

Till Mortgage Do Us Part: Refinancing Costs and Household's Bank Switching[§]

Brunetti, M.^{**} Ciciretti, R.^{††} Djordjevic, Lj.^{‡‡}

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Abstract

We show that the mortgage refinancing costs, which serve as a “lock-in” for mortgage holders, play an important role in household's decision to switch a bank. To this end, we use a unique household panel dataset that enables us to infer individual bank switching, in conjunction with a legal reform that exogenously slashed down the mortgage refinancing costs. We find that the households responded to this change by increasingly changing their main bank to refinance or prepay an existing mortgage loan (switching *with* a mortgage), as well as to take out a mortgage (switching *for* a mortgage). Dissecting these results, we show that the effects of the reform were not uniform across households. The more educated households, those residing in ex-ante less competitive markets, or in the markets where relationship banking is relatively weaker were at the forefront of the wave of bank switching.

Keywords: bank switching, mortgage refinancing costs, switching costs, natural experiment

JEL Classification: G21; D14

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^{**} Department of Economics and Finance, University of Roma Tor Vergata, CEIS and CeFin Fellow.

E-mail: marianna.brunetti@uniroma2.it

^{††} Department of Economics and Finance, University of Rome Tor Vergata, CEIS and RCEA-Rimini Fellow.

E-mail: rocco.ciciretti@uniroma2.it

^{‡‡} Department of Money and Macroeconomics, Goethe University Frankfurt. E-mail: djordjevic.ljubica@hof.uni-frankfurt.de

1. Introduction

Given the size of a mortgage relative to household's total assets and liabilities in many developed countries (see, e.g., Badarinza et al., 2016), the households should be particularly motivated to keep an eye out for the best mortgage deal. Besides, given that the mortgage loans constitute the bulk of bank loans in developed economies (Beck et al., 2012), sound household finances are among the priorities of policy makers as they entail, among other things, a reduced risk of mortgage default.

This paper empirically investigates the role that (a reduction in) mortgage refinancing costs play in the decision of households to change their main bank, a question largely neglected in the current body of work. On the one hand, the existing empirical literature looking at household's decision to switch a bank typically investigates the role of bank characteristics, with particular attention to its perceived or actual condition of distress (see, e.g., van der Crujisen et al., 2012; Iyer and Puri, 2012; Iyer et al., 2016a and 2016b; Kim, 2016), and/or to households' characteristics (see, e.g., Kiser, 2002, and Brunetti et al., 2016). To the best of our knowledge, there has not been any study to date that relates the household's decision to switch a bank to refinancing costs. On the other hand, the existing studies investigating the role of refinancing costs and/or prepayment penalties have primarily focused on the household's decision to refinance their mortgage (see, e.g., Agarwal et al., 2015; Andersen et al., 2015, and Bajo and Barbi, 2015) or to prepay it (see, e.g. Beltratti et al, 2017), not on the one of switching a bank.

In this paper we study the household's decision to change its main bank, in relation to a market liberalization, which substantially reduced mortgage refinancing costs, the largest chunk of a mortgage holder's cost of switching. In doing so, we look separately at the households already holding a mortgage (i.e., those "switching *with* a mortgage"), and those taking out new mortgages

(i.e., those “switching *for* a mortgage”). We also try to pinpoint which households were the most responsive to such market liberalization measure, in particular focusing on the role of household sophistication, market competition, and relationship banking.

To discipline our empirical analysis of switching *with* a mortgage, we draw the theoretical underpinning from the well-established switching costs theory (see, e.g., Klemperer, 1995). According to this theory, when a rational mortgage holder faces switching costs, as largely reflected in the cost of refinancing a loan, she will not switch to the bank offering her a “better deal” if these costs outweigh the terms differential between the two banks. Thus, the switching costs are detrimental to competition, as the incumbent bank can exploit this situation to maintain unfavorable loan terms (i.e., to extract rents) without concern of losing the client. This “lock-in” effect should however weaken as the refinancing costs decrease. In a nutshell, if the theory holds, the fall of such costs to a sufficiently low level should spark bank switching among the mortgage holders in the presence of better alternatives. To the best of our knowledge, this straightforward theoretical prediction has not been empirically tested yet, in any sector.¹

We rely on a unique dataset and on an empirical strategy that exploits a plausibly exogenous shock to switching costs for mortgages. More specifically, we study a legal change occurred in Italy in early 2007 – aimed at increasing the flexibility and competition in several economic sectors – that, among other things, sharply reduced the refinancing costs for mortgage loans, a bank product that entailed particularly large switching costs a priori. The reform, commonly referred to as the “Bersani Law”, enabled the bank borrowers to prepay the loan at their

¹ Not surprisingly, most of the literature on switching costs is quite general and theoretical in nature (see Klemperer, 1995, and Farrell and Klemperer, 2007, for excellent reviews of the literature). There are several challenges to an empirical investigation of the switching costs, the first one being that they are typically not directly observable (Shy, 2002) and the economists must infer their magnitude from clients’ observed switching behavior (Dubé et al., 2009). Moreover, such analysis requires detailed micro-level data, which are very rarely available (Kim et al., 2003).

current bank or to change their loan provider without requiring authorization from the initial lender and without any (or at significantly reduced) charges. The main cost reduction was introduced by cutting refinancing fees, namely, early termination penalties imposed by the banks, as well as mortgage registration, and notary fees.

As reported e.g. in Beltratti et al. (2017), this reform was largely unexpected, thus providing an ideal *quasi*-natural experiment, which we exploit using a difference-in-differences methodology to compare switching propensities of a treated group with a control group of households. The treated households have a mortgage that is still outstanding around the change in the law. The control group is instead composed of households without a mortgage. In comparing the treated households' switching propensities with those of the control group, we control for a rich set of household and bank observable characteristics. Since the legal change is exogenous to both households and banks, we are able to identify a causal relationship between the reduction in refinancing costs and the hike in switching *with* a mortgage. The second key strength of our empirical strategy stems from a rich, representative household panel survey dataset, provided by the Bank of Italy, that we complement with Bankscope information on banks, which allows us to control for a wide set of household and bank characteristics, as well as for the main features of their relationship. Next, we investigate the prevalence of switching *for* a mortgage, by comparing the switching propensities of household taking out new mortgages before and after the Bersani reform, which we thus treat as a “structural break” in our sample period.

Our main findings can be summarized as follows. First, we document that, before the reform, the households with an outstanding mortgage had a significantly lower propensity to switch a bank than comparable households without a mortgage, while the change in the Law sparked mortgage holders' propensity to switch to a rate higher than among the clients in the

control group. This result suggests that initially the mortgages resulted in a “lock-in”, but the reform has effectively “broken the chains”. Besides this increase in switching *with* a mortgage, we also document that following the reform taking out new mortgages became an important motivation to switch, i.e., the reform also boosted switching *for* a mortgage. The latter evidence supports the notion of Bennett et al. (2001) that the reduction in transaction costs in the US mortgage market (including the costs associated with prepaying the existing loan and obtaining replacement financing) made mortgage origination more competitive. Dissecting these results, our subsample analyses show that the households with more educated household heads and those residing in ex-ante less competitive markets or in markets with weaker relationship banking were at the forefront of the wave of mortgage shopping.

The rest of the paper is organized as follows. Section 2 surveys the related literature; Section 3 describes the Italian mortgage market and the Bersani Law passed in 2007; Section 4 introduces our data and variables; Section 5 outlines our methodological approach; Section 6 presents our results and the associated robustness checks, and Section 7 concludes.

2. Literature Review

Our theoretical motivation draws from the literature which studies impediments to switching in the context of entry deterrence, market power, and rent extraction (in addition to Ausubel, 1991, see, e.g., Klemperer, 1995, for an excellent summary of the pioneering theoretical work, and Barone et al., 2011, for a more recent literature review). Such impediments are primarily reflected in the search costs, i.e., the costs in time and effort to acquire and process information on alternative offers, and in the switching costs, i.e., the costs of switching to an alternative provider. Although the nature of these costs largely varies across different industries, one common

denominator is that, in general, they are very difficult to measure. Kim et al. (2003), for example, infer the significance of switching costs in Norwegian credit markets by analyzing the aggregated data on interest rates and market shares. Brunetti et al. (2016) investigate the determinants of household's bank switching and use various household level proxies for switching costs. In this paper, we investigate the role of impediments to switching by exploiting an exogenous variation in the switching costs occurred in the (Italian) mortgage market.

We also touch upon the literature that investigates the motivation of a firm for switching a bank (Gopalan, 2011), the impact of bank mergers on the small firms' decision to stay, switch, or drop a bank (Degryse et al., 2011), and loan conditions when firms switch banks (Ioannidou and Ongena, 2010). A related literature on relationship banking evolved around the proprietary information that the banks gather about their customers over time (Boot, 2000), which results in a comparative advantage of incumbent bank over its competitors (see, e.g., Berger and Udell, 1995). We show that this mechanism played an offsetting role in our context, as it is only in the provinces with relatively weaker role of relationship banking that the Bersani reform significantly boosted switching *with* a mortgage.

