The Supply Side of Household Finance*

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Using matched borrower-lender data, we document strong nonprice supplier effects in mortgage contract choice. For given relative price of adjustable and fixed rate mortgages, households borrowing from banks hit by shocks to the cost of long term funding, or to the deposits base or to access to securitization are more likely to choose adjustable rate mortgages. Supply factors have larger effects on less-sophisticated households and at times of price inaction. A model in which banks affect borrowers' choices through prices and distorted advice predicts these findings. We contrast the distorted advice interpretation of the evidence against the potential alternative nonprice channels. (*JEL* D14, E43, G11, G12, G21)

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This paper proposes a data-based methodology to assess the presence of nonprice, supplier-induced distortions in households' financial choices on mortgages focusing on a specific channel: lenders biased advice to borrowers. When households have limited knowledge about financial products suitability to their needs, observing prices may not be enough for them to make a choice

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and may have a strong incentive to ask for experts' advice. They often rely on the supplier of the financial product itself to obtain counsel.[1] The problem is that advisors may have an incentive to distort their recommendations to serve their own needs rather than those of their customers, who may have little or no ability to detect this conflict of interest.[2]

To study supplier-induced nonprice effects on individual financial decisions we look at the choice between fixed rate (FRM) and adjustable rate mortgages (ARM) using credit-register matched bank-borrower data on a sample of 1.6 million mortgages originated in Italy by 132 banks between 2004 through 2010. Besides information on the terms of the loans and on the characteristics of the households, the data identifies the bank originating the mortgage and shows its balance sheet and a rich set of characteristics. On top of the high quality of the data, studying the Italian mortgage market is well suited to the purpose of this study because of a number of institutional characteristics. Namely, two main products are available to customers, plain-vanilla fixed and adjustable rate mortgages, and both are popular. Advice is usually provided by the banks issuing the mortgages (rather than brokers), and banks retain mortgages that they originate on their balance sheets. This means that Italian banks have both motive and opportunity to provide biased advice.

The idea of the test is simple. On the hypothesis that banks have heterogeneous relative advantages in offering the two types of mortgage (e.g., some banks have access to cheaper long-term financing and thus have a relative advantage in offering FRM) we suppose that they may influence households' mortgage choices in the direction that is more advantageous to them. If a borrower is "sophisticated," the only supplier variable affecting his choice should be the relative cost of FRMs and ARMs faced: this price should be a sufficient statistic to influence a given household's mortgage choice. Thus, controlling for the relative price of the two types of mortgage, within banks variation over time in their cost characteristics should play no role. Differences over time in banks' efficiency in supplying FRMs and ARMs should be revealed in their relative prices and affect household's choice through this channel alone.[3]

On the other hand, if some households are naive and rely on supplier advice, relative prices are in general no longer a sufficient statistic for mortgage contract

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[1] Hung et al. (2011) report that 73% of U.S. investors rely on professional advice to conduct stock market or mutual fund transactions. About 60% of the investors in the 2007 Unicredit Clients Survey, a survey on a sample of Italian investors, rely on the help of an intermediary as advisor when making financial decisions, and only 12% decide without counsel.

[2] A number of papers (e.g., Inderst 2010; Inderst and Ottaviani 2010, 2012a, 2012b; Carlin and Manso 2011; Ottaviani and Squintani 2006; Kartik, Ottaviani, and Squintani 2007) set forth the theoretical underpinnings of the literature on how advice affects unsophisticated households' financial choices when intermediaries who give the advice are in conflict of interest.

[3] The importance of bank-specific fixed effects in a mortgage choice equation may reflect market sorting. For this reason, our test focuses on time-varying bank characteristics, which should be irrelevant for households' mortgage choices once prices are controlled for even in cases in which fixed effects matter. We will discuss this point in detail at various point in the paper.

choices. If banks exploit the conflict of interest by biasing advice, the time-varying characteristics of the bank should affect households' choice, apart from any effect that time variation in supply characteristics may exert via the relative prices of ARMs and FRMs. Our strategy is to test the null hypothesis that the mortgage choice is unaffected by price-relevant bank characteristics when households' characteristics and the relative prices of the two types of mortgage are controlled for.

