credit market—and the associated reduction in switching costs—allows well-behaved repeat borrowers to increase their borrowing limits and enjoy better loan conditions. Overall, our findings therefore illustrate how mandatory information sharing can help loan officers to make better informed credit decisions and to match loan offers more precisely with applicants' repayment capacity.

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Table 1. Summary statistics

	Mean pre-Credit registry	Mean post-Credit registry	Obs.	Median	St. dev.	Min	Max
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: Extensive margin							
Loan rejected	0.082	0.147***	210,044	0	0.322	0	1
Loan rejected: Hard information	0.038	0.123***	210,044	0	0.277	0	1
Loan rejected: Soft information	0.045	0.023***	210,044	0	0.180	0	1
Loan rejected: Private information	0.064	0.058***	210,044	0	0.234	0	1
Loan rejected: Public information (negative)	0.004	0.034***	210,044	0	0.141	0	1
Loan rejected: Public information (positive)	0.014	0.054***	210,044	0	0.186	0	1
Panel B: Intensive margin							
Dependent variables:							
Loan amount (BAM)	3,564	3,173***	236,893	3,000	2,802	500	15,000
Loan maturity	23	23	236,893	19	11	6	60
Interest rate	18.54	21.21***	236,893	20.50	3.90	12	26
Collateral	2.30	2.41***	236,893	2.00	1.51	0	10
Problem loan	0.059	0.017***	236,893	0	0.208	0	1
Return on loan	16.95	20.20***	56,787	21	13.18	-84.84	26
Independent variables:							
Competition: 1-HHI	0.807	0.799***	236,893	0.806	0.068	0.556	0.898
Perceived competition	4.981	5.099***	234,185	5.5	1.1555	3	6.5
Lender's market share	0.264	0.264	236,893	0.273	0.114	0.122	0.625
Loan/income ratio	3.186	2.975***	236,893	2.484	2.332	0.444	11.765
Borrower age	40	42***	236,893	40	12.094	20	68
Borrower male	0.593	0.612***	236,893	1	0.490	0	1
Borrower education	1.93	1.95***	236,893	3	0.392	2	4
Borrower monthly income (BAM)	1,212	1,159***	236,893	1,031	577	350	3,691
Borrower urban	0.39	0.33***	236,893	2	0.674	1	3
Loan immovable	0.081	0.010***	236,893	0	0.282	0	1
Loan movable	0.427	0.531***	236,893	0	0.498	0	1
Loan stock	0.408	0.181***	236,893	0	0.472	0	1
Loan household	0.071	0.142***	236,893	0	0.291	0	1
Personal collateral	0.248	0.319***	236,893	0	0.546	0	2
Social collateral	1.968	1.994	236,893	2	1.021	1	5
Third-party collateral	0.040	0.088	236,893	0	0.308	0	2
Loans/officer	21.42	17.66***	236,893	20	9.048	2	45
Branch growth (quarterly)	0.058	0.044***	236,131	0.023	0.265	-0.495	1.241

Notes: Panel A: Sample period is January 2007-December 2012. Panel B: Sample period is June 2002-December 2012 except for *Return on loan* for which the sample period is June 2008-July 2010. Asterisks refer to the p-value of a t-test of equality of means and *** indicates significance at the 1% level. BAM is Bosnian Convertible Mark.

Table 2. Extensive margin: Information sharing and loan rejections

Dependent variable →	Loan rejected					
	Logit	Linear probability model				
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	0.409***	0.043***	0.055***			
	(0.034)	(0.004)	(0.004)			
Competition	0.170***	0.016***		0.014***		
	(0.032)	(0.003)		(0.003)		
Credit registry*Competition	0.236***	0.044***	0.016***	0.050***	0.026***	0.041***
	(0.046)	(0.006)	(0.005)	(0.005)	(0.005)	(0.007)
No. of applications	63,893	63,893	63,893	63,893	63,893	63,893
(Pseudo) R-squared	0.022	0.019	0.036	0.028	0.043	0.043
Applicant covariates	Yes	Yes	Yes	Yes	Yes	Yes
Loan covariates	Yes	Yes	Yes	Yes	Yes	Yes
Branch fixed effects	No	No	Yes	No	Yes	Yes
Month fixed effects	No	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	No	Yes

Notes: This table shows logit (column 1) and linear probability (columns 2-6) regression results to explain the probability that a loan application was rejected. Robust standard errors in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively. A Hausman test rejects equivalence of random and fixed effects models. Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include applicant covariates, loan size and type, and a time-varying night-light measure of local economic activity. Constant not shown. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where market shares are measured in number of branches). Table A1 in the Appendix contains all variable definitions.

TABLE 3. Types of borrower information and loan rejections

	Hard vs Soft information		Private vs Public information		
			Private	*Public information*	
Rejection reason →	*Hard*	*Soft*	*information*	*Negative*	*Positive*
	[1]	[2]	[3]	[4]	[5]
Credit registry	0.653***	-0.768***	0.205***	0.739***	1.254***
	(0.037)	(0.086)	(0.048)	(0.053)	(0.105)
High competition	0.222***	0.029	0.219***	-0.021	0.647***
	(0.037)	(0.059)	(0.043)	(0.057)	(0.108)
Credit registry*Competition	0.130**	0.814***	0.235***	0.285***	-0.041
	(0.051)	(0.112)	(0.065)	(0.077)	(0.132)
No. of applications	63,893			63,473	
Pseudo R-squared	0.026			0.032	
Applicant covariates	Yes	Yes	Yes	Yes	Yes
Loan covariates	Yes	Yes	Yes	Yes	Yes

Notes: This table presents multinomial logit regressions to explain the probability that a loan application was rejected due to the use of various types of applicant information. The base probability is that the application was accepted. Robust standard errors in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively. Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include applicant covariates, loan size and type, and a time-varying night-light measure of local economic activity. Constant not shown. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where market shares are measured in number of branches). Table A1 in the Appendix contains all variable definitions.

TABLE 4. Intensive margin: Information sharing and loan terms

(A) Loan amount

	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	-0.185***	-0.103***	-0.125***			
	(0.019)	(0.028)	(0.025)			
Competition		-0.039	0.028			
		(0.024)	(0.025)			
Credit registry*Competition		-0.146***	-0.127***	-0.124***	-0.103***	-0.136***
		(0.033)	(0.031)	(0.029)	(0.027)	(0.030)
No. of loans	28,240	28,240	28,240	28,240	28,240	28,240
R-squared	0.435	0.439	0.443	0.461	0.461	0.464

(B) Loan maturity

	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	-0.131***	-0.092***	-0.092***			
	(0.012)	(0.017)	(0.017)			
Competition		-0.043**	0.010			
		(0.017)	(0.017)			
Credit registry*Competition		-0.071***	-0.076***	-0.060***	-0.044**	-0.079***
		(0.022)	(0.023)	(0.020)	(0.019)	(0.021)
No. of loans	28,240	28,240	28,240	28,240	28,240	28,240
R-squared	0.332	0.337	0.339	0.356	0.356	0.357

(C) Interest rate

	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	0.685***	0.494***	0.498***			
	(0.061)	(0.087)	(0.087)			
Competition		0.025	-0.207***			
		(0.073)	(0.075)			
Credit registry*Competition		0.343***	0.381***	0.331***	0.409***	0.312***
		(0.120)	(0.123)	(0.124)	(0.115)	(0.121)
No. of loans	28,240	28,240	28,240	28,240	28,240	28,240
R-squared	0.241	0.243	0.247	0.315	0.315	0.315

(D) Collateral

	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	0.320***	0.117***	0.107**			
	(0.041)	(0.044)	(0.043)			
Competition		0.225***	0.345***			
		(0.049)	(0.058)			
Credit registry*Competition		0.357***	0.185***	0.231***	0.204***	0.167**
		(0.065)	(0.068)	(0.052)	(0.048)	(0.067)
No. of loans	28,228	28,228	28,228	28,228	28,228	28,228
R-squared	0.372	0.391	0.084	0.470	0.470	0.219
Month and loan officer fixed effects	No	No	No	Yes	Yes	Yes
Group-specific trend	No	No	No	No	Yes	Yes
Matching: Competition	No	No	Yes	No	No	No
Perceived competition	No	No	No	No	No	Yes

Notes: This table shows OLS regressions at the loan level to estimate the impact of the introduction of the credit registry on loan amount (Panel A); loan maturity (Panel B); interest rate (Panel C) and number of pledged collateral items (Panel D). Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include a time-varying night-light measure of local economic activity. Constant not shown. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index where local market shares are measured in number of branches. Table **??** in the Appendix contains all variable definitions. Sample contains first-time EKI borrowers only. Standard errors are robust and clustered by loan officer. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