While the above-cited streams of literature are quite extensive, to the best of our knowledge, there is little (if any) empirical evidence on bank switching by households. As Zinman (2014) puts it, "...*household debt is a neglected topic within the relatively neglected sub-field of household finance*". In particular, since the mortgage loans constitute the predominant type of financial liabilities of households (Guiso and Sodini, 2012; Campbell, 2013; ECB, 2013) and bank loans in developed economies (Beck et al., 2012), there is surprisingly little empirical work on the dynamics of mortgage relationships. One recent exception is Brown and Hoffmann (2016) that compares mortgage and non-mortgage relations of Swiss households using survey data, and

documents systematic differences between the two types of relationships. Yet, mortgage-motivated bank switching is largely neglected. The bulk of the literature on retail client switching investigates switching by the depositors (i.e., the banks' creditors), focusing on (perceived) bank distress as a driver of deposit withdrawals (Diamond and Dybvig, 1983 and Iyer et al., 2013). In a recent contribution, using Swiss survey data, Brown et al. (2016) investigate to which extent the switching costs mitigate a run-off from a distressed bank. However, the bank borrowers are expected to be less concerned about the bank's soundness than the bank depositors are, whereas their main motivation for switching a bank are the offered loan terms.

By investigating bank switching *for* and *with* a mortgage across households of different levels of education, our work also relates to the literature on household sophistication. A large body of work documents that poorer education (Campbell, 2006; Calvet et al., 2007 and 2009), financial literacy (see Lusardi and Mitchell, 2014), and cognitive abilities (e.g., Christelis et al., 2010, and Grinblatt et al., 2011) are all positively correlated with sub-optimal financial behavior. The investigated financial decisions range from inefficient saving (Jappelli and Padula, 2013), lack of retirement planning (van Rooij et al., 2012), more costly financial instruments (Hastings and Mitchell, 2011, and Lusardi and Tufano, 2009), and lack of portfolio diversification (Guiso and Jappelli, 2009) to reluctance to hold stocks (van de Rooij et al., 2011), inertia and lack of market timing in stock market participation and trading (Bilias et al., 2010, and Guiso and Viviano, 2015), and sluggish mortgage refinancing. On the latter, for instance, Campbell (2006) reports that one of the most widespread financial mistakes of (US) households is failure to refinance a mortgage when it becomes profitable, reporting that in the late 1990s and early 2000s as much as 14% of American households were paying interest rates on their mortgage that were at least 2 percentage points above the prevailing mortgage interest rate and that this figure rose above 25% in 2003,

after the interest rates further dropped. Further empirical evidence on this topic is provided, e.g., by Keys et al. (2014) and Andersen et al. (2015). The former find that approximately 20% of US households have failed to refinance a mortgage despite being optimal and feasible for them to do so, estimating a median loss of \$11,500 per household. They also report that failure to refinance is less widespread among more financially savvy households. Likewise, Andersen et al. (2015) exploit a rich Danish dataset and show that sluggish mortgage refinancing is extremely common, entails substantial costs and is less likely among younger, richer and highly educated households. It has also been documented that less educated individuals have more difficulties recalling the terms of their mortgage (Bucks and Pence, 2008) and self-report implausibly low mortgage rates (Schwartz, 2006; Campbell, 2006, 2013). Using the US data from the recent financial meltdown, Gerardi et al. (2010) find that a low numerical ability of the households was a contributing factor to the massive mortgage delinquencies and foreclosures. Devlin (2002) reports that less financially savvy households choose their banks primarily based on convenience and referral, whereas for the financially sophisticated ones the product quality and price are more important factors. Along the same lines, Brown and Hoffmann (2016) document that the financially sophisticated borrowers are less likely to establish a mortgage relationship based on geographical proximity. Finally, Cole et al. (2015) document that education not only increases the equity holdings, but also reduces the probability of financial hardship (declaring bankruptcy, facing a foreclosure or becoming delinquent on a loan). Similarly, Brunetti et al. (2015) find the household financial fragility to be strongly and negatively associated with education.

We also contribute to the literature that evaluates the ability of government policy intervention to influence bank borrowers' behavior, such as mortgage debt renegotiation and mortgage refinancing. For example, Agarwal et al. (2012) evaluate the effect of Home Affordable

Modification Program (HAMP), a multi-billion dollar program that targeted the borrowers in danger of an imminent default, providing financial incentives (through one-time and “pay for success” incentives) to loan servicers, mortgage investors and borrowers to renegotiate such mortgages. In another contribution, Agarwal et al. (2015) evaluate the effect of Home Affordable Refinancing Program (HARP), a program that targeted borrowers with insufficient home equity by extending federal credit guarantee on such loans in order to enable their refinancing. Unlike these two papers, our outcome of interest is bank switching rather than mortgage renegotiation or refinancing.² Furthermore, the Bersani Law we exploit in our analyses did not entail direct payouts to market participants like HAMP, but it has foreseen indirect financial incentives coming from the market in the form of better loan terms – a result of increased competition. Finally, in order to investigate the role of refinancing costs, we focus on the Italian mortgage market, rather than on the US one, where the mortgage refinancing costs are generally very low (Andersen et al., 2015) while a significant friction for refinancing arises from low collateral (Agarwal et al., 2015).

3. Institutional Background

The Law 40/2007, proposed by the minister Pier Luigi Bersani and commonly referred to as the “Bersani Law”, introduced a set of liberalization measures in several sectors in Italy, aimed to promote consumer protection, enhance competition, and increase the overall economic activity.³ The most relevant measure concerning the market for mortgage loans was a substantial reduction in the mortgage refinancing costs, mainly through a cut of the early repayment fees imposed by

² For a household with a mortgage, bank switching implies mortgage refinancing, but only with an outside bank whereas for a household without a mortgage, the refinancing is clearly impossible while switch might still occur.

³ The Law 40/2007 underwent a swift legislative process: it came into force in mid-April 2007, after being conclusively approved by the Italian parliament on 2 April 2007. The Law converted the corresponding Decree issued on 31 January of the same year.

the banks, the mortgage registration, and the notary fees. Prior to the Bersani Law, banks used to charge a penalty of at least 1% of the value of the loan for early mortgage termination.⁴ Furthermore, since the old mortgage had to be canceled and replaced by a new contract, the procedure involved additional costs, such as the registration taxes and the notary fees. As a result, the overall cost of changing a mortgage provider reached at least 3% of the mortgage amount, and the magnitude of these costs discouraged the mortgage holders to switch to another bank.⁵

One of the main provisions introduced by the reform was the facilitation of the mortgage subrogation (i.e., “portability”), where a mortgage is transferred to another bank by the will of the debtor, with the declared intent of increasing the mobility of the mortgage holders.⁶ To exercise the mortgage subrogation, several conditions have to be met, the most important one being that the amount of the outstanding debt cannot be changed. On the other hand, the reform now allowed the level and type of the interest rate (e.g., fixed vs. floating) and the maturity of the loan to be altered, with fully transferable collateral and without losing the tax benefits that the initial mortgage might have entailed. After the reform, the full procedure was possible at significantly reduced (or even without) costs.⁷ The new legislation, according to which the current bank was

⁴ As reported in Beltratti et al. (2017), “[...] the average prepayment penalty was [...] between one and two percent of the mortgage amount for ARMs, and between two and three percent for FRMs, but some penalties were as high as eight percent [...]. Most penalties were fixed for the entire duration of the mortgages and displayed only small differences across banks”.

⁵ For more details on this, see e.g. Bajo and Barbi (2015).

⁶ The mechanism of subrogation was already provided by the Italian Law (see art. 1202 of the Civil Code). In practice, however, this article was not applied to the banking relationships, due to certain clauses the banks commonly included in the contracts prior to the introduction of the Bersani Law.

⁷ Specifically, the new Law introduced the following set of thresholds for the prepayment penalties:

- a) All payment penalties were abolished for first home mortgages granted after 2 February 2007 and for all the mortgage loans granted from 3 April 2007.
- b) For mortgages originated after 2001, a fee of up to 1.9% of the principal outstanding can be imposed.
- c) The maximum applicable penalty equal to 0.5% of the principal outstanding is set for the mortgages originated prior to 2001.
- d) A further reduction in the penalties was mandated for loans maturing in 3 (2) years, for which the fees are cut to 0.2% (no fee), respectively.

not entitled to oppose the household's choice, was closely followed by the media and, not surprisingly, warmly welcomed by the households while fiercely opposed by the banks.

In sum, the Bersani reform made mortgage prepayment and/or refinancing substantially simpler and cheaper, allowing the households to exercise full mortgage refinancing at a reduced cost, and eventually at no cost at all. The Italian mortgage market thus became similar to the US and Danish markets, where the prepayment penalties are set to zero. Thus, the reform diminished the mortgage lock-in potential and increased the competitive pressure on the mortgage providers.