Like Koijen, Van Hemert, and Van Nieuwerburgh (2009), we find that the choice between ARM and FRM is strongly affected by the relative prices of FRM and ARM. But we also find that time variation in bank specific measures of the relative advantage in originating FRM and ARM does predict mortgage type choice even when the relative price is controlled for. This result holds controlling for bank fixed effects which capture any sorting of customers into banks based on unobserved stable characteristics. Hence, identification of the presence of nonprice supplier distortions only relies on time-varying characteristics that measure changes over time in banks' incentives to recommend a particular type of mortgage. For example, time variation in the bank bond spread, which measures changes in the banks' relative cost to provide fixed rate mortgages, has a direct effect on household mortgage choices, in addition to the effect it has through the relative prices of FRM and ARM. This is consistent with the hypothesis that banks with a relative disadvantage in providing FRMs try to influence households' decisions in favor of ARMs, not only by making the latter cheaper but also by distorting advice. We find similar effects for time variation in the bank deposit basis and in securitization (a positive shock to either one increases the chance a borrower chooses an FRM). Economically, the effect of these distortions is significant. For example, a 1-standard-deviation increase in the bank bond spread lowers the probability of a household choosing a fixed rate mortgage by 3.3 percentage points. The magnitude of this effect, though important, is one-half as great as that of a 1-standard-deviation increase in the relative price of FRMs, suggesting a limit to how much banks can distort choice through nonprice channels.

To obtain further insights, we develop a model of the mortgage market, where banks originating FRM and ARM mortgages and subject to a set of supply shifters, choose the rates and the advice to offer to borrowers that differ in sophistication. To validate our interpretation, we also exploit two implications of the model. First, the effect of distorted advice should be stronger among unsophisticated consumers; second, supplier characteristics should distort choices more when there are frictions in adjusting prices. Consistent with these implications, we find that banks supply factors have a greater effect on the mortgage choices of unsophisticated consumers; further, these effects are stronger during periods when relative prices do not change.

Our paper is closely related to the recent wave of work trying to detect distortions in financial advice. Existing papers have taken two approaches: the first compares the investment performance of individuals who rely on advice with that of those who do not (e.g., Hackethal, Haliassos, and Jappelli 2012; Hackethal, Inderst, and Meyer 2010) or with some benchmark (Foester et al. 2017). These papers find that advised accounts underperform nonadvised individuals once the cost of the advice is taken into account. This is consistent with the hypothesis of biased advice. But the result is also consistent with less capable investors choosing to get advice and nevertheless being unable to overcome the deficit in ability or to make proper use of the advice received (Battacharya et al. 2012).[4] A second approach, which should deal with this problem, uses randomized field experiments, tracking the recommendations that trained auditors, posing as customers, receive from financial advisors. It finds evidence consistent with biased advice (e.g., Mullainathan, Nöth, and Schoar 2012; Anagol, Cole, and Sarkar 2012).[5] Both type of studies look at cases in which advice is *sought* by the investors and is observed. In practice, however, advice—especially distorted advice—may be offered even when it is not actually solicited by the customer: for example, the intermediary may emphasize a given financial product steering the households' choice to the intermediary's advantage. If so, comparing customers who do and do not solicit advice may fail to detect supply-side distortions or produce an underestimate of their importance.

Our approach to detect supplier nonprice distortions does not require information on whether a household asked for advice or even received it unilaterally, so we can detect its effects even when advice is not observed. Of course, this requires some identifying assumptions. But, as we will show, they are milder than those required by comparisons of advised and nonadvised accounts and potentially testable.

While we focus on mortgage choice distortions induced by biased advice, lenders can distort mortgage choice through other nonprice channels, such as advertising—like in Gurun, Matvos, and Seru (2016)—or affect it through rationing. Because these channels can in principle generate correlations between individual mortgage contract choice and lender supply shocks like those implied by biased advice, our evidence can broadly be taken as documenting strong lender nonprice influences in mortgage choice. But we discuss differential implications of the advice and these alternative channels and bring evidence that seems to speak against advertising and rationing.

1. The Model

In a standard demand framework, prices are a sufficient statistic for the effect of supply factors on consumer choices. We use a simple model to refute this

[4] Though advised investors do worse than the unadvised, they may do better than they would by choosing on their own. Advice may still help unsophisticated investors avoid mistakes or mitigate behavioral biases (Shapira and Venezia 2001; Gennaioli, Shleifer, and Vishny 2015). This possible benefit cannot be detected by comparing investors who rely on advice with those who do not.