TABLE 5. Information sharing: First-time versus repeat loans

Dependent variable →	Loan amount	Loan maturity	Interest rate	Collateral
	[1]	[2]	[3]	[4]
Credit registry	-0.251***	-0.191***	1.036***	0.621***
	(0.021)	(0.016)	(0.077)	(0.038)
Credit registry*Competition	-0.073***	-0.041***	-0.008	0.271***
	(0.013)	(0.010)	(0.051)	(0.028)
2^{nd} loan	0.345***	0.284***	-1.045***	0.028
	(0.013)	(0.010)	(0.049)	(0.026)
3^{rd} loan	0.583***	0.475***	-1.720***	0.043
	(0.023)	(0.018)	(0.081)	(0.044)
4^{th} loan	0.774***	0.662***	-2.346***	0.076
	(0.034)	(0.028)	(0.118)	(0.061)
2^{nd} loan*Credit registry	0.051**	0.026	-0.115	-0.494***
	(0.021)	(0.016)	(0.076)	(0.037)
3^{rd} loan*Credit registry	0.104***	0.070***	-0.411***	-0.576***
	(0.026)	(0.021)	(0.097)	(0.049)
4^{th} loan*Credit registry	0.120***	0.070***	-0.348***	-0.564***
	(0.033)	(0.027)	(0.117)	(0.058)
Branch covariates	Yes	Yes	Yes	Yes
Client fixed effects	Yes	Yes	Yes	Yes
No. of loans	81,883	81,883	81,883	81,883
R-squared	0.317	0.303	0.121	0.303

Notes: This table shows client fixed effect OLS regressions to estimate the impact of the introduction of the credit registry and of credit history on the (log) loan amount [1]; the (log) loan maturity [2]; interest rate [3] and total number of collateral contracts [4] across branches that experience varying degrees of credit market competition. Robust standard errors in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively. Sample only includes repeat clients. Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include a time-varying night-light measure of local economic activity and control dummies for product type. Constant not shown. Local competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Table **??** in the Appendix contains all variable definitions.

TABLE 6. Information sharing and loan quality: Hazard analysis

Functional form →	Cox				Exponential	Weibull
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	-0.860***	-0.577***	-0.507***	-0.486***	-0.856***	-0.532***
	(0.127)	(0.078)	(0.091)	(0.083)	(0.105)	(0.080)
Competition	-0.230	-0.175	-0.178	-0.188	-0.035	-0.182
	(0.169)	(0.170)	(0.171)	(0.186)	(0.141)	(0.178)
Credit registry*Comp.	-0.467***	-0.511***	-0.556***	-0.496***	-0.739***	-0.501***
	(0.170)	(0.117)	(0.126)	(0.126)	(0.151)	(0.121)
Borrower education	-0.224***	-0.263***	-0.253***	-0.270***	-0.250***	-0.267***
	(0.052)	(0.038)	(0.041)	(0.042)	(0.042)	(0.040)
Borrower age	-0.014***	-0.014***	-0.014***	-0.015***	-0.014***	-0.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Borrower male	0.075**	-0.001	-0.008	-0.016	-0.020	-0.009
	(0.030)	(0.021)	(0.021)	(0.023)	(0.025)	(0.022)
Urban borrower	-0.009	0.025	0.033	0.028	0.048	0.025
	(0.046)	(0.040)	(0.045)	(0.044)	(0.042)	(0.042)
Stable income	-0.135***	-0.044	-0.079	-0.013	0.034	-0.028
	(0.051)	(0.051)	(0.050)	(0.054)	(0.083)	(0.053)
Interest rate	0.035***	0.028***	0.024***	0.040***	-0.002	0.034***
	(0.009)	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
Loan maturity	0.015***	0.023***	0.023***	0.005*	0.028***	0.015***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Loan/income ratio	0.042***	0.025***	0.032***	0.027***	0.028***	0.026***
	(0.009)	(0.007)	(0.008)	(0.008)	(0.009)	(0.007)
First loan		-0.012	-0.009	-0.057*	0.042	-0.036
		(0.030)	(0.033)	(0.033)	(0.036)	(0.031)
Credit registry*First loan		-0.201**	-0.230**	-0.201**	-0.197	-0.198**
		(0.096)	(0.107)	(0.101)	(0.144)	(0.099)
Ln(Alpha)						-0.645***
						(0.023)
No. of loans	101,883	185,934	162,746	185,934	185,934	185,934
LiTS controls	No	No	Yes	No	No	No
Branch stratification	Yes	Yes	No	No	Yes	Yes
Loan sample	First	All	All	All	All	All
Log-likelihood ratio	-45,728	-92,204	-102,917	-119,697	-52,650	-49,605
Proportionality	Yes	Yes	Yes	No	na	na

Notes: This table shows the results of Cox proportional hazard models in column [1] to [3], a Cox non-proportional hazard model in [4], a parametric exponential hazard model in [5] and a parametric Weibull hazard model in [6]. The dependent variable is the hazard rate, the probability that a loan i is defaulted on in a given month t given that default did not occur earlier. A default event occurs when a borrower is at least 30 days late in making a payment and the loan was eventually written off. Sample period: June 2002-December 2010. We restrict the sample to first-time borrowers in column [1]. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given month, zero otherwise. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). In column [4] we relax the proportionality assumption and allow for time-varying coefficients. All specifications include a time-varying night-light measure of local economic activity and controls for collateral use. Robust standard errors are clustered by loan officer and appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% significance level, respectively. Table **??** in the Appendix contains all variable definitions.

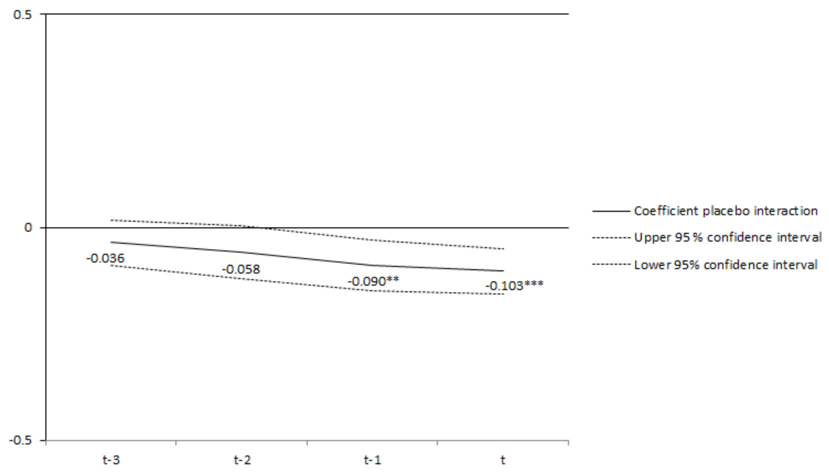
TABLE 7. Information sharing and return on loans

	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry	0.035***	0.029***				
	(0.003)	(0.004)				
Competition		0.004				
		(0.006)				
Credit registry*Competition		0.010	0.012***	0.010**	0.012***	0.013***
		(0.007)	(0.005)	(0.005)	(0.003)	(0.003)
No. of loans	28,194	28,194	28,194	28,194	56,787	56,787
R-squared	0.058	0.059	0.181	0.181	0.030	0.030
Month fixed effects	No	No	Yes	Yes	Yes	Yes
Group-specific trend	No	No	No	Yes	No	Yes
Loan officer fixed effects	No	No	Yes	Yes	No	No
Client fixed effects	No	No	No	No	Yes	Yes

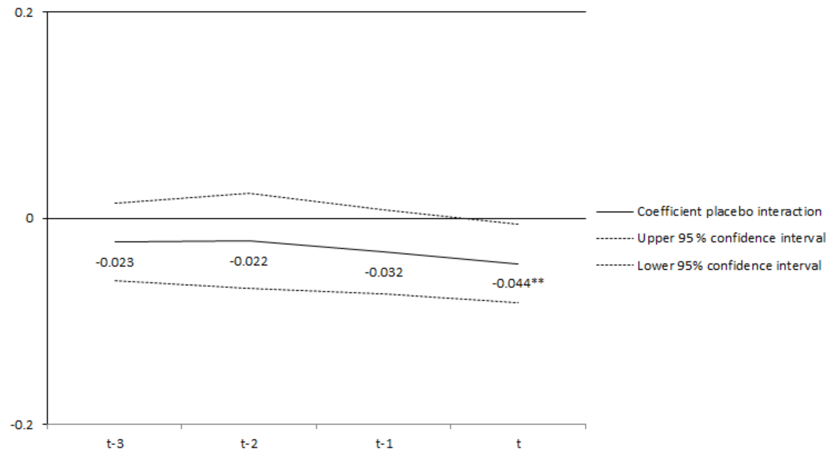
Notes: This table shows loan-level OLS regressions to estimate the impact of the introduction of the credit registry on the return on loans. Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include a time-varying night-light measure of local economic activity. Constant not shown. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index where local market shares are measured in number of branches. Table **??** in the Appendix contains all variable definitions. Sample contains first-time EKI borrowers in columns [1] to [4] and all loans in columns [5] and [6]. Standard errors are robust and clustered by loan officer. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Figure 1. Intensive margin: Placebo tests