4. Data and Variables

4.1. Data sources

Our main data source is the Survey on Household Income and Wealth (SHIW), a biannual population-representative household survey set up by the Bank of Italy. The basic statistical unit of the survey is a household, which includes all people who normally reside in the dwelling and contribute at least part of their income. Each household reports a household head, defined as the person primarily responsible for the household financial decisions, who answers the bulk of the questions on behalf of his/her household members. The sample consists of about 8,000 households (24,000 individuals) for each wave, half of whom are rotating panel units, distributed across about 300 Italian municipalities. The scope of the survey has grown over the years, with a questionnaire comprising about 200 questions.⁸ In addition to the demographic and socio-economic details, the survey now provides a rich set of information on different aspects of economic and financial behavior, including the choice of the financial intermediaries and financial services used. What is

⁸ See Bank of Italy (2012) for more information on the sampling and interviewing methods employed in the SHIW.

important for our purpose is that the panel component of the survey, combined with the household's bank identifier, enables us to infer whether a household changed its (main) bank between two survey waves. Using the same identifier, we are also able to supplement the dataset with the bank-level information from Bankscope (BS) – a source providing general information on banks, such as their history and specialization, as well as extensive bank data collected from their balance sheets and income statements.

4.2. Variable Definitions

This section describes the variables used in our empirical analysis.

The variables central to the analysis are constructed from a SHIW module that collects information on household-bank relationships. The households are asked to report the bank(s) they use, to single out the one they use most often (their main bank), and to list the services used with this bank. Households may indicate one or more services among the following: (i) payments of utilities, rent, or other expenses; (ii) mortgage; (iii) consumer credit and personal loans; (iv) securities custody, administration, and management; and (v) insurance.

Exploiting the panel component of SHIW, we follow the households over time and construct our binary dependent variable $Switch_{it}$, being 1 in t if household i changed its main bank between wave $t-1$ and t , and 0 otherwise.⁹

We also build two control variables: one for exclusivity of a bank relationship, namely *The only bank used*, defined as a dummy being 1 if a household has dealings with one bank only, and

⁹ To account for a possible bank consolidation between two SHIW waves, we adopt a conservative definition of switching where we do not count as a switch if a household turns to a bank that has been involved in a merger or an acquisition with the household's previous bank. We do not count "forced" switching either, where a household had to switch simply because his previous bank ceased to exist.

0 otherwise; and one for the intensity of the bank relationship, namely *Broad Relationship*, a dummy taking value 1 if a household uses two or more bank services in addition to a bank account.

In line with the literature on household finance, we control for a wide set of standard socio-economic and demographic characteristics that may affect the households' economic decisions. The economic condition of a household is captured by *Income* and *Net wealth*, both in the form of quintiles dummies. We also control for the *Household Size* by means of a variable counting the number of household members, for the household head's *Age*, in both linear and quadratic terms, and for gender and marital status, by means of two dummies for *Male* and *Married*, respectively. Education is controlled for with a set of dummies for the highest educational attainment of the household head, these being elementary school (*Edu_2*), middle school (*Edu_3*), high school (*Edu_4*), bachelor's degree (*Edu_5*), post-graduate qualification (*Edu_6*), with those without any education being the reference category. We also control for homeownership by means of a dummy for owning the residential house (*Homeowner*) and for household head's main professional occupation, including dummies for being *Employee* or *Self-employed*, thus having the non-working as the reference category. Finally, we control for the bank features such as its specialization, performance, size, and recent involvement in a merger or an acquisition. The bank specialization is captured by means of two dummies for the bank being *Cooperative* or *Savings*, with the commercial banks being the reference category. We proxy the bank profitability by Return on Assets (*ROA*), whereas we proxy the *Bank Size* by the bank total assets. We also include a dummy for recent M&A involvement (*M&A*), this being 1 if the bank was involved in M&A process between $t-1$ and t , and 0 otherwise. For a detailed definition of all the variables used in the analysis see Table A1.

4.3. Data and Descriptive Statistics

The empirical analyses rely on data spanning the period from 2004 to 2012 and use the variables listed in Table 1.

In Panel A, we report the descriptive statistics at the household level for the observations that used to estimate our empirical model in equation (1), for which only the waves 2004, 2006 and 2008 are used. Over the three waves, we end up with an unbalanced panel of 2,110 unique households. Our dependent variable captures the change in the household's main bank: this implies that each unique household in the sample has to be observed at least in two consecutive waves. Panel B provides statistics for our main variable of interest in equation (2), for which we use the data from all the waves available during the 2004-2012 period. In this model, the estimation sample counts 3,628 unique households.¹⁰

The average age of the household heads is around 54 years, about 70% are males, 72% are married, whereas around 11% have a graduate degree. The median household is composed of 3 people, has a disposable income of around €36,000, and net wealth of around €216,000. Homeownership is widespread, since 74% of households in our sample own their primary residence. About 41% of the household heads work as employees, 17% are self-employed, and the rest are retirees and other non-working. A large majority of the households use only one bank and no more than one bank service in addition to a bank account. Overall, 88% of households use a commercial bank (as their main bank), 6% use a cooperative bank, and 6% use a savings bank.¹¹

¹⁰ In all analyses we drop the households in which the household head is aged over 91 or below 19, as well as the households that report negative total consumption or possess neither financial nor real assets.

¹¹ There are 83 banks used by the households in our estimation sample, which hold 94% of total assets of all commercial, cooperative and savings banks operating in the Italian market, making our final dataset highly representative also of the Italian banking market.

5. Methodology

5.1. The Effect of the Reform on Switching *with* a Mortgage

We examine the effect of the reform on switching *with* a mortgage (i.e., switching when prepaying/refinancing a mortgage loan) using a difference-in-differences (DiD) approach. This methodology relies on measuring the differential effect of the event on a group that is affected by the legal change, which we refer to as the treated group, and a group that is not directly affected by it, which is the control group. Given the focus of the Bersani reform on mortgages, our treated group consists of the households that have a mortgage that is still outstanding around the time of the reform, while the control group consists of the households that do not hold a mortgage.¹²

Figure 1 illustrates the timeline that we use in our methodological approach. The notches on the axes represent the timing of our biannual observations, and the Bersani reform, carried out in early 2007, occurred between two SHIW waves. We can thus compare household's bank switching propensity before and after the legal change. In our empirical framework, we face at least two conceptual limitations. The first is related to the nature of our dependent variable, $Switch_{it}$. As we pinpoint switching by comparing the main bank a household declared to use in survey waves $t-1$ and t , we are unable to observe the exact timing of a switch, but we can only identify whether switching took place at some point between the two interviews. To address this peculiarity of the data, we use lagged regressors. The second limitation concerns the time span to be used in our DiD estimation. In each period, a household may choose to take out a mortgage

¹² We focus on the mortgage holding at the main bank, a minor simplification given that almost 95% of the treated households have a mortgage at their main bank. The households that take out a mortgage between two consecutive SHIW waves are excluded from DiD analyses as to ensure that the treatment does not contaminate our control group.

and/or switch bank, which confounds our identification strategy in a setting with more than two periods. For this reason, we restrict our analysis to a two-period DiD estimation.¹³

A crucial element in any DiD specification is the viability of the common trend assumption. This assumption implies that the treated and the control group would have similarly evolved if there had not been a legal change, i.e., that the non-mortgage holders are a valid control group to assess the counterfactual of what would have been the trend for the mortgage holders if the Bersani Law had not been introduced. The evolution of the behavior of the treated and the control group prior to the event, as depicted in Figure 2, provides a reasonably reliable indicator. Figure 2 plots the shares of switchers among the mortgage holders, i.e., the households that always report having a mortgage, vs. the non-mortgage holders, i.e., the households that never report having a mortgage, from 2004-2010. The nodes correspond to the statistics computed biannually, connected by the lines to ease the visual representation. In support of our identification strategy, prior to the reform in 2007 the share of switchers among the mortgage holders and non-mortgage holders has been evolving close to parallel.¹⁴ It is important to note that we do not observe a dip in switching activity of mortgage holders before the passage of the Law that would suggest strategic deferral of switching and would inflate our DiD estimates.

¹³ For clarification, looking at Figure 1, it is clear that if we included observations from 2010 onwards to our analysis, we could include mortgages that may have been taken out after 2007, i.e., after the Bersani Law was introduced. This setting would be inappropriate for a DiD estimation of the impact of the reform, as refinancing costs for these new mortgages are set to zero since their very origination. A two-period DiD also enables us to avoid the main concerns raised by Bertrand et al. (2004) on the serial correlation of the standard errors, which often plagues the DiD analysis. The alternative method the authors propose is to compare averages of the outcome variable pre- vs. post-reform (as, e.g., in Cerqueiro et al., 2015), but since our outcome variable is a dummy and given the short panel, this method is not applicable in our context.