[5] In this context one may doubt that the audited advisors would offer the same biased advice in the kind of long-term client relationships that one finds in the real world.

property where the lender can give biased advice and apply it to the choice between fixed rate and adjustable rate mortgages. If a consumer is unsure about which of the two mortgages best fits her needs, the bank can opportunistically bias her choice by giving advice. If the advice is followed, variables that are correlated with the bank's incentives predict consumer choices even controlling for prices: two households with identical characteristics facing the same prices may make different choices if they get different advice. Because *biased* advice is uniquely determined by lender profitability, supply factors will affect consumer choices regardless of prices.

1.1 Households

A continuum of households live for two periods, and they all need to finance a house purchase. Households have constant absolute risk aversion (CARA) utility and differ in risk aversion γ. G denotes the distribution of risk aversion across households. Income is constant over time; nominal interest rates follow a random walk; and inflation is unpredictable. Under these assumptions (as is shown by Koijen et al. 2009), household γ chooses an adjustable rate mortgage (ARM) over a fixed rate mortgage (FRM) if and only if

$$\phi > \frac{\gamma H}{2}(\sigma_\varepsilon^2 - \sigma_\pi^2),$$

where ϕ is the FRM premium, H is the value of the house, σ_ε^2 is the variance of interest rates and σ_π^2 is the variance of inflation. In the first section of the Online Appendix (OA henceforth), we illustrate the full derivation of the above decision rule. We normalize $H = 2$ and $\sigma_\varepsilon^2 - \sigma_\pi^2 = 1$ so that the household decision rule is $\phi > \gamma$. The normalization does not affect the results qualitatively. Under these assumptions, $G(\phi)$ households choose ARMs and $1 - G(\phi)$ choose FRMs.

1.2 Banks

The economy has a continuum of regions with one bank in each region. Customers cannot borrow from other regions and the distribution of risk aversion is G in every region. Under these assumptions each bank is a local monopolist.[6] Banks have fixed balance sheet size and fixed liabilities. They can only determine asset composition, choosing between long-term FRM and ARM. Every bank i is characterized by exposure to N supply factors $(\theta_1, \ldots, \theta_N)$. Banks are heterogeneous in their exposure to these factors. The bank has a payoff function[7] $U(x, \phi, \theta)$ that depends on the share x of short-term assets (i.e., adjustable rate mortgages), the FRM premium and supply factors. The bank takes θ as given and chooses x and ϕ.

6 Our qualitative results also hold under more general market structures, as long as banks have some market power. What really matters for us is the bank's ability to choose both prices and advice: thus, the absence of perfect competition is sufficient for our result. Market power may affect the quantitative importance of biased advice. Section 5 investigates this empirically.

7 We call it payoff rather than profit, because banks' choices typically include an adjustment for risk.

1.3 No advice

Under these assumptions, in absence of advice the problem of a bank choosing the fraction x of short-term assets and the relative price ϕ can be written as

$$\max_{x,\phi} U(x,\phi,\theta)$$

s.t.

$$x = G(\phi)$$

Because the bank has market power, the objective function can be rewritten as $v(\phi,\theta) \equiv U(G(\phi),\phi,\theta)$ so that the optimal FRM premium $\phi(\theta)$ is determined by the first-order condition:[8]

$$v_\phi(\phi(\theta),\theta) = 0.$$

This simply leads us to our first result:

Proposition 1. In absence of advice, the household's mortgage choice is independent of bank supply factors conditional on the relative prices of ARM and FRM. In particular, $E(m|\phi) = E(m|\phi,\theta)$, where m denotes mortgage choice.

Prices depend on supply factors, which affect household choice only through this channel, not otherwise. Because bank supply factors are orthogonal to risk aversion,[9] they add no information, beyond relative mortgage prices, to household choices. In a model with no advice, prices encapsulate all the relevant supply characteristics. Appendix A houses the proofs of this and the following propositions.

1.4 Advice

To model advice we assume that a fraction μ of the banks' customers are naive. They do not know what their decision rule should be. Thus, it is within the scope of well-informed banks to provide counseling.[10] The rest of the population is

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[8] We take interest rates as given and only allow the bank to set the relative price of FRM and ARM. Clearly, if we let banks choose the interest rate level, they would charge an infinite rate, because households must choose one of two mortgages. This is assumed for simplicity. It can be easily solved by allowing households to rent at a certain rental rate instead of owing. This option would limit the interest rate that the monopolist bank can charge. Nothing relevant changes in our analysis if we add this feature.

[9] In the model orthogonality between supply factors and borrowers risk aversion is by construction. In reality, sorting can break this lack of correlation. We discuss how we deal with this in Section 2.2; in Section 4.2 we show evidence consistent with this assumption.