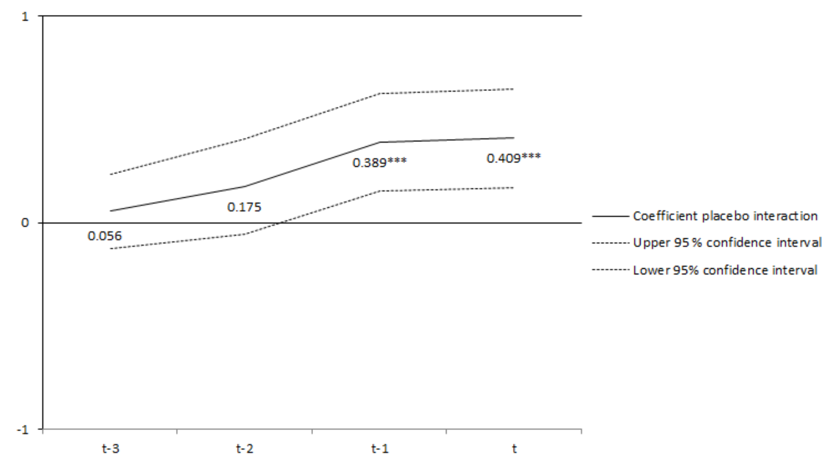
(A) Loan amount



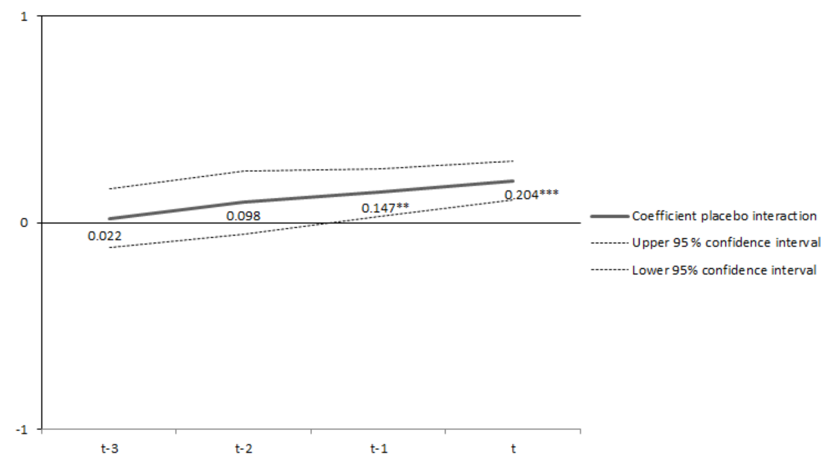
(B) Loan maturity



(C) Interest rate



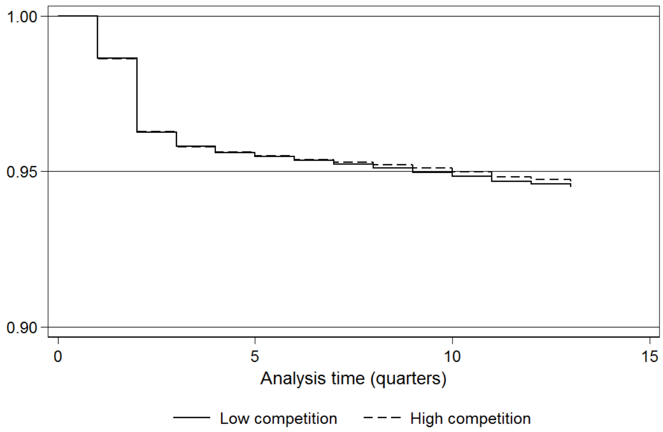
(D) Collateral



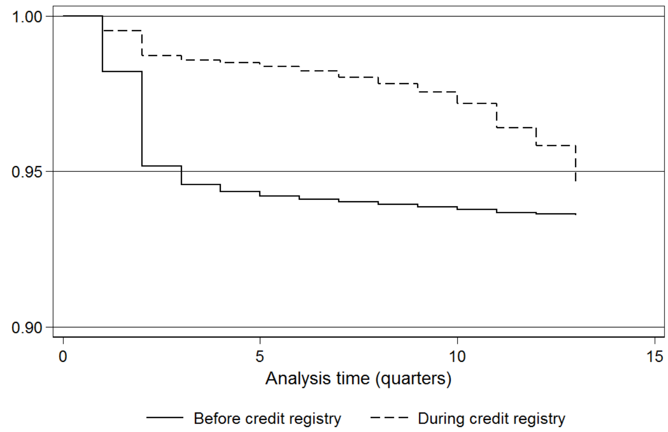
Notes: This graph shows the coefficient estimates (and a 95% confidence interval) for the interaction term *Creditregistry*Competition* as used in column 5 of Table **??**. The value at t shows the coefficient when using the actual timing of the credit registry introduction. The values at $t-1$, $t-2$, etc. show the coefficient estimates when introducing the credit registry 1 quarter, 2 quarters, etc. earlier than the real introduction date.

Figure 2. Information sharing and loan quality: Kaplan-Meier survival analysis

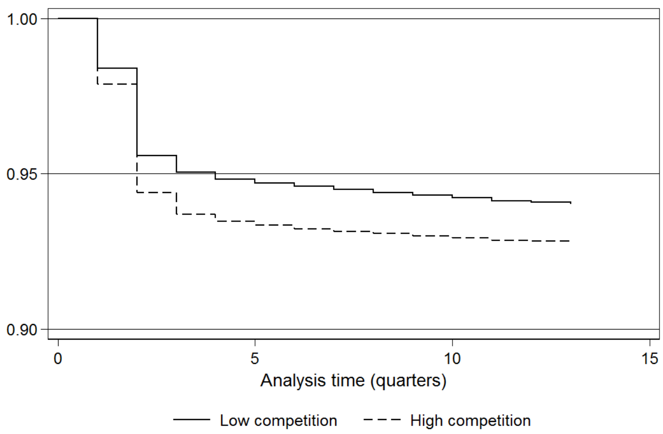
(a) High versus low competition branches



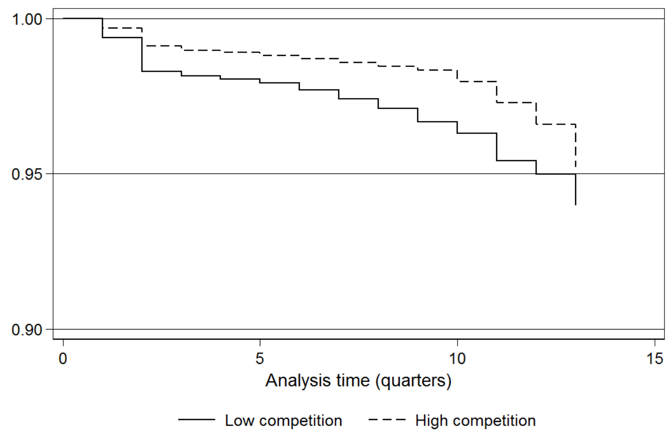
(b) Before versus during credit registry



(c) High versus low competition before credit registry



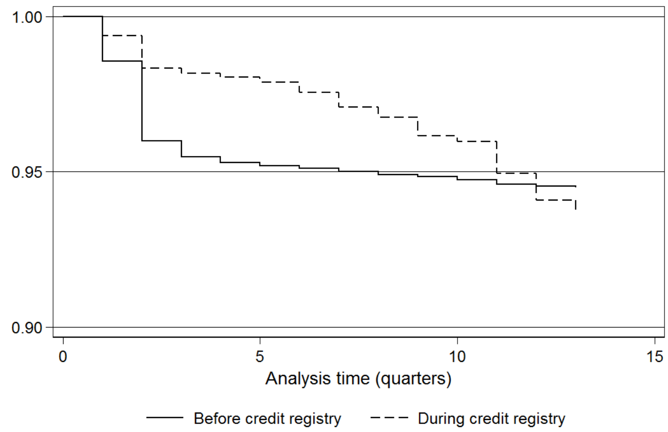
(d) High versus low competition during credit registry