¹⁴ We also statistically test for the common trend prior to the reform. We cannot reject the null of trend equivalence at the conventional levels of statistical significance.

Figure 2 also provides support to the notion that the Bersani reform had a strong impact on the switching behavior of the mortgage holders. In fact, while before the reform they were significantly less likely to switch their bank compared to the non-mortgage holders, following 2007 they became the prime switchers, and this “inversion” effect is large. Most of the switching occurs right after the passage of the Law (i.e., in 2008), since as we move further from the event, the more likely that the households have already gone to the right banks.¹⁵ This spike in bank switching of households with outstanding mortgages coincides with a sharp increase in total number of banks originating mortgages, a likely realization of an increased competition. Specifically, in our sample there were 33 banks giving mortgages before 2004, a number raised to 36 by 2006 (9.09% increase), to 60 by 2008 (66.67% increase), to 88 by 2010 (46.67% increase) to get to 89 banks by 2012 (1.14% increase). Such developments suggest that, while the wave of mortgage-motivated bank switching soon abated, the increase in bank competition, as measured by number of banks in the mortgage market, has been permanent. To evaluate more rigorously the effect of the reform to switching *with* a mortgage, we estimate the following regression equation:

$$Switch_{it} = \alpha_0 + \alpha_1 \mathbf{Treated}_{it-1} + \alpha_2 \mathbf{Post}_t + \alpha_3 (\mathbf{Treated}_{it-1} \times \mathbf{Post}_t) + \mathbf{X}_{it-1} \boldsymbol{\theta} + \varepsilon_{it} \quad (1)$$

where $Switch_{it}$ takes the value of 1 if a household changes its main bank between $t-1$ and t ; $\mathbf{Treated}_{it-1}$ is a binary variable taking value 1 if in $t-1$ household i has a mortgage with its main bank, and 0 if a household does not have a mortgage in either $t-1$ nor t ; \mathbf{Post}_t is a binary variable taking the value of 1 if the year falls after the introduction of the Bersani Law, and 0 otherwise; finally, \mathbf{X}_{it-1} is a vector of control variables, and ε_{it} is the error term. The main variable of interest is the interaction term $\mathbf{Treated}_{it-1} \times \mathbf{Post}_t$, since the associated coefficient α_3 measures the

¹⁵ One can also think of this evolution of switching in terms of a “backlog”, i.e., a mass of people who would like to refinance, but are discouraged by high switching costs, and that switch as soon as these costs are slashed down. After that, the switching stabilizes at its (let’s call it) natural rate.

differential effect of the change in the law across households that held a mortgage around the time the Bersani Law was introduced and those that did not.¹⁶ An identification assumption behind this comparison is that, in the absence of the reform, the switching rates in the treatment group (mortgage holders) and in the control group (households without a mortgage) would follow similar patterns (up to a constant difference). If the reform had the predicted effect of boosting mortgage shopping, and in line with Figure 2, α_3 is expected to be statistically significant and positive.

In order to compare the switching right before and after the legal change, we estimate our main model on the sample spanning the period 2004-2008 (i.e., we compare switching between 2004 and 2006 to switching between 2006 and 2008). In doing so, we also seek to isolate the motivation to switch *with* a mortgage stemming from a reduction in the refinancing costs as opposed to a fall in the interest rates. In fact, a significant reduction in the interest rates may represent a sufficient incentive for the mortgage holders to switch, regardless of the switching costs. In other words, if the interest rates are sufficiently reduced, the refinancing alternatives may become attractive even if the refinancing costs remain unaltered. As shown in Figure 3, during the 2004-2008 period the interest rates substantially increased, as a result of the restrictive monetary policy pursued by the European Central Bank, starting to fall only in late 2008. Thus, if anything, the evolution of the interest rates would work against us finding a significant effect of the Bersani reform.

¹⁶ To the extent that the bank acts to keep the customer, and some mortgages may be refinanced at the current main bank, our analysis actually captures a lower bound of the refinancing volume. Next, as the reform cut the prepayment fees in addition to the costs of changing the mortgage lender, our definition of the treated group allows us to account for the mortgage prepayments as well.

5.2. The Effect of the Reform on Switching *for* a New Mortgage

So far, our methodological framework analyzed the mortgage loans outstanding around the change in the law, i.e., the extant mortgages. Our second empirical model aims to assess the effect of the reform on switching *for* a mortgage, thus focusing on the new mortgage issues. The economic rationale is the following. The reform is likely to have affected the incentives of both banks and their retail clients. The banks, now operating in the mortgage market with substantially lower switching costs, compete more aggressively to attract new mortgage buyers. On the other hand, the households taking out a mortgage are less reluctant to do so at an outside bank (increasing the households' mobility), since they are aware that newly ensured flexibility in the market will enable them to switch again should they wish. In other words, the households can now afford to potentially make a mistake in choosing a bank, since they can change their mind anytime in the future at no penalty. As a result, we expect the households taking out a mortgage to have become less inert, i.e., more likely to switch banks, than those taking out a mortgage prior to the reform. We test this prediction empirically by estimating the following regression on the subsamples before and after the introduction of the Bersani Law:

$$Switch_{it} = \beta_0 + \beta_1 \mathbf{Add Mortgage}_{it} + \mathbf{X}_{it-1} \boldsymbol{\theta} + \varepsilon_{it} \quad (2)$$

where $\mathbf{Add Mortgage}_{it}$ is a dummy taking value 1 for the households that did not hold a mortgage with their bank in $t-1$ and take it out in t , and 0 otherwise. Vector \mathbf{X}_{it-1} gathers a set of control variables lagged one period and ε_{it} is the error term. If the sharp reduction in switching costs induced by the reform had the predicted effect, we expect β_1 to be positive and statistically significant in the period following the legislative change, whereas the association should be weaker (if at all significant) prior to the reform.

6. Results

6.1. Switching *with* a Mortgage

The univariate analysis of our sample provides preliminary evidence of the effect the Bersani Law had on the switching of the extant mortgage holders. Table 2 displays the share of switchers for the mortgage holders (treated group) and the households not holding a mortgage (control group), as well as differences of the means test. Prior to the reform, significantly fewer households in the treated group had switched between two consecutive SHIW waves compared to the share of switchers in the control group, corroborating the anecdotal evidence of high switching costs for the mortgage holders. The magnitude of this wedge is economically important and statistically significant at 10% confidence level. In turn, following the change in the Law, the treated group experienced a much sharper increase in the share of switchers compared to the control group, resulting in a reversed wedge, i.e., a larger share of switchers among the mortgage holders than among the households without a mortgage. Again, the difference between the two groups is statistically significant and economically important. In a nutshell, the univariate analysis shows that prior to the reform there had been 7.5% fewer switchers among the mortgage holders than among the bank clients without a mortgage, whereas after the reform the mortgage holders switched about 6% more than clients in the control group. When interpreting Table 2, however, we should bear in mind that the statistics do not address the potential heterogeneity across the households, banks, and time.

To account for these factors, we carry out a multivariate analysis using a difference-in-differences estimation, as explained in Section 5. The estimates in Table 3 correspond to the coefficients obtained by an ordinary least squares (OLS) estimation of equation (1), varying the

controls included in the estimation. All regressions are estimated using the sample weights provided by SHIW, with the robust standard errors clustered at the province level.¹⁷ In the first column, we present a model where we estimate the “baseline” DiD, i.e., a difference-in-differences model without the control variables. The results are in line with the statistics in Table 2 and the visual representation in Figure 2. Most importantly, our DiD coefficient estimate ($Treated \times Post$) is positive, large in magnitude, and statistically significant at 5% level. A simple interpretation of the results in column (1) is that the increase from 2006 to 2008 in the proportion of switchers among the mortgage holders is 15% higher than the increase in the share of switchers in the control group for the same period. This result, however, may be driven by the differences (in the cross-section and over time) in the socio-economic features of the households in the treated group compared to the clients in the control group, of the banks they are using and/or of the regions they reside in, for example. To obtain a DiD estimate net of such potentially confounding factors, in columns (2)-(4) we add various controls to the baseline model. Column (2) presents the results of the model that controls only for the region fixed effects and for the population size of the municipality of the household residence. In column (3), we add to the model the household controls which the household finance literature conventionally relates to their financial choices: age (linear and squared), gender, marital status, household size, education, homeownership, employment status and income and wealth (both in the form of quintiles). We also control for the exclusivity of the relationship with the bank via the dummy *The only bank used*, being 1 if the main bank is the only one a household uses. In column (4), we further add the controls for the bank

¹⁷ OLS is employed for the ease of interpretation of the reported results. Furthermore, the interaction terms in non-linear models may be biased and imprecisely estimated (see Ai and Norton, 2003). Yet, as a robustness check, we re-estimated our regressions using a probit model, obtaining largely comparable results (available upon request). Our main results are also robust to clustering of the standard errors at fewer clusters with respect to the provinces (i.e., clustering at the level of regions and macro-regions), as well as to clustering at the bank level.

characteristics, such as its specialization, profitability, size, and recent organizational restructuring. As we increase the number of controls across columns, our DiD coefficient remains largely comparable to that obtained in column (1). In our full model specification in column (4), we estimate that a mortgage holder was around 11% less likely to switch his/her bank than a comparable household in the control group preceding the reform, whereas after the introduction of the Bersani Law the likelihood of switching was about 14% higher for the mortgage holders than for the households without a mortgage. Based on the results presented in Table 3, we can assert a causal relation of the Bersani reform to the hike in switching *with* a mortgage.