[10] If households don't know what is best for them, advice is valuable. We do not model "good advice." This is not a limit of this model or of our econometric test because, by definition, good advice should reflect household-specific factors (e.g., their level of financial knowledge or—like in Gennaioli et al. 2015—of their "anxiety") and as such should not depend on bank characteristics. In our model advice should be interpreted as suggestions beyond (or short of) what would be needed to make up for the customer's ignorance. Put this way, in our model all advice is biased advice.

sophisticated: they understand their decision rule. Naiveté is independent of risk aversion and is private information, so that the bank cannot distinguish between naive and sophisticated borrowers. The bank can choose an optimal distortion α in the decision rule. This means that where biased advice has been given, the household's decision rule becomes:

$$\phi - \alpha > \gamma$$

so that a bank that tilts the decisions toward ARMs will choose $\alpha > 0$, and one distorting it toward FRMs will choose $\alpha < 0$. Because sophistication is unobservable, the bank gives the same advice to all the customers. Naive customers just follow the advice, sophisticated ignore it (they know what is best for them). Additionally, they realize that the bank has tried to mislead them, so that when it gives advice to a wary customer the bank suffers a reputation loss. We call this cost $c(\alpha,\mu,\theta)$. Under these assumptions, the share of customers effectively choosing ARMs is: $g(\phi,\mu,\alpha) = \mu G(\phi-\alpha) + (1-\mu)G(\phi)$ so the problem becomes:

$$\max_{\alpha,\phi} v(\phi,\alpha,\theta,\mu) \equiv \max_{\alpha,\phi} (U(g(\phi,\mu,\alpha),\phi,\theta) - c(\alpha,\mu,\theta))$$

The bank's choices $\alpha(\theta)$ and $\phi(\theta)$ solve the pair of first-order conditions:

$$v_\alpha(\phi(\theta),\alpha(\theta),\theta,\mu) = 0,$$

$$v_\phi(\phi(\theta),\alpha(\theta),\theta,\mu) = 0.$$

Here, the N bank-specific factors θ affect both the optimal distortion and the mortgage price. In this case, θ may have an independent role in determining mortgage choice even after the price ϕ has been controlled for. This is because an observed variable (prices) and a latent one (advice) affect choices. Adding θ to a regression of mortgage choice on prices may add information on the unobserved value of α. This result does not always hold: if prices are a sufficient statistic for the effect of θ on α, they would capture everything that the econometrician needs to know about α to predict mortgage choice, so that θ would play no *detectable* independent role and the existence of distorted but unobserved advice would not be inferred. We can give the following definition:

Definition: The above model satisfies the sufficient statistic property (SSP) if there exists a unidimensional sufficient statistic of the supply factors that fully determines α and ϕ. That is, if there exists a real-valued function $y = f(\theta)$ such that $\phi = h_1(y)$ and $\alpha = h_2(y)$.

If the model satisfies the SSP, knowing prices *and* advice gives the same information as knowing only prices or advice. Therefore, θ has no additional predictive power on mortgage choice once ϕ is controlled for. The following proposition clarifies the conditions under which we can identify the presence of advice.

Proposition 2. If the model does not satisfy SSP, household choices depend on the factors θ even after prices are controlled for. In other words, $E(m|\phi,\theta) \neq E(m|\phi)$, where $E(m|)$ is the conditional expectation of the household decision. (Proof. See Appendix A.)

Under SSP, $E(m|\phi) = E(m|y) = E(m|\phi,\theta)$ so that the result in Proposition 2 fails. If $N=1$ the SSP is mechanically satisfied with $f(\theta)=\theta$: with only one supply factor, the factor itself is the sufficient statistic. In short, for the econometrician advice is a latent choice variable. For this reason, whenever distortionary advice is unobserved, supply factors generally matter for consumer mortgage choice even conditioning on prices. But if there is a sufficient statistic of supply factors that determines banks' price and advice choices, the test fails, in that observing prices and advice gives the same information. In OA.2, we provide examples in which the SSP does not hold, and, thus, our test strategy can reveal biased advice. In Section 4.1, we show evidence that the SSP does not hold in our data.