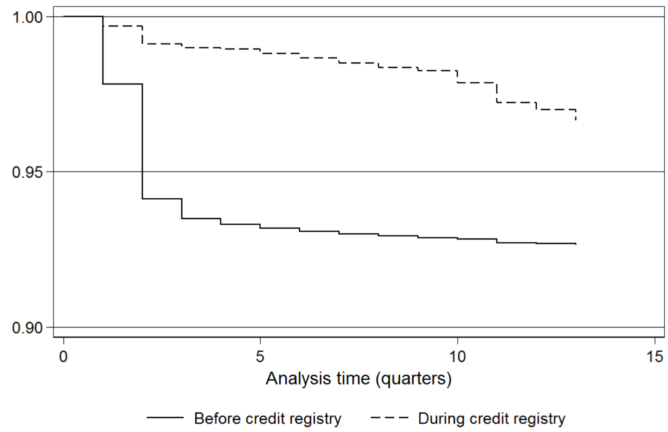
Notes: These four graphs show Kaplan-Meier survival estimates over the sample period June 2002-December 2012. Logrank test statistics for differences between the curves:
Panel A: $\chi^2(1) = 0.27$ (*p-value*= 0.60). Panel B: $\chi^2(1) = 1667.53$ (*p-value*= 0.00).
Panel C: $\chi^2(1) = 113.72$ (*p-value*= 0.00); Panel D: $\chi^2(1) = 106.89$ (*p-value*= 0.00).

Figure 3. Information sharing and loan quality: First-time versus repeat borrowers (Kaplan-Meier survival analysis)

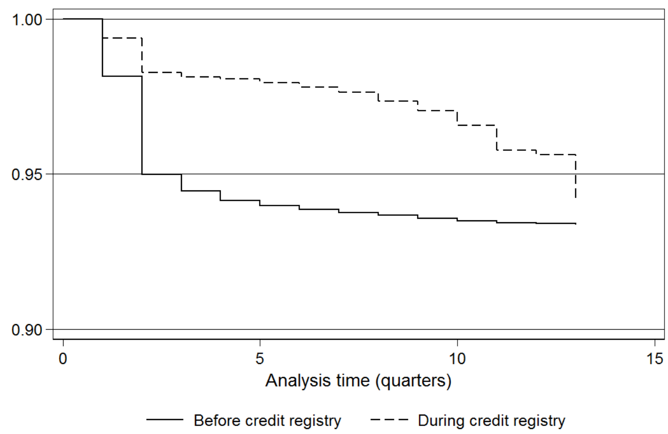
(a) Low competition, new borrowers



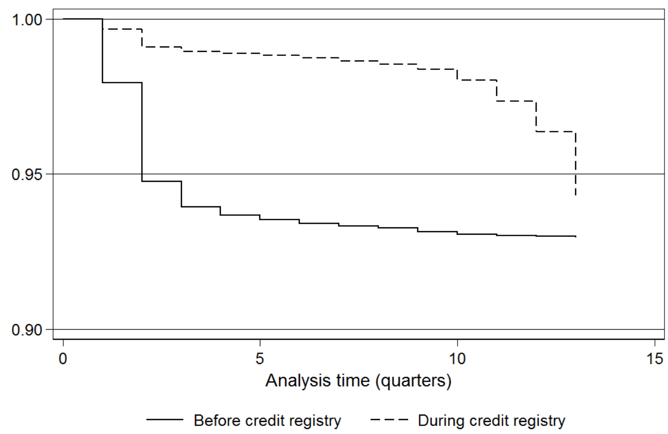
(b) High competition, new borrowers



(c) Low competition, repeat borrowers



(d) High competition, repeat borrowers

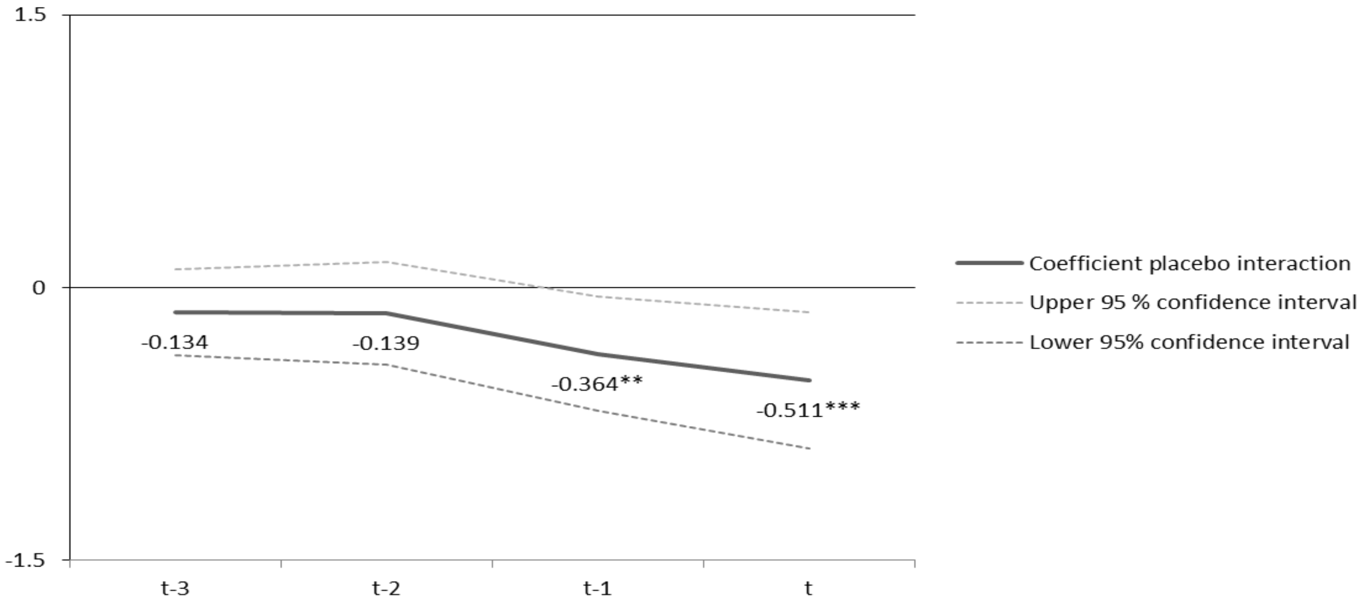


Notes: These four graphs show Kaplan-Meier survival estimates over the sample period June 2002-December 2012. Logrank test statistics for differences between the curves:

Panel A: $\chi^2(1) = 723.77$ (*p-value*$= 0.00$). Panel B: $\chi^2(1) = 392.57$ (*p-value*$= 0.00$).