6.1.1. The Role of Household Education

We argue that for the mortgage holders to respond (in a timely manner) to the changed market circumstances, two conditions must be met. First, they must learn and understand the content of the new Law and the implications for the dynamics of their relationship with a bank. Second, they must be able to compare the competing offers across banks and choose the most suitable one. Both tasks are highly likely to depend on the household sophistication.¹⁸

We re-estimate the equation (1) for the subsamples of households whose head obtained at least a high school degree and those with a lower level of education. The results for the two subsamples are presented in Table 4, panels A and B, respectively. It is apparent that the findings in Table 3 are confined to the pool of better-educated individuals and, thus, that the household sophistication drives the results therein.¹⁹ This finding is particularly worrying for the policy

¹⁸ In order to collect and process the relevant information, the individuals incur the search costs, i.e., the costs in time and effort required for gathering and processing information. The extant literature acknowledges that the cost of singling out “the best deal” may be substantial for the individuals with limited knowledge and/or cognitive capacity (see, e.g., Smith et al., 1999). The role of search costs – and hence of education – is thus relevant, since high search costs may impede switching to a better alternative even when the switching costs are negligible.

¹⁹ As discussed in Badarinsa et al. (2016), in the mortgage industry slang, the households that fail to refinance their mortgages even when it is profitable for them to do so are referred to as the “woodheads”.

makers, as the majority of households that take out a mortgage lack knowledge of basic financial concepts (Lusardi and Tufano, 2009; and Campbell, 2013). These households also seem more likely to have inappropriate mortgage terms in the first place, and subsequently to be in need of having their loan arrangement altered.²⁰ Our findings thus support the rich body of literature that relates various measures of household sophistication (education, financial literacy, and cognitive abilities) to the sub-optimal financial behavior (see, e.g., Campbell, 2006; Calvet et al., 2007 and 2009; Lusardi and Mitchell, 2014; Christelis et al., 2010; and Andersen et al., 2015).²¹

6.1.2. The Role of Competition

Next, we investigate the role of competition in the households' responsiveness to the Bersani reform. The effect of competition is ambiguous a priori. On the one hand, low competition may imply fewer attractive outside opportunities, which should make the households less responsive to the facilitated portability of a mortgage ("availability of substitutes" argument). On the other hand, the households in less competitive environments may have less affordable mortgage terms in the first place, and may thus be more likely to seek banks to switch to, should they offer them better loan deal ("initial condition" argument). Therefore, ex-ante, it is difficult to sign the effect of competition in our context.

We answer this question empirically by splitting the sample according to the bank competition in the market, as proxied by the Herfindahl index computed from the number of ATMs

²⁰ For example, Moore (2003) reports that, in the US, the victims to predatory lending are less likely to understand basic financial concepts, suggesting that they were not aware of the cost of their mortgage loans. Furthermore, Mayer et al. (2013) report that, in the late 1990s and early 2000s, the banks in the US also used the prepayment fees in a predatory manner.

²¹ A very low percentage of the households in our sample who reported having been rejected for a mortgage application or having been discouraged from applying for a loan because they thought they would be turned down, provides a reasonable indicator that the difference in creditworthiness between the two subsamples is unlikely to be a concern.

of banks operating in the province of the household residence. Panel A and Panel B of Table 5 report the results for the low and high level of bank competition, respectively. Our main findings are confirmed only for the households living in the provinces with less fierce bank competition (Panel A), providing evidence in support of the “initial condition” argument.²² These results can also be related to Agarwal et al. (2015), which finds that a legal change that increased competition – by limiting the legal risk for lenders to refinance a loan originated by another lender – effectively increased refinancing. In our context, the rise in competition induced by the Bersani Law was particularly pronounced in the provinces where the initial competition was very low, which led to a significant rise in bank switching in such markets.

6.1.3. The Role of Relationship Banking

Last, we study whether relationship banking was a counteracting force to the reform. Using the Survey of Consumer Finance, Chakravarty and Scott (1999) show that relationship lending lowers the price of credit for consumer loans. Asymmetric information is at the core of relationship banking (see, e.g., whereas the severity of the asymmetric information problem itself typically increases with the distance between the client and its bank. The competing banks will face an increasing disadvantage while approaching the borrowers closer to the most informed bank.²³ We thus proxy for the importance of the relationship banking at the province level by means of the share of households in a province that in 2006 listed proximity (to home or work) as a reason for

²² An additional factor contributing to our result may be that, *ceteris paribus*, high competition equipped the households with significant bargaining power to renegotiate a mortgage with their initial bank. According to this argument, in such environments the Bersani reform should have little or no effect on refinancing with an outside bank (i.e., switching to transfer a mortgage) – as supported by Panel B. Since we can only gauge the mortgage refinancing if it coincides with the bank switching, i.e., if a household refinances a mortgage with a bank that is different from its original lender, we do not investigate this topic further (on this issue, see, e.g., Bajo and Barbi, 2015).

²³ For a more detailed discussion on why the distance between the borrower and the lender and the distance between the borrower and the closest competing bank may affect lending conditions, see, e.g., Degryse and Ongena (2005).

choosing their main bank. We split the sample around the median of this measure. The results for the two subsamples are presented in Table 6, panels A and B, respectively. Our findings confirm the offsetting effect of the relationship banking in our context, as it is only in the provinces with relatively weaker role of relationship banking that the reform significantly boosted switching *with* a mortgage.

6.1.4. Placebo Test

In order to test the robustness of our identification strategy, we perform a placebo test. Specifically, we analyze whether the reform had an effect on the households holding a consumer loan, as market conditions for this type of liabilities should not have been directly affected by the Bersani reform. That is, we re-estimate the model as in Table 3, but specifying as treated the households holding a consumer and/or personal loan, instead of those with a mortgage.²⁴ The results are reported in Table 7. In support of our identification strategy, the DiD estimate is not significantly different from zero at conventional confidence levels.

6.2. Switching *for* a (New) Mortgage

We now turn to investigate the effect of the Bersani reform on the newly originated mortgages. In particular, we analyze whether the reform advanced the role that taking out a mortgage plays in a household's decision to switch its bank, i.e., whether it sparked switching *for* a mortgage. Table 8 reports the results of estimating equation (2) on two subsamples – prior to the reform and following the reform. The sample used for the estimation spans the full 2004-2012 period. Since we want to compare the switching behavior of the households that take out a

²⁴ So as not to confound the treated and control groups, we drop from the sample all households that also hold a mortgage in addition to a consumer loan. In doing so, we lose very few of the treated observations, since few households in our sample hold both a consumer loan and a mortgage with the bank.

mortgage in t with those that do not, we drop from the sample households that already had a mortgage in $t-1$. Besides the main variable of interest, *Add Mortgage* _{it} , we include the household controls, regional fixed effects, and municipality size dummies. In the subsample following the reform, we include the time dummies to capture the economy-wide time fixed effects that may affect the households' set of opportunities. The results in Table 8 show that, prior to the reform, taking out a mortgage was not a significant factor for a household's decision to switch its bank, as captured by an insignificant coefficient on *Add Mortgage* _{it} , in column (1), whereas it became a relevant factor thereafter, as shown in column (2). According to our estimates, the households taking out a mortgage after the reform are about 14% more likely to switch their bank than clients who do not have a mortgage in either of the two periods.²⁵ Given that the share of switchers in our sample ranges from 23-32% biannually, this effect is economically important and identifies taking out a new mortgage as a strong motivation to switch following the reform. This result is largely in line with Brown and Hoffmann (2016) who document that in Switzerland among the households with multiple bank relationships the mortgage relations are typically more recently established. Our findings point towards an increased bank competition, as a result of a more flexible mortgage market that once provided banks with powerful means to retain their clients. The results may also capture a decrease in the household inertia, as they are aware of a much lower probability of being locked in an inappropriate mortgage deal. It seems very likely that both factors are at play, but with the data at hand we are not able to disentangle which of the two contributed more to our results. As for the extant mortgages, we redo our analysis for new mortgages on the subsamples differentiated based on the household head's level of education and a proxy for competition in the

²⁵ To account for the difference in the sample size before the reform compared to after the reform, we perform a Chow test for the difference between the two subsamples in the coefficient on our main variable of interest – *Add Mortgage*. The test rejects the null of coefficients equivalence at 5% statistical significance.

province of the household residence. Consistently, we find that the effect of the reform is more salient for the households headed by the more educated individuals and residing in ex-ante less competitive markets, reinforcing the role played by the household sophistication and competition in this context.²⁶

In column (3), we add a control for the intensity of the household relationship with the bank, which we proxy for via a dummy being 1 if a household uses more than one bank service in addition to a bank account (i.e., more than the median number of services with the bank). The dummy *Broad relationship*_{it-1} thus identifies the households that have a more intensive relationship with their bank and are hence expected to be more reluctant to switch.²⁷ The coefficient on the breadth of the relationship has the expected negative sign, which confirms the role of switching costs arising from the cross-selling of services (found also by Brunetti et al., 2016), but the effect is precisely estimated only on the post-reform subsample.