1.5 Price rigidity

Because advice is just a soft communication it is extremely flexible. On the other hand, prices may be more difficult to change (we discuss evidence of mortgage price sluggishness in Section 3.2.2). Hence, prices and advice may differ in responsiveness to supply factors. We show that if prices are less flexible than advice, one can infer the presence of biased advice from the correlation of mortgage choices with supply factors even when SSP holds. To see why, consider a small menu cost of changing prices.[11] If supply conditions change only modestly, banks find it optimal not to change prices, so that all movements in θ are reflected in movements in α and supply effects on consumer mortgages reveal biased advice. Moreover, the magnitude of the effect of θ on α may increase: if a bank cannot adjust prices, it is giving up the natural channel to twist demand toward the product it prefers. The alternative way to influence demand is to give advice, which thus under price rigidity, becomes a substitute for resetting prices. This can be summarized in the following proposition:

Proposition 3. Under price rigidity, $E(m|,\phi,\theta) \neq E(m|\phi)$. Moreover, price rigidities may amplify the effects of supply factors because advice substitutes for pricing in distorting demand. (Proof. See Appendix A.)

[11] We model price stickiness as a menu cost only for convenience. Any other friction in resetting rates would work equally well. For example, "internal coordination costs" arising in a complex organization, where several people at the top must agree to change the FRM/ARM spread, or communication costs inside the bank, particularly those with a geographically dispersed organizations such as banks with many branches.

2. Empirical Strategy

The model clarifies the conditions under which it is possible to test for biased advice. In particular, we establish that if supply factors affect prices and advice differently enough, a regression of household mortgage choice on supply factors that affect banks' funding access and costs—controlling for prices—should find an important role not only for prices but also for biased advice. In this section, we illustrate our empirical strategy to test for the presence of biased advice and discuss the assumptions that enable us to identify the effect of advice.

2.1 Basic specification

We run the following baseline regression:

$$x_{ibt} = \beta_1 \phi_{ibt} + \beta_2 z_{ibt} + \beta_3 B_{bt} + f_b + f_t + u_{ibt}, \tag{1}$$

where x_{ibt} denotes the mortgage choice of customer i at bank b at time t and ϕ_{ibt} is its relative price. z_{ibt} is a set of customer-specific covariates and B_{bt} a set of bank-specific supply factors (corresponding to the $\theta's$ in the model); f_b, f_t are bank and time fixed effects, and u_{ibt} is an error term. We denote the choice of FRM by $x_{ibt} = 1$ and ARM by $x_{ibt} = 0$. We include ϕ and z because they are natural determinants of choices, and B to test for advice. Controlling for f_b and f_t helps us to identify the presence of advice, as explained below. Our test of advice relies on the economic and statistical importance of coefficients in β_3: biased advice makes these coefficients significant and their sign should be as predicted by the bank's incentives. Specification (1) makes it clear that the effect of advice on mortgage choice is identified only if household-specific unobserved heterogeneity is not correlated with time-varying bank supply factors. First, time-varying factors other than prices affect mortgage choices even in the absence of advice. For example, changes in interest rate volatility simultaneously affect choices and banks' balance sheets. These time-varying factors tend to be aggregate, not bank-specific, so that adding a time effect takes care of them.

Another potential problem is sorting: one might argue that more risk-averse consumers tend to be found at banks that are better able at managing interest rate risk, creating a correlation between choices and supply factors regardless of advice, unless individual risk aversion or banks' relevant characteristics are observed. To account for this, we include bank-specific fixed effects. The idea is that any sorting should take place through bank characteristics that are stable over time: while one could argue, for example, that larger banks attract more risk-averse customers, it is implausible that quarterly changes in securitization activity or the share of deposits in total funding at a given bank are key drivers of borrowers' choice of the bank to apply for a mortgage. Therefore, the association of stable bank characteristics with different borrower pools is consistent with identification, in our model, as long as time-varying bank-specific supply factors do not affect the composition of such pools. Formally, identification requires that

$$E(u_{ibt}, B_{bt}|\phi_{ibt}, z_{ibt}, f_b, f_t)=0. \qquad (2)$$

In other words, the unobserved characteristics u_{ibt} of the consumers who borrow in quarter t from bank b, should bear no systematic relation with the variation from quarter to quarter in the bank specific component of supply factors, once we control for the relative price of the mortgages, fixed bank characteristics, common time effects and borrowers' observable characteristics. This requires that borrowers *not be sorted* into banks purely on the basis of quarterly change in some bank-specific supply factor, such as the cost of long-term funding. We regard this as a mild and reasonable assumption. We discuss this assumption further in Sections 4.2 and 5 and provide supportive evidence.