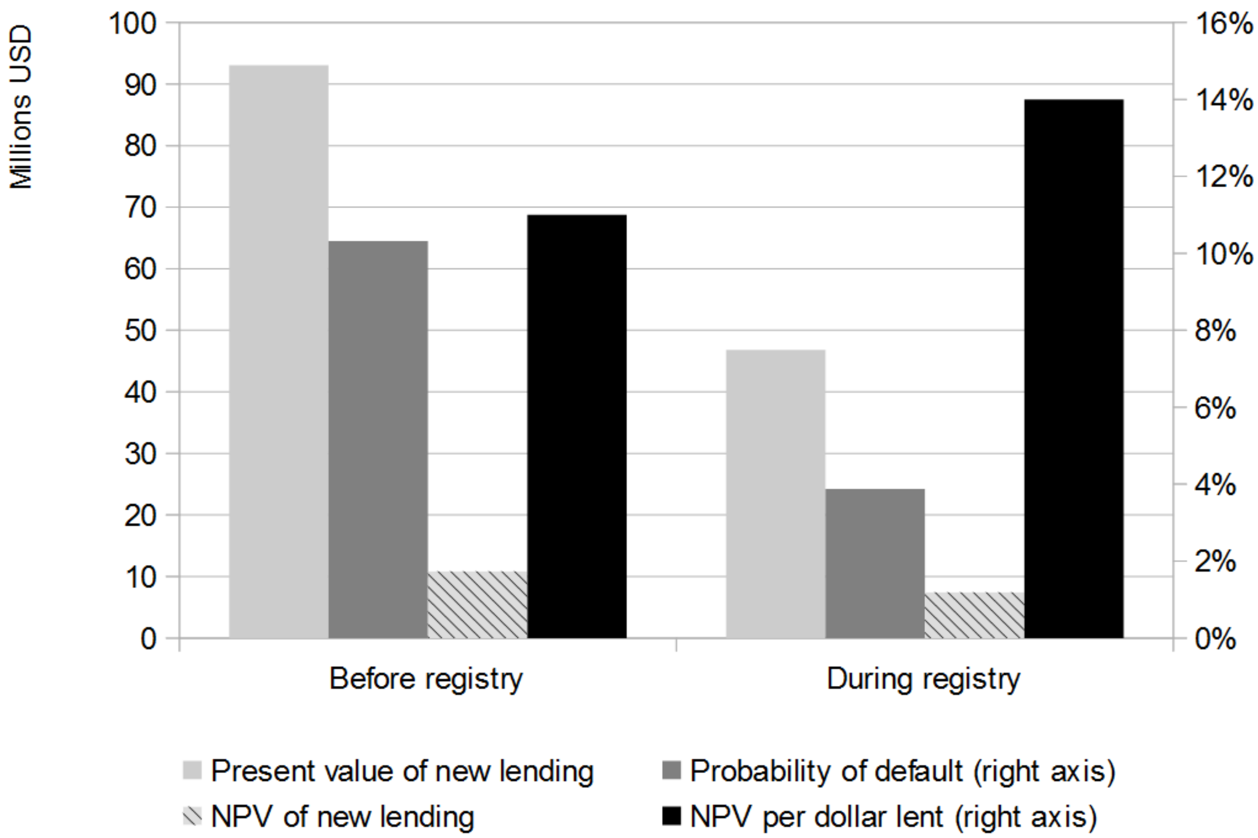
Panel C: $\chi^2(1) = 630.53$ (*p-value*$= 0.00$). Panel D: $\chi^2(1) = 130.52$ (*p-value*$= 0.00$).

FIGURE 4. Cox proportional hazard model: Placebo test



Notes: This graph shows coefficient estimates (and a 95% confidence interval) for the interaction term *Creditregistry*Competition* as used in column 2 of Table **??**. The value at t shows the coefficient when using the actual timing of the credit registry introduction. The values at $t-1$, $t-2$, etc. show the coefficient estimates when introducing the registry 1 quarter, 2 quarters, etc. earlier than the real introduction date.

FIGURE 5. Information sharing and aggregate lending profitability



Notes: This figure compares the portfolio of all loans disbursed in the year before (June 2008-June 2009, left) and after (July 2009-July 2010, right) the introduction of the credit registry. The present value of new lending (left axis) is the total amount of new lending, net of fees, disbursed in the year before (after) the credit-registry introduction. The present value of these amounts is calculated by discounting back to the beginning of each period, using the average funding cost of the lender, and is shown as a positive number. The probability of default is the probability that repayment on a loan is at least 30 days late and that eventually part of the loan is written off (right axis). The NPV is the net present value of new lending in the year before (after) the credit-registry introduction (left axis). The net present value is calculated as the present value of all loan repayments and interest payments minus the initial loan disbursements (net of fees). This amount is discounted back to the beginning of each period using the average funding cost of the lender. The NPV per dollar lent is the net present value divided by the present value of total lending in each of the two periods (right axis).

Appendix

Table A1. Variable definitions and data sources: Extensive margin

Dependent variables:	Definition	Source	Unit
Loan rejected	Dummy=1 if loan application is rejected.	EKI	Dummy
Loan rejected: Hard information	Dummy=1 if loan application is rejected because of borrower age, low credit score in the registry, too many outstanding loans elsewhere, previous late or non-repayment with EKI, bad financial ratios or insufficient collateral.	EKI	Dummy
Loan rejected: Soft information	Dummy=1 if loan application is rejected because of a bad recommendation from someone else, because the purpose of the loan was unclear, or because the loan officer had doubts about certain character traits of the applicant.	EKI	Dummy
Loan rejected: Private information	Dummy=1 if loan application is rejected because of information that the lender has in its own systems or has collected itself: information on financial ratios of the borrower, the purpose of the loan; the character of the client, or the available collateral.	EKI	Dummy
Loan rejected: Public information (negative)	Dummy=1 if loan application is rejected because of a low credit score in the registry.	EKI	Dummy
Loan rejected: Public information (positive)	Dummy=1 if loan application is rejected because of too many outstanding loans with competing lenders.	EKI	Dummy

Notes: BAM is Bosnian Convertible Mark.

Table A2. Variable definitions and data sources: Intensive margin

Dependent variables:	Definition	Source	Unit
Loan amount	Loan amount at time of disbursement.	EKI	BAM
Loan maturity	Maturity of the loan at time of disbursement.	EKI	Months
Interest rate	Annual nominal interest rate on the loan.	EKI	%
Collateral	Total number of collateral items pledged.	EKI	Discrete
Problem loan	Dummy=1 if borrower was at any time at least 30 days late in making a payment and the loan was subsequently written off.	EKI	Dummy
Return on loan	Measure of loan profitability taking into account loss given default.	EKI	%
Independent variables:			
Credit registry	Dummy=1 for all quarters after and including July 2009 (time of CRK introduction); 0 otherwise.	Central Bank of Bosnia	Dummy
Competition: 1-HHI	1 minus HHI index. The (time-varying) HHI index ranges between [0, 1] and measures microcredit market concentration in the locality where an EKI branch is based. Market shares are expressed as number of branches.	BEPS, MIX, Annual reports	[0, 1]
Perceived competition	Competition intensity as perceived by the two most senior loan officers in each branch. Average score on a 7-point Likert scale to the question: Over the past ten years, I think that other microcredit providers have increased their competitiveness in my area.	Loan officer survey	0.5 increments
Lender's market share	Market share of EKI, expressed in number of branches and offices, in a locality (city or town). Measured at the introduction of the credit registry.	EKI	Ratio
Loan/income ratio	Loan amount at time of disbursement divided by monthly borrower income. Income includes primary plus secondary income.	EKI	Ratio
Borrower age	Borrower age.	EKI	Years
Borrower male	Dummy= 1 if borrower is male; 0 otherwise.	EKI	Dummy
Borrower education	1 = None, 2 = Primary, 3 = Secondary, 4 = Tertiary (College/University/Post Graduate).	EKI	1 to 4
Borrower income	Total annual borrower income (primary plus secondary income source).	EKI	BAM
Urban borrower	0 = Rural; 1 = Urban.	EKI	Dummy
Stable income	0 = unemployed or casually employed; 1 = stable employment (agricultural producer; full-time employed; own business; part-time employed) or pension.	EKI	Dummy
Loan immovable	Loan purpose = Purchase immovable assets (land and/or buildings).	EKI	Dummy
Loan movable	Loan purpose = Purchase movable assets (equipment, fixed assets, vehicles).	EKI	Dummy
Loan stock	Loan purpose = Purchase of stock (merchandise, raw material, working capital, agricultural inputs, livestock for reproduction, seedlings for orchards).	EKI	Dummy
Loan household	Loan purpose = Private (non-business related) expenses for the household.	EKI	Dummy
Personal collateral	Number of personal collateral pledges for each loan (includes mortgages, administrative bans on the borrower's salary, and pledges of movable assets).	EKI	Discrete
Social collateral	Number of social collateral pledges for each loan (includes total and partial guarantees provided by family and friends of the borrower).	EKI	Discrete
Third-party collateral	Number of third party collateral pledges for each loan (includes checks or bills of exchange issued by a guarantor company).	EKI	Discrete
Stock index	Bosnia Investment Index (May 28th 2002=1).	Sarajevo Stock Exchange	Index
Local GDP	Time varying measure of local economic activity as proxied by the night-light intensity (derived from satellite images) in the locality where an EKI branch is based. Scale ranges from 0 to 63 where higher values indicate higher light intensity.	National Geophysical Datacenter; Henderson et al. (2011)	[0, 63]
Loans/officer	Monthly number of loans per loan officer.	EKI	Loans
Branch growth	Quarterly growth in total new lending volume (flow) per branch.	EKI	%

Notes: BAM is Bosnian Convertible Marka. BEPS is the EBRD Banking Environment and Performance Survey. MIX: www.mixmarket.org/.