We further want to investigate whether this effect is strong enough to diminish switching *for* a mortgage. In fact, if a more intensive use of the bank services results in sufficiently high switching costs, it should reduce the inclination of such households to change their bank if they plan to take out a mortgage and a better mortgage deal is offered elsewhere. Thus, in columns (5) and (6) of Table 8, we re-estimate the model adding the interaction of the two terms: *Add Mortgage*_{it} \times *Broad Relationship*_{it-1}. This effect would result in a negative and statistically significant coefficient on the interacted variable. However, the coefficient estimate in column (6) turns out statistically insignificant and positive, which reinforces the motivation behind

²⁶ The results are omitted due to space considerations, and are available on request.

²⁷ As discussed in Brunetti et al. (2016), the reluctance to switch may be due to time and effort needed to evaluate net gains of switching for each particular bank service, but the clients using more services may also enjoy an advantage of economies of scope with the bank.

switching *for* a mortgage. A mortgage is one of the most important financial decisions the households make, because of its long maturity and high debt burden. Thus, the advantages of a better mortgage deal seem to well outweigh the one-off costs associated with the switching from a current bank.

7. Conclusion

In this paper, we use a unique panel dataset to study households' bank switching behavior in relation to the mortgage refinancing costs. To this end, we exploit the (exogenous) drop in these costs entailed by a reform carried out in Italy in early 2007, known as "the Bersani Law". By cutting the high refinancing costs that served as a "lock-in" for the mortgage clients, we argue that the reform caused the consequent increase in bank switching. Our results thus show that the mortgage refinancing costs play an important role in shaping the decision of households to change their bank.

To the best of our knowledge, this is the first attempt in the literature to investigate this topic empirically. We first focus on the extant mortgages, using a difference-in-differences (DiD) methodology that compares the switching propensities before and after the reform of the households that have an outstanding mortgage around the date of the reform (treated group) and those that do not hold nor take out a mortgage (control group). Our estimates point to a statistically significant and economically important relationship between the fall in refinancing costs and hike in switching *with* a mortgage, i.e., switching to prepay/refinance an existing mortgage loan. In a subsample analysis, we show that this effect was not uniform across households. Indeed, we find a significant impact of the Bersani reform only for the pool of better-educated households. The residual impediments to switching as reflected in, e.g., search costs (costs of identifying a better

alternative) are likely to be particularly high for the less educated individuals, thus, the Bersani Law had little (if any) effect on switching of these households. These results convey an important policy implication that the factors other than the switching costs may represent important impediments to switching, even with negligible costs of mortgage refinancing. Besides, we find that the Bersani primarily affected the households in ex-ante less competitive markets, perhaps because these households are highly likely to have less affordable mortgage terms in the first place and thus be in need of refinancing. We also show that the provinces with strong relationship banking were not affected by the reform.

We then analyze the effect of the Bersani Law on the switching behavior of the households taking out new mortgages, i.e., “switching *for* a mortgage”. Our results show that prior to the reform, taking out a mortgage did not have any explanatory power for switching a bank, whereas it became a highly relevant factor thereafter. These findings indicate that the newly increased flexibility in the mortgage market increased bank competition for new clients and decreased households’ inertia.

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Table 1: Descriptive statistics

All statistics are computed at the household level using the sample weights.

Variables	Obs.	Mean	Median	Min	Max	St.Dev.
PANEL A						
Age	2,650	53.79	53	20	90	14.31
Male	2,650	0.70	1	0	1	0.46
Married	2,650	0.72	1	0	1	0.45
Edu_2 (elementary school)	2,650	0.19	0	0	1	0.39
Edu_3 (middle school)	2,650	0.28	0	0	1	0.45
Edu_4 (high school)	2,650	0.40	0	0	1	0.49
Edu_5 (graduate degree)	2,650	0.11	0	0	1	0.32
Edu_6 (postgraduate degree)	2,650	0.004	0	0	1	0.07
Household size	2,650	2.69	3	1	9	1.23
Income (€1,000)	2,650	44.20	35.86	0	922.60	45.37
Net wealth (€1,000)	2,650	355.71	216.12	-875.42	17,878.38	771.19
Homeowner	2,650	0.74	1	0	1	0.44
Employee	2,650	0.41	0	0	1	0.49
Self-employed	2,650	0.17	0	0	1	0.38
Only one bank used	2,650	0.85	1	0	1	0.36
Total number of services used at the main bank	2,650	1.28	1	0	5	0.80
Broad relationship	2,650	0.31	0	0	1	0.46
Commercial bank	2,650	0.88	1	0	1	0.32
Cooperative bank	2,650	0.06	0	0	1	0.23
Savings bank	2,650	0.06	0	0	1	0.23
Return on assets (ROA)	2,650	0.71	0.79	-1.74	1.27	0.29
Bank size (in logs)	2,650	11.39	11.41	9.15	13.72	1.26
M&A	2,650	0.10	0	0	1	0.30
Switch						
<i>Pooled waves (2006-2008)</i>	2,650	0.29	0	0	1	0.45
<i>2006</i>	806	0.23	0	0	1	0.42
<i>2008</i>	1,844	0.32	0	0	1	0.47
Treated (Having a mortgage)						
<i>Pooled waves (2004-2006)</i>	2,650	0.16	0	0	1	0.37
<i>2004</i>	806	0.12	0	0	1	0.33
<i>2006</i>	1,844	0.18	0	0	1	0.38
PANEL B						
Add mortgage						
<i>2004</i>	702	0.07	0	0	1	0.26
<i>2006</i>	788	0.08	0	0	1	0.27
<i>2008</i>	1689	0.06	0	0	1	0.24
<i>2010</i>	1992	0.05	0	0	1	0.22
<i>2012</i>	1915	0.09	0	0	1	0.29

Figure 1: Timeline

The line represents time. The notches represent the timing of our observations.

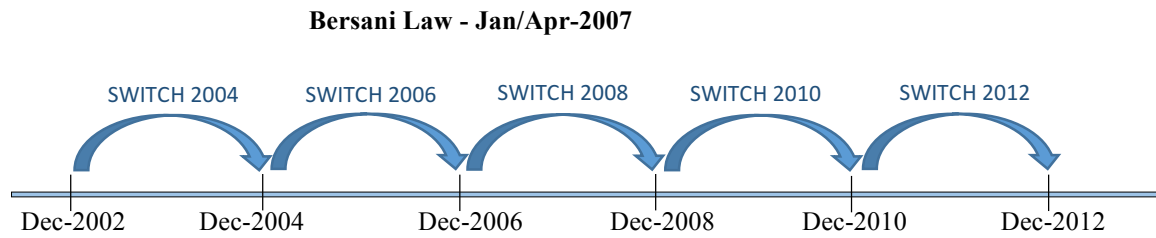


Figure 2: Evolution of switching propensities

The graph depicts the evolution of switching propensities over the period 2004-2012 for the mortgage holders and the households without an outstanding mortgage. The green dashed line indicates the introduction of the new Law.