2.2 Specification with price rigidity

The model in Section 1 carries two further implications about the observables. First, the correlation between bank supply factors and mortgage choice, controlling for prices, should be stronger where there is some price rigidity. As we show in Section 3, our data exhibit evidence of price adjustment inaction, so we can test for this implication by estimating

$$x_{ibt} = \beta_1\phi_{ibt} + \beta_2 z_{ibt} + \beta_3 B_{bt} + \beta_4 D_{bt} \times B_{bt} + f_b + f_t + u_{ibt}, \qquad (3)$$

obtained adding the term $\beta_4 D_{bt} \times B_{bt}$ to the baseline model, where D_{bt} is a dummy for price inaction in bank b at time t. Based on the model, we expect the effect to be stronger during periods of inaction, so that β_4 should be significant and of the same sign as β_3, reinforcing the effect of bank-specific supply shocks. To reiterate, the intuition for this is that banks have a stronger incentive to distort advice in response to supply shocks when they cannot change prices. Second, the effect of supply shocks should be stronger for less sophisticated customers as they are more dependent on advice.

Before leaving this section, we highlight an important role of price inaction in helping identification. Two additional instances may lead to a failure to identify condition (2). First, in case ϕ_{ibt} is subject to a measurement error. If the relative price of FRM and ARM is measured with error, because the true price is correlated with the bank supply factors, B_{bt} will capture part of the true variation in the relative price and show significance even without biased advice. Second, when some price-relevant demand controls are omitted from the model: in this case the price ϕ_{ibt} also captures the effect of these omitted factors on mortgage choice. This implies that ϕ_{ibt} is no longer sufficient to characterize bank supply conditions. Hence, supply factors B_{bt} may become significant because they are correlated with the true price even without distorted advice.

But under price inaction, these biases disappear. In fact, in both instances the bias arises because of the correlation between ϕ_{ibt} and B_{bt}. Price inaction breaks this correlation and allows one to identify the presence of distorted advice even if ϕ_{ibt} is measured with error or if demand controls are omitted.[12]

3. Institutional Features and Data

3.1 Institutional features

We have argued that the Italian mortgage market is well suited to the purpose of this study because of a number of institutional characteristics. First, two main products are available to customers, plain-vanilla fixed and adjustable rate mortgages, and both are popular. Second, banks are the main originators of mortgages and act as advisors for their customers. Third, banks retain on their balance sheets the mortgages that they originate. Fourth, origination fees are small and independent of the type of contract, so banks have little motivation to originate mortgages just to cash in fees. But because they bear the interest rate risk, banks have incentive to steer customers either toward FRM or ARM at origination. This means that banks in Italy have both motive and opportunity to steer borrowers' choices, thus making the Italian mortgage market a good laboratory to detect the presence of distorted advice. Details of these and other institutional features are discussed at greater length in the third section of the OA.

3.2 Data

We use data from two main administrative sources: the Italian Credit Register (CR) and the Survey on Loan Interest Rates (SLIR). Both data sets are administered by the Bank of Italy. The first collects information on the loan exposures above the threshold of Euro 75,000 originated by all Italian banks. A subset of 132 banks active in the mortgage market participate in the SLIR. We have obtained quarterly data on all the mortgages originated between 2004 and 2010 for all the 132 banks that participate in the SLIR. The data set has complete records on around 1.9 million mortgages. Excluding contracts with a partially adjustable interest rates and maturity of less than 10 years (103,814 observations), mortgages granted on special terms or conditions (13,470 observations) and loans to sole proprietorships (160,574 observations) we were left with 1,662,429 observations on plain vanilla FRMs or ARMs (see OA.4 for details). The data set contains information on the type of loan (FRM or ARM), the contractual rate (which, if present, includes rate discounts) and the original loan size. Concerning the loan maturity, we only know whether it is above 10 years, but we do not observe the exact maturity. However, we

12 Suppose that ϕ is measured with error ξ, the variance of which is σ_ξ^2 . The ordinary least squares (OLS) estimate of β_3 will be $\hat{\beta}_3 = \frac{\sigma_\xi^2 \sigma_{\phi B}}{\sigma_\phi^2 \sigma_B^2 - \sigma_{\phi B} + \sigma_\xi^2 \sigma_B^2} \beta_1 + \beta_3$, where σ_X^2 is the variance of X; this estimate is clearly biased, unless the covariance between the bank supply shocks and the FRM premium $\sigma_{\phi B}$ is zero. This is the case when the FRM spread does not vary even if supply factors move.

know from survey data that the vast majority of the originated mortgages have a maturity ranging between 10 and 25 years. As it is typical in administrative data we observe a number of borrower characteristics, but the set is limited. In addition, we have the identifier of each of the originating banks; and, most importantly, we can merge the mortgage data set with detailed supervisory data on banks' characteristics and balance sheets. Finally, we complement the mortgage-originator data with information on the structure of the local market, the local market power of the bank and the distance between the bank's headquarter and the borrower residence. In the end, our data set includes features of borrowers, lenders, the specific terms of the mortgage, and information on the local market.