Table A3. Extensive margin: Robustness and placebo tests

	Robustness tests			Placebo tests		
	Narrow window	Broad window	Broadest window	Post is pre	Pre is post	Random assignment
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	0.030***	0.047***	0.054***	0.006	0.007	0.000
	(0.011)	(0.007)	(0.006)	(0.008)	(0.006)	(0.001)
Applicant covariates	Yes	Yes	Yes	Yes	Yes	Yes
Loan covariates	Yes	Yes	Yes	Yes	Yes	Yes
No. of applications	29,829	96,215	183,066	54,022	79,769	69,427
Adjusted R-squared	0.037	0.045	0.046	0.040	0.052	0.041

Notes: Columns [1], [2] and [3] show robustness tests of our main results as reported in Table **??**. In columns [1] we use a shorter time window February 2009-February 2010. In column [2] the window is May 2008-December 2010. In column [3] we use the largest possible window January 2007-December 2012. Columns [4], [5] and [6] show placebo tests of our main results as reported in Table **??**. In columns [4] and [5] we move the two-year window one year forward and backward, respectively. In column [6], we randomly allocate branches to either high or low competition status. We repeat this random allocation a thousand times and show the average result. The treatment period starts in July 2009. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given month, zero otherwise. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Dummies for the introduction of the credit registry and for high competition are included but not shown. Robust standard errors are clustered by loan officer and appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Table A1 in the Appendix contains all variable definitions. The same borrower and loan covariates as in Table **??** are included but not shown.

Table A4. Intensive margin: Placebo tests

Dependent variable →	Loan amount			Loan maturity		
	Post is pre	Pre is post	Random allocation	Post is pre	Pre is post	Random allocation
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	0.010	-0.046	0.000	-0.007	-0.022	0.000
	(0.040)	(0.030)	(0.000)	(0.034)	(0.020)	(0.000)
Borrower covariates	Yes	Yes	Yes	Yes	Yes	Yes
Local econ. control	Yes	Yes	Yes	Yes	Yes	Yes
No. of loans	12,627	38,407	28,240	12,626	38,407	28,240
R-squared	0.380	0.459	0.007	0.247	0.357	0.006

Dependent variable →	Interest rate			Collateral		
	Post is pre	Pre is post	Random allocation	Post is pre	Pre is post	Random allocation
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	-0.093	0.028	0.000	0.105	0.024	0.000
	(0.134)	(0.094)	(0.000)	(0.064)	(0.075)	(0.001)
Borrower covariates	Yes	Yes	Yes	Yes	Yes	Yes
Local econ. control	Yes	Yes	Yes	Yes	Yes	Yes
No. of loans	12,626	38,407	28,240	12,627	38,407	28,240
R-squared	0.194	0.255	0.004	0.449	0.414	0.007

Notes: This table shows loan-level estimates for OLS models where the dependent variables are: loan amount, loan maturity, interest rate and collateral. In columns [1] and [4] we show results for a placebo test where we move the two-year window one year forward while in columns [2] and [5] we move the two-year window one year backward. In columns [3] and [6] we randomise the allocation to high and low competition branches over 1,000 trials. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given month, zero otherwise. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Robust standard errors are clustered by loan officer and appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Table **??** in the Appendix contains all variable definitions. The same borrower and loan covariates as in Table **??** are included but not shown.

Table A5. Intensive margin: Robustness tests

Dependent variable →	**Loan amount**			**Loan maturity**		
	Narrow window	Broad window	Broadest window	Narrow window	Broad window	Broadest window
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	-0.133***	-0.132***	-0.101***	-0.063**	-0.080***	-0.048***
	(0.042)	(0.031)	(0.027)	(0.022)	(0.033)	(0.019)
Borrower covariates	Yes	Yes	Yes	Yes	Yes	Yes
Local econ. control	Yes	Yes	No	Yes	Yes	No
No. of loans	11,842	33,965	88,623	11,842	33,965	88,623
Adjusted R-squared	0.452	0.447	0.391	0.333	0.340	0.271

Dependent variable →	**Interest rate**			**Collateral**		
	Narrow window	Broad window	Broadest window	Narrow window	Broad window	Broadest window
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	0.492***	0.358***	0.146	0.223***	0.236***	0.237***
	(0.123)	(0.114)	(0.094)	(0.064)	(0.060)	(0.073)
Borrower covariates	Yes	Yes	Yes	Yes	Yes	Yes
Local econ. control	Yes	Yes	No	Yes	Yes	No
No. of loans	11,842	33,965	88,623	11,842	33,965	88,623
Adjusted R-squared	0.231	0.268	0.266	0.451	0.431	0.382

Notes: This table shows robustness tests of our main results as reported in Table **??**. In columns [1] and [4] we use a shorter time window February 2009-February 2010. In columns [2] and [5] the window is May 2008-December 2010. In columns [3] and [6] we use the widest possible window May 2006-December 2012. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given month; '0' otherwise. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Robust standard errors are clustered by loan officer and appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Table **??** in the Appendix contains all variable definitions. The same borrower and loan covariates as in Table **??** are included but not shown.

Table A6. Lender's market share, information sharing and lending outcomes

	Extensive margin	Intensive margin				Loan quality
	Loan rejection	Loan amount	Loan maturity	Interest rate	Collateral	Hazard rate
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*(1-Market share)	0.164***	-0.197*	-0.117	1.470***	0.343**	-0.594
	(0.021)	(0.104)	(0.080)	(0.447)	(0.180)	(0.454)
Applicant covariates	Yes	Yes	Yes	Yes	Yes	Yes
Loan covariates	Yes	Yes	Yes	Yes	Yes	Yes
No. of loans	65,102	28,240	28,240	28,240	28,240	185,934
Adjusted R-squared	0.042	0.460	0.355	0.275	0.438	na

Notes: This table shows our baseline results where the variable *Competition* is replaced with *Marketshare*. In column [1] we run the same model as in Table **??**, column 5. In columns [2] to [5] we run the same models as in Table **??**, column 4. In column [6] we run the same model as in Table **??**, column 2. The treatment period starts in July 2009. *Creditregistry* is a dummy variable that is '1' if the credit registry was in place in a given month, zero otherwise. Market share: market share of EKI, expressed in number of branches and offices, in a locality (city or town). A dummy for the introduction of the credit registry and market share variable are included but not shown. Robust standard errors appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Tables A1 and A2 in the Appendix contain all variable definitions. Depending on the model, the same borrower and loan covariates as in Table **??**, Table **??** and Table **??** are included but not shown.

Table A7. Information sharing, competition and repeat borrowers

Dependent variable →	Loan amount	Loan maturity	Interest rate	Collateral
	[1]	[2]	[3]	[4]
Credit registry	-0.207***	-0.178***	0.747***	0.380***
	(0.023)	(0.018)	(0.085)	(0.044)
Credit registry*Competition	-0.153***	-0.064***	0.511***	0.474***
	(0.021)	(0.016)	(0.082)	(0.044)
2^{nd} loan	0.362***	0.308***	-1.177***	0.386***
	(0.017)	(0.013)	(0.063)	(0.037)
3^{rd} loan	0.623***	0.515***	-1.862***	0.654***
	(0.028)	(0.022)	(0.099)	(0.059)
4^{th} loan	0.839***	0.735***	-2.509***	0.946***
	(0.041)	(0.034)	(0.143)	(0.083)
2^{nd} loan*Competition	-0.031*	-0.045***	0.238***	0.137***
	(0.019)	(0.015)	(0.070)	(0.042)
3^{rd} loan*Competition	-0.075***	-0.070***	0.242**	0.215***
	(0.028)	(0.022)	(0.103)	(0.062)
4^{th} loan*Competition	-0.116***	-0.129***	0.268*	0.189**
	(0.040)	(0.034)	(0.148)	(0.086)
2^{nd} loan*Credit registry	-0.006	0.010	0.256**	-0.327***
	(0.028)	(0.022)	(0.103)	(0.055)
3^{rd} loan*Credit registry	0.012	0.026	0.086	-0.427***
	(0.035)	(0.028)	(0.130)	(0.073)
4^{th} loan*Credit registry	0.010	0.000	0.163	-0.417***
	(0.047)	(0.037)	(0.160)	(0.090)
2^{nd} loan*Registry*Comp.	0.095***	0.031	-0.618***	-0.412***
	(0.030)	(0.023)	(0.110)	(0.062)
3^{rd} loan*Registry*Comp.	0.153***	0.075**	-0.794***	-0.339***
	(0.039)	(0.030)	(0.145)	(0.083)
4^{th} loan*Registry*Comp.	0.187***	0.123***	-0.826***	-0.298***
	(0.052)	(0.042)	(0.186)	(0.108)
Branch controls	Yes	Yes	Yes	Yes
Client fixed effects	Yes	Yes	Yes	Yes
No. of loans	81,883	81,883	81,883	81,883
R-squared	0.318	0.305	0.124	0.096

Notes: This table shows client fixed effect OLS regressions to estimate the impact of the introduction of the Bosnian credit registry and credit history on log of loan amount granted [1]; log of loan maturity granted [2]; interest rate [3] and total number of collateral contracts [4] across branches that experience varying degrees of credit market competition. Robust standard errors in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively. Sample only includes repeat clients. Before credit registry: June 2008-June 2009. During credit registry: July 2009-July 2010. All specifications include a time-varying night-light measure of local economic activity and control dummies for product type. Constant not shown. Local competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Table **??** in the Appendix contains all variable definitions.