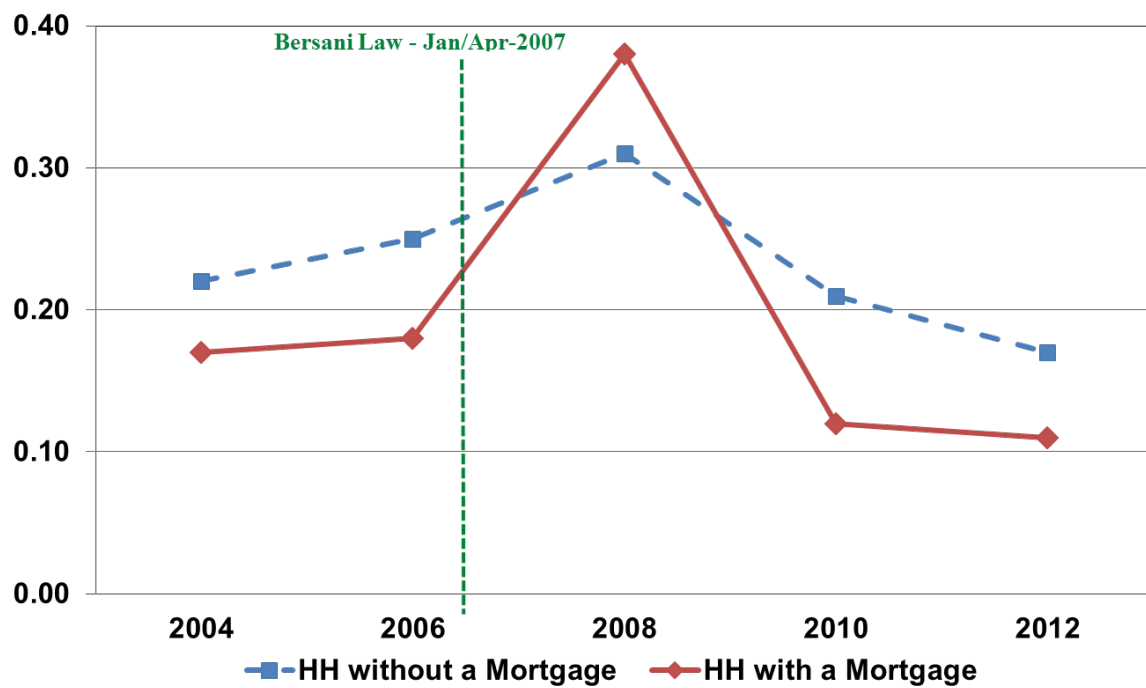
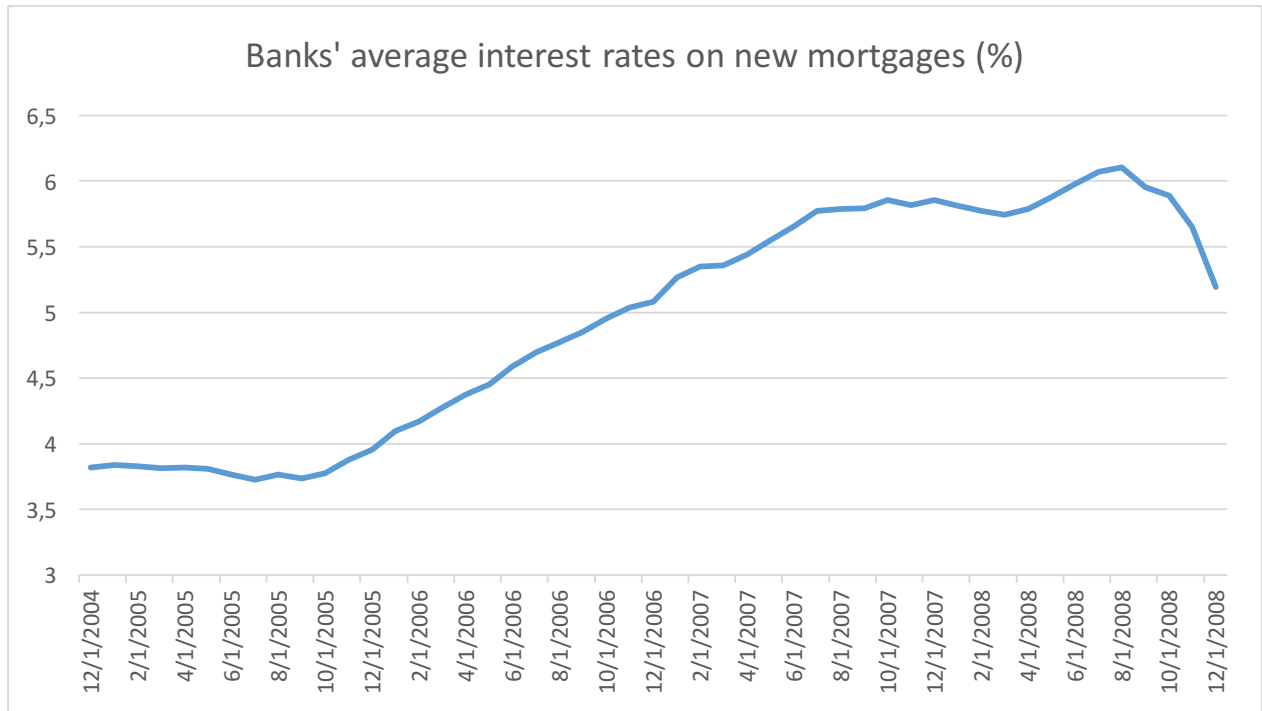


Figure 3: Interest rates evolution



Source: Bank of Italy, Base Dati Statistica.

Table 2: Bersani reform and switching propensity: Comparison of means

The table displays the switching propensities (mean of the dependent variable *Switch*) between $t-1$ and t for the *control* and *treated* groups, before and after the introduction of the Reform. *Treated* indicates that the household had a mortgage outstanding on December 31st of $t-1$. *Control* refers to the households that did not have a mortgage on December 31st of $t-1$ nor on December 31st of t . *Before* refers to switching between 2004 and 2006, and *After* refers to switching between 2006 and 2008. Standard errors are provided in the parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Control	Treated	Difference (Treated-Control)
Switch			
Before	0.267 (0.016)	0.191 (0.037)	-0.075* (0.044)
After	0.33 (0.012)	0.40 (0.028)	0.064** (0.03)
Difference (After-Before)	0.068*** (0.02)	0.207*** (0.051)	

Table 3: Bersani reform and the extant mortgages (switching *with* a mortgage)

The table reports the estimation output of the following regression:

$Switch_{it} = \alpha_0 + \alpha_1 Treated_{it-1} + \alpha_2 Post_t + \alpha_3 (Treated_{it-1} \times Post_t) + X_{it-1} \theta + \varepsilon_{it}$,
 where vector X_{it-1} is incrementally added with different sets of controls. Robust standard errors in the parentheses, clustered at the province level. All regressions are estimated using ordinary least squares and the sample weights. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Treated	-0.087 (0.059)	-0.100* (0.057)	-0.107* (0.063)	-0.118* (0.062)
Post	0.066** (0.033)	0.059* (0.031)	0.060* (0.032)	0.039 (0.030)
Treated × Post	0.154** (0.060)	0.162** (0.062)	0.153** (0.067)	0.144** (0.070)
Age			0.005 (0.007)	0.005 (0.006)
Age ²			-0.004 (0.006)	-0.003 (0.006)
Male			-0.044 (0.037)	-0.048 (0.035)
Married			-0.087** (0.036)	-0.087** (0.034)
Household size			0.025* (0.014)	0.025* (0.013)
Edu_2			-0.008 (0.075)	0.014 (0.069)
Edu_3			0.066 (0.077)	0.071 (0.075)
Edu_4			0.075 (0.082)	0.094 (0.076)
Edu_5			0.023 (0.095)	0.043 (0.084)
Edu_6			0.011 (0.169)	-0.006 (0.173)
Homeowner			0.012 (0.047)	-0.003 (0.040)
Employee			0.028 (0.028)	0.038 (0.030)
Self-employed			0.067 (0.067)	0.071 (0.071)
Income – Q2			(0.045)	(0.046)
Income – Q3			-0.005 (0.056)	-0.011 (0.053)
			-0.015 (0.053)	-0.028 (0.052)

Table 3 (continued)

Income – Q4			-0.040 (0.054)	-0.051 (0.051)
Income – Q5			-0.064 (0.064)	-0.080 (0.063)
Net Wealth – Q2			-0.034 (0.046)	-0.000 (0.043)
Net Wealth – Q3			0.011 (0.057)	0.055 (0.055)
Net Wealth – Q4			-0.006 (0.055)	0.032 (0.054)
Net Wealth – Q5			-0.040 (0.066)	0.003 (0.064)
The only bank used			-0.075** (0.031)	-0.078** (0.030)
Cooperative Bank				-0.067 (0.045)
Savings Bank				0.045 (0.068)
ROA				0.066 (0.047)
Bank Size (in logs)				0.078*** (0.014)
M&A				-0.023 (0.050)
Macro Regions controls	NO	YES	YES	YES
Municipality Size controls	NO	YES	YES	YES
Observations	2,650	2,650	2,650	2,650
R-squared	0.012	0.026	0.051	0.098

Table 4: Bersani reform and switching *with* a mortgage: Education split

The table replicates the results from Table 3 on two subsamples differentiated based on the household head's level of education. Panel A reports the results for a subsample of households whose head completed at least high school (i.e., with relatively higher level of educational attainment); Panel B reports the results for those who did not obtain a high school degree (i.e., having a relatively lower level of educational attainment).