3.2.1 Computing the relative price of FRM. Two views consider the best gauge of the long-term finance premium (LTFP), the relative price of $FRMs$ and $ARMs$ in a household's mortgage choice. Campbell and Cocco (2003) posit that the choice of liquidity constrained households is driven by the current difference in funding costs, defined as the spread between FRM and ARM rates ($r^{FRM} - r^{ARM}$). Using panel data for nine countries, Badarinza et al. (2014) find empirical support for this view.

Koijen et al. (2009) propose an alternative measure of the LTFP. The mortgage's choice is driven by the time-varying FRM risk premium, defined as the difference between the fixed rate and expected future average values of the ARM rate ($r^{FRM} - E(r^{ARM})$). This spread is ordinarily positive, as borrowers pay a premium to be shielded from interest rate increases. Because they only have aggregate data, they proxy the FRM risk premium by the long-term bond risk premium, computed as the difference between the 10-year bond yield and the expected 1-year bond yield, proxying expectations about the latter with a moving average of past yields.

In our analysis we compute both measures at the borrower-bank level. In particular, we calculate (1) $Spread = r_{ibt}^{FRM} - r_{ibt}^{ARM}$ and (2) FRM risk premium$= r_{ibt}^{FRM} - E(r_{ibt}^{ARM})$ for household i borrowing from bank b at time t. Because we observe the interest rate on the chosen mortgage at time of origination, we can rely on both time series and individual-specific variation in the relative cost of the two types of loans.[13] Obviously, while we observe the rate on the mortgage actually chosen by individual i and originated by bank b—say a FRM (ARM)—we do not observe the rate on the alternative type of mortgage at the bank. We overcome this problem by imputing the rate that customers would have been charged had they chosen an ARM (FRM). For this, we group customers that chose FRM and ARM, respectively, and

[13] For instance, the adjustable rate mortgage is given by the 1-month interbank rate plus an individual-specific credit spread. The first reflects time-varying market conditions and is common to all borrowers choosing ARM in a given quarter from a certain bank, but can potentially vary across banks; the second reflects individual-specific creditworthiness and differs in the cross-section of borrowers that obtain an ARM in the same quarter.

then run a sequence of regressions, one for *each* bank, of the rate charged on each type of loan on loan characteristics, borrower characteristics, and a full set of time dummies. We then use the estimated parameters to impute the interest rate to the specific household (for details on the imputation, see OA.5). Consider the following three key points. First, because we run bank-specific regressions any systematic interest rate difference across banks is reflected in the imputed interest rate. Second, because each regression includes a full set of time dummies, any effect on interest rates of any time-varying bank-specific variable is also reflected in the imputed rate, in particular, any variation in its supply factors. Thus, the residual difference between the true rate the consumer would have faced on the alternative mortgage and the imputed rate reflects only unobserved borrower-specific characteristics. This measurement error may create attenuation bias in the estimated effect of the relative price of FRM on mortgage choice but is orthogonal to the time varying bank variables that we will use as proxies for the incentive to distort advice.[14]

Finally, to compute the FRM risk premium $r^{FRM} - E(r^{ARM})$ we follow Koijen et al. (2009) and measure $E(r^{ARM})$ using different lags and leads of the ARM rates. Clearly, zero lag coincides with the current spread. Figure 1 shows that, like in Koijen et al. (2009), the 1-year lag measure of the FRM risk premium has the greatest predictive power for the ARM share using either aggregate data (the light color bars) or individual data (the darker bars). Hence, we will use this as our reference measure. But notice the very close correlation of ARM share with the current spread.

Panel A in Table 1 reports summary statistics for the actual and imputed rates together with other information on the mortgage contract. The rest of the table reports summary statistics on the borrower (panel B), the balance sheets of the lenders (panel C) and the bank-borrower relationship (panel D). OA.4 and OA.5 provide more information.

3.2.2 Identifying price inaction. A vast literature in banking documents sluggishness in the adjustment of bank interest rates to market conditions (Freixas and Rochet 1997, for a review). To identify periods of inaction in setting the relative price of $FRMs$ and $ARMs$ in our sample we look at the quarter to quarter changes in spread, $r_{bt}^{FRM} - r_{bt}^{ARM}$, the price that banks control. For each bank and quarter sample, we compute it by first taking averages across borrowers of the rates charged on the two types of mortgages originated by the bank. The left side of Figure 2 shows the cross-sectional distribution of $\Delta Spread = \Delta(r_{bt}^{FRM} - r_{bt}^{ARM})$ over the whole sample (2004–2010), before the

[14] We have cross-validated our imputation procedure by leaving out a certain fraction of the observations for which we know the rate, for them impute the rate, and then compute the imputation error as the difference between the true and imputed rate. We find a contained mean error, implying that our imputation method is reliable. Furthermore, we find that the imputation error is always uncorrelated with the bank supply factors implying that the effect of the latter on mortgage choice, that we document in Section 4, cannot be attributed to imputation error in the LTFP. OA.4 discusses details about these checks.

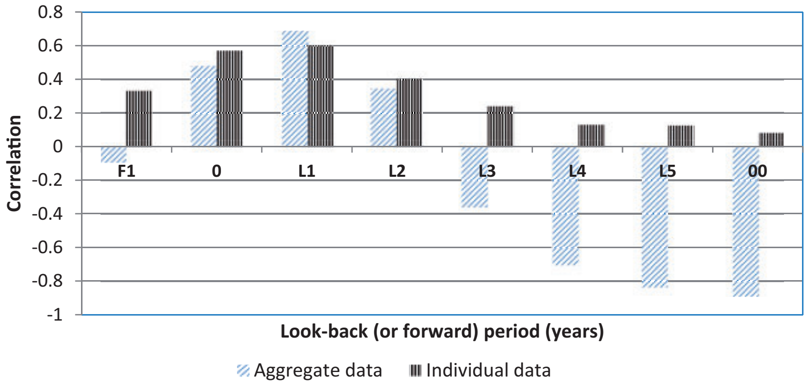

Figure 1
Correlation between the ARM share and alternative measures of the "FRM risk premium"
The figure shows the correlation between alternative measures of the FRM risk premium and the ARM share. The shaded bars are correlations computed on aggregate data; the black bars use data at the bank-client level. The FRM risk premium is the difference between the FRM rate and the expected value of the interbank rate. This is calculated under various assumptions about the horizon: a forward-looking horizon of 1 year (F1), the actual value (0), a backward-looking horizon of 1, 2, 3, 4, and 5 years (L1 to L5) and an infinite horizon (∞) approximated using the whole sample. The correlation at 0 is the correlation with the current FRM/AMR. Correlations are calculated over the period January 2004 through December 2010.

global financial crisis (2004–2007) and during it (2008–2010). In all periods the distribution has a spike around zero, consistent with infrequent adjustments of relative mortgage prices.[15] The distribution tends to be symmetric around zero, except during the financial crisis, when it shows a fat tail to the right: that is because following the Lehman Brothers' default Italian banks had trouble issuing fixed rate bonds, which resulted in a higher cost of $FRMs$ (Levy and Zaghini 2010). Therefore, part of the adjustment of the spread reflects changes in the slope of the yield curve. Filtering these out,[16] the distribution of the changes in the relative price of FRM and ARM becomes symmetric around zero (Figure 2, right side). This confirms that most of the changes during the crisis reflect an increase in the cost of fixed-term borrowing common to all banks.

Our main indicator of price inaction for bank b in quarter t is a dummy equal to 1 if $\Delta(r_{bt}^{FRM} - r_{bt}^{ARM})$ lies between $\pm\frac{sd}{3}$, where sd is the standard deviation of the spread of bank b. [17] Using this definition, banks are inactive about 40%

[15] Because we are considering the average spread over quarters, the change may slightly differ from zero because of time aggregation. For instance, if adjustment takes place in the last 10 days of the quarter, the change in that quarter will not be exactly zero. Accordingly, we define inaction as a change in the spread within a small interval around zero.

[16] The slope of the yield curve is obtained by taking the difference between the 15-year swap rate and the 1-month interbank rate.

[17] For robustness we also compute alternative measures. First, we define inaction using a tighter threshold, namely $\pm\frac{sd}{4}$. Second, we define inaction as the case in which the change in the spread of bank b in a given quarter falls

Table 1
Descriptive statistics of the main variables used in the estimation

Variables	Obs.