Table A8. Information sharing and loan quality: Hazard model extensions and alternative specifications

Functional form	Cox proportional		Exponential		Weibull	
Time structure	Time-varying predictors					
	[1]	[2]	[3]	[4]	[5]	[6]
Credit registry*Competition	-0.301**	-0.247**	-0.268*	-0.203**	-0.264*	-0.200*
	(0.153)	(0.101)	(0.156)	(0.102)	(0.154)	(0.102)
Credit registry	-1.332***	-1.230***	-0.789***	-0.732***	-0.922***	-0.856***
	(0.115)	(0.080)	(0.118)	(0.083)	(0.117)	(0.082)
Competition	-0.056*	-0.102***	-0.049	-0.096***	-0.069**	-0.119***
	(0.030)	(0.022)	(0.034)	(0.025)	(0.033)	(0.024)
First loan		0.683***		0.665***		0.684***
		(0.022)		(0.024)		(0.024)
Credit registry*First loan		-0.329***		-0.219***		-0.244***
		(0.076)		(0.081)		(0.080)
Alpha					0.624***	0.633***
					(0.009)	(0.007)
No. of obs.	356,131	1,119,122	356,131	1,119,122	356,131	1,119,122
Log-likelihood ratio	-49,419	-101,919	-20,653	-41,799	-20,115	-40,842

Notes: This table shows the results of (semi-)parametric hazard models. The dependent variable is the hazard rate: the probability that a loan i is defaulted on in month t given that default did not occur earlier. A default event occurs when a borrower is at least 30 days late in making a payment and the loan was eventually written off. The same controls as in Table **??** and a constant are included but not shown. Sample period: June 2002-December 2010. We restrict the sample to new customers in columns [1], [3], and [5]. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given quarter; '0' otherwise. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index where local market shares are measured in number of branches. Robust standard errors in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Table **??** in the Appendix contains all variable definitions.

Table A9. Information sharing and loan quality: Robustness tests

Local factor →	Mean food spending	Percentage owns a computer	Percentage bank account	Percentage risk takers	Percentage employed	Percentage orthodox	Cantonal unemployment	Cantonal GDP
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Credit registry*	-0.644***	-0.517**	-0.450**	-0.524***	-0.556***	-0.602***	-0.524***	-0.431**
Competition	(0.201)	(0.220)	(0.197)	(0.200)	(0.209)	(0.194)	(0.184)	(0.187)
Credit registry*	0.002	-0.001	-0.003	0.009	0.001	-0.002	0.799	-0.000
Local factor	(0.002)	(0.004)	(0.005)	(0.006)	(0.006)	(0.002)	(1.226)	(0.000)
Credit registry	-1.575**	-0.984***	-0.892***	-1.554***	-1.038***	-0.970***	-1.502***	-1.089***
	(0.699)	(0.208)	(0.274)	(0.335)	(0.249)	(0.174)	(0.527)	(0.315)
Local factor	0.000	-0.001	-0.003	0.003	-0.005	-0.005**	1.341	0.000
	(0.001)	(0.003)	(0.004)	(0.004)	(0.005)	(0.002)	(0.932)	(0.000)
Borrower covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	104,732	104,732	104,732	104,732	104,732	104,732	100,505	84,914
No. of branches	14	14	14	14	14	14	14	14
Log-Likelihood ratio	-53,770	-53,771	-53,762	-53,757	-53,756	-53,681	-57,727	-55,026

Notes: This table shows the results of a Cox proportional hazard model where the dependent variable is the hazard rate, the probability that a loan i is defaulted on in month t given that default did not occur earlier. A default event occurs when a borrower is at least 30 days late in making a payment and the loan was eventually written off. Sample period: June 2002-December 2010. We restrict the sample to first-time borrowers. Credit registry is a dummy variable that is '1' if the credit registry was in place in a given month; '0' otherwise. Local factor: in columns [1] to [6] dummy from Life in Transition Survey that is '1' if local factor is above median level, in columns [7] and [8] dummy from the Agency of Statistics of Bosnia-Herzegovina is '1' if local factor is above median level. Competition: Dummy variable that is '1' if local credit market competition is above the median level of competition as measured by 1 minus the HHI index (where local market shares are measured in number of branches). Local factors are measured as the mean monthly food spending of households in a locality; the percentage of households that own a computer; the percentage of households that have a bank account; the percentage of households that can be classified as risk takers based on LiTS II; the percentage of household heads that are employed; the percentage of orthodox Christian households; the unemployment rate in the canton and the cantonal GDP. Robust standard errors are clustered by loan officer and appear in parentheses. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Table A1 in the Appendix contains all variable definitions. The same borrower and loan covariates as in Table **??** are included but not shown.

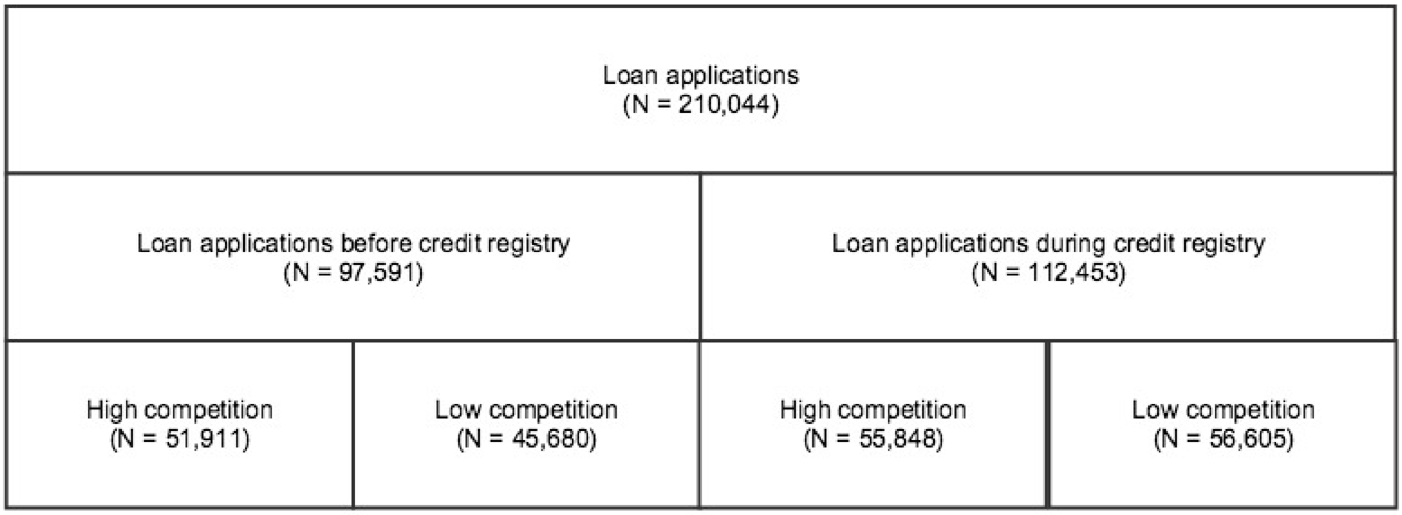
Table A10. High vs. low competition areas: Means comparisons

	Low competition areas	High competition areas
	[1]	[2]
Panel A: *Socio-economic characteristics*		
Food spending	349.72	388.67
Percentage owns a computer	30.47	61.30**
Percentage bank account	42.99	54.30
Percentage risk takers	60.30	57.31
Percentage employed	28.94	42.59
Percentage orthodox	30.50	28.03
Crisis impact	2.33	2.19
Cantonal unemployment	0.46	0.46
Cantonal GDP	3,189	3,305
Panel B: *Night-light measure of economic activity*		
Level	11.869	10.454*
Before credit registry	10.128	8.894
During credit registry	17.791	15.756
Growth	0.140	0.156
Before credit registry	0.183	0.198
During credit registry	0.008	0.030
Panel C: *Credit market characteristics*		
Loan terms		
Loan amount	3,869	3,939***
Loan maturity	26.95	26.95
Interest rate	20.88	20.81***
Collateral	2.75	3.06***
Loan/income ratio	3.25	3.48***
Borrower quality		
Borrower monthly income	1,267	1,224***
Borrower stable employment	0.920	0.815***

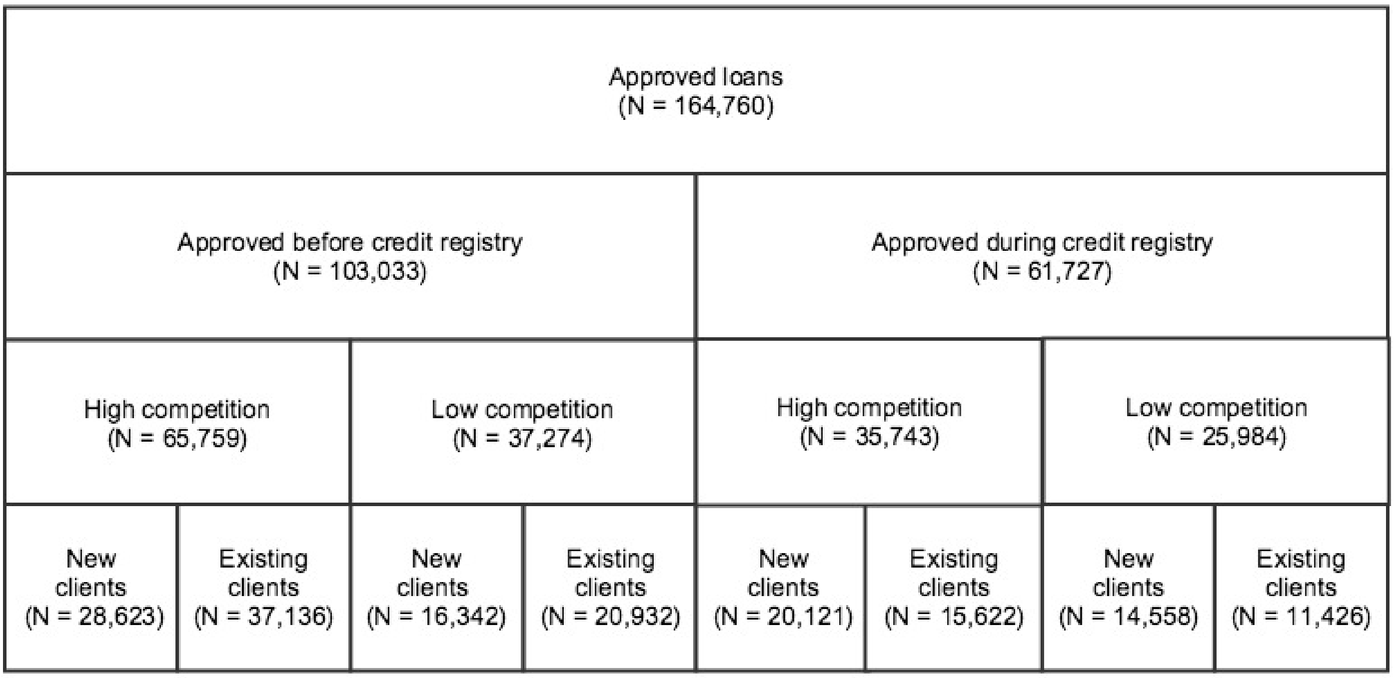
Notes: Panel A: Socio-economic characteristics are measured as the mean monthly food spending of households in a locality; the percentage of households that own a computer; the percentage of households that have a bank account; the percentage of households that can be classified as risk takers based on LiTS II; the percentage of household heads that are employed; the percentage of orthodox Christian households; the unemployment rate in the canton and the cantonal GDP. Source: EBRD-World Bank Life in Transition Survey (2010) and the Agency of Statistics of Bosnia-Herzegovina. Panel B: Sample period is 1992-2013. Source: Geophysical Datacenter. Panel C: The sample contains all loans disbursed between June 2008 and June 2009. Source: EKI. Asterisks refer to p-value of t-test of equality of means. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

FIGURE A1. Data structure

Panel A



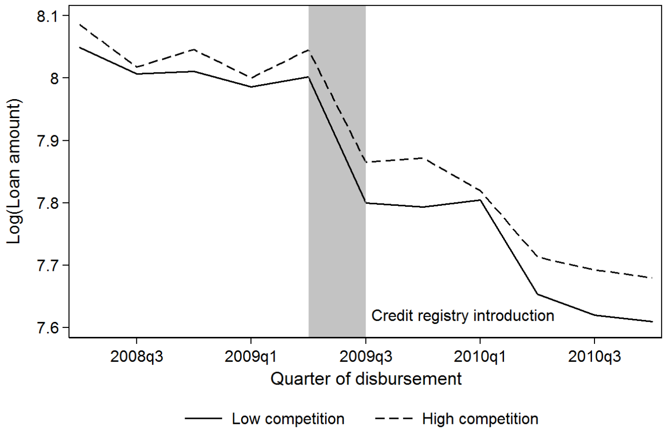
Panel B



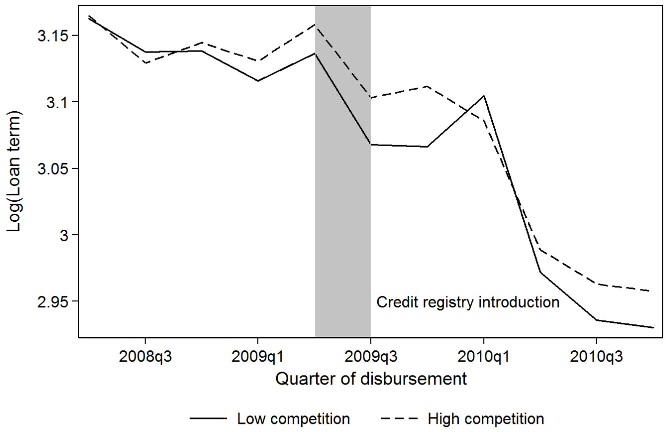
Notes: This figure summarizes the data structure for the overlapping sample of loan applications and loan portfolio (January 2007-December 2012). In the loan performance analysis a longer sample stretching back to June 2002 is used. Out of the total number of applications, 20,627 were withdrawn by the borrower before a decision was taken or the loan disbursed.

Figure A2. Loan terms: Parallel trends in high and low-competition areas

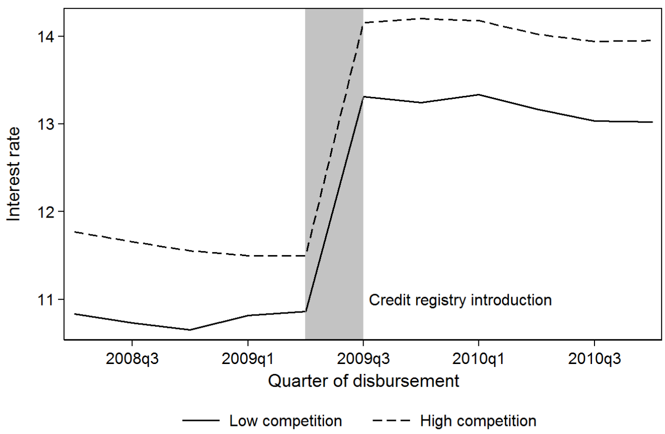
(a) Loan amount



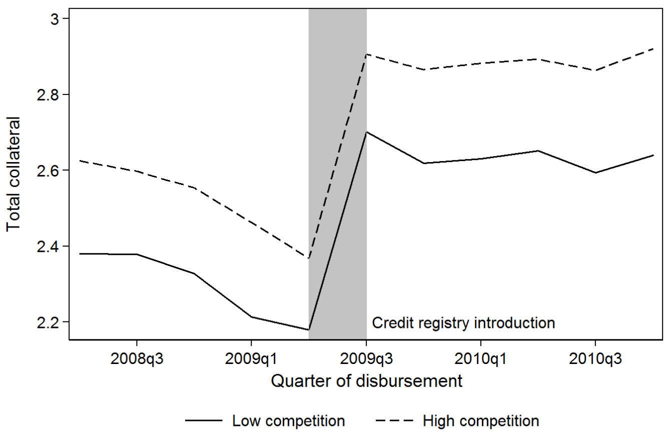
(b) Loan maturity



(c) Interest rate



(d) Collateral



Notes: Conditional trends over the sample period January 2008-December 2010. Loan terms have been regressed on client and loan characteristics. The fitted values from these regressions are shown for high versus low competition areas in the graphs above. The grey areas indicate the quarter in which the credit registry was introduced.