	(1)	(2)	(3)	(4)
PANEL A: Higher education				
Treated	-0.151** (0.068)	-0.149** (0.061)	-0.139* (0.079)	-0.162** (0.079)
Post	0.063* (0.038)	0.058 (0.036)	0.051 (0.037)	0.034 (0.037)
Treated × Post	0.179** (0.078)	0.180** (0.085)	0.178* (0.095)	0.172* (0.098)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,278	1,278	1,278	1,278
R-squared	0.015	0.036	0.062	0.101
PANEL B: Lower education				
Treated	0.004 (0.104)	-0.010 (0.111)	-0.042 (0.115)	-0.033 (0.112)
Post	0.061 (0.039)	0.055 (0.038)	0.060 (0.038)	0.038 (0.037)
Treated × Post	0.119 (0.116)	0.137 (0.118)	0.136 (0.125)	0.129 (0.124)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,372	1,372	1,372	1,372
R-squared	0.011	0.028	0.061	0.124

Table 5: Bersani reform and switching *with* a mortgage: Competition split

The table replicates the results from Table 3 on two subsamples differentiated based on a proxy for competition in the province of the household residence: the Herfindahl index computed from the number of ATMs of banks operating in the province. Panel A reports the results for a subsample of households in a province with a Herfindahl index higher than the median in a given year (i.e., relatively low competition); Panel B reports the results for those who reside in a province with a Herfindahl index lower than the median in a given year (i.e., relatively high competition).

	(1)	(2)	(3)	(4)
PANEL A: Low competition				
Treated	-0.107 (0.067)	-0.096 (0.064)	-0.103 (0.086)	-0.123 (0.084)
Post	0.088** (0.038)	0.109** (0.044)	0.111** (0.046)	0.095** (0.042)
Treated × Post	0.175** (0.074)	0.168** (0.075)	0.158* (0.084)	0.160* (0.084)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,394	1,394	1,394	1,394
R-squared	0.021	0.055	0.072	0.119
PANEL B: High competition				
Treated	-0.067 (0.085)	-0.083 (0.092)	-0.100 (0.091)	-0.097 (0.090)
Post	0.029 (0.062)	0.065 (0.048)	0.075 (0.051)	0.058 (0.051)
Treated × Post	0.129 (0.086)	0.129 (0.107)	0.109 (0.107)	0.092 (0.108)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,256	1,256	1,256	1,256
R-squared	0.005	0.055	0.105	0.141

Table 6: Bersani reform and switching *with* a mortgage: Relationship banking

The table replicates the results from Table 3 on two subsamples differentiated based on a proxy for relationship banking in the province of the household residence: share of households in a province that in 2006 listed proximity (to home or work) among the reasons for choosing their main bank. Panel A reports the results for a subsample of provinces with the share of households being lower than the median (i.e., where relationship banking is relatively less important); Panel B reports the results for a subsample of provinces with the share of households being higher than the median (i.e., where relationship banking is relatively more important).

	(1)	(2)	(3)	(4)
PANEL A: Weaker role of relationship banking				
Treated	-0.039 (0.079)	-0.052 (0.075)	-0.046 (0.079)	-0.052 (0.078)
Post	0.064 (0.038)	0.064* (0.035)	0.057 (0.034)	0.052 (0.044)
Treated × Post	0.170** (0.078)	0.186** (0.074)	0.182** (0.082)	0.177** (0.087)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,285	1,285	1,285	1,285
R-squared	0.020	0.035	0.088	0.134
PANEL B: Stronger role of relationship banking				
Treated	-0.124 (0.083)	-0.115 (0.085)	-0.111 (0.102)	-0.138 (0.098)
Post	0.070 (0.051)	0.058 (0.045)	0.062 (0.046)	0.036 (0.040)
Treated × Post	0.123 (0.082)	0.093 (0.089)	0.054 (0.094)	0.064 (0.089)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	1,365	1,365	1,365	1,365
R-squared	0.011	0.049	0.091	0.149

Table 7: Placebo test (consumer and/or personal loans)

The table replicates the results from Table 3 using a sample of non-mortgage owners and a treatment indicator that singles out the households that reported having a consumer and/or personal loan. Robust standard errors in the parentheses, clustered at the province level. All regressions are estimated using ordinary least squares and the sample weights. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Treated	0.079 (0.195)	0.073 (0.187)	0.103 (0.185)	0.060 (0.179)
Post	0.064* (0.033)	0.055* (0.031)	0.062* (0.033)	0.031 (0.030)
Treated × Post	-0.195 (0.193)	-0.191 (0.182)	-0.233 (0.191)	-0.175 (0.195)
Macro regions controls	NO	YES	YES	YES
Municipality size controls	NO	YES	YES	YES
Household characteristics	NO	NO	YES	YES
Bank characteristics	NO	NO	NO	YES
Observations	2,201	2,201	2,201	2,201
R-squared	0.005	0.024	0.056	0.107

Table 8: Bersani reform and new mortgages (switching *for* a mortgage)

The table reports the estimation output of the following regression:

$$Switch_{it} = \beta_0 + \beta_1 Add\ Mortgage_{it} + X_{it-1}\theta + \varepsilon_{it},$$

where i indexes households and t indexes the year. Robust standard errors in the parentheses, clustered at the province level. We drop the households that already had a mortgage in $t-1$. The sample is split with regard to the Bersani Reform occurred in 2007. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	2004-2006	2008-2012	2004-2006	2008-2012	2004-2006	2008-2012
Add mortgage	0.053 (0.064)	0.144*** (0.038)	0.054 (0.062)	0.143*** (0.039)	0.097 (0.071)	0.128*** (0.036)
Broad relationship			-0.062 (0.044)	-0.049** (0.022)	-0.041 (0.047)	-0.054** (0.023)
Add mortgage × Broad relationship					-0.203* (0.121)	0.074 (0.102)
T2006	0.008 (0.024)		0.008 (0.024)		0.007 (0.024)	
T2010		-0.108*** (0.030)		-0.107*** (0.030)		-0.108*** (0.030)
T2012		-0.118*** (0.025)		-0.116*** (0.025)		-0.117*** (0.025)
Controls	YES	YES	YES	YES	YES	YES
Observations	1,490	5,596	1,490	5,596	1,490	5,596
R-squared	0.072	0.045	0.074	0.046	0.076	0.047

Table A1: Description of variables

Variable	Description	Data source
<i>Dependent variable</i>		
Switch	Binary variable taking value 1 if between two consecutive SHIW waves the household changes its (main) bank, 0 otherwise.	SHIW
<i>Regressors</i>		
Treated	Binary variable taking value 1 if a household has a mortgage still outstanding with its (main) bank, and 0 otherwise.	SHIW
Post	Binary variable taking value 1 if the year of observation falls after 2007, i.e., the Law 40/2007 was introduced.	SHIW
The only bank used	Binary variable taking value 1 if a household has only one bank, and 0 otherwise.	SHIW
Add mortgage	Binary variable taking value 1 if a household does not have a mortgage with the (main) bank in wave $t-1$, but has a mortgage in wave t , and 0 otherwise.	SHIW
Broad relationship	Binary variable taking value 1 if a household uses more than 1 bank service (in addition to a bank account), and 0 otherwise.	SHIW
Age, Age ²	Integer variables representing the age of the household head and its quadratic form.	SHIW
Male	Binary variable taking value 1 for a male household head, 0 for female.	SHIW
Married	Binary variable taking value 1 if the household head is married, and 0 otherwise.	SHIW
Education dummies	Binary variables taking value 1 for the corresponding level of education: Edu_2 – elementary school Edu_3 – middle school Edu_4 – high school Edu_5 – bachelor’s degree Edu_6 – post-graduate qualification The reference category is having no education at all.	SHIW
Household size	Categorical variable counting the number of household members	SHIW
Income (Net wealth) quintiles	Binary variables taking value 1 if the household’s yearly disposable income (net wealth, defined as the sum of real and financial assets, net of liabilities) is within the relevant distribution quintiles, and 0 otherwise.	SHIW
Homeowner	Binary variable taking value 1 if the household owns his primary residence, and 0 otherwise	SHIW

Variable	Description	Data source
Employee, Self-employed	Binary variables taking value 1 for the household heads being in the corresponding occupational status, 0 otherwise. Reference category is “non-working position”.	SHIW
Commercial, Cooperative, Savings	Binary variables taking value 1 for the corresponding bank’s specialization. The reference category is Commercial bank.	BS
Return on assets (ROA)	Variable representing the ratio between the bank’s pre-tax profits and assets, in percentage points.	BS
Size	Bank's total assets, in logs.	BS
M&A	Dummy variable taking value 1 if the bank underwent a process of Merge & Acquisition between $t - 1$ and t , and 0 otherwise.	BS
Regional dummies	Dummy variables taking value 1 for the relevant macro-region (North-West, Centre, South), and 0 otherwise. The reference category is North-East.	SHIW
Municipality size	Categorical variable representing the size of the residential municipality: 1 = less than 5,000; 2 = [5,000-20,000]; 3 = [20,000-50,000]; 4 = [50,000-200,000]; 5 = more than 200,000 The model specifications include four dummies for municipality size from 2 to 5, i.e., the reference category is 1 (municipality size less than 5,000)	SHIW
Competition proxy	Normalized Herfindahl index of banks’ ATM points in the province of the household residence, ranging between 0 (perfect competition) and 1 (monopoly).	BI

Note: *SHIW is Survey on Household Income and Wealth; BS is Bankscope; BI is Bank of Italy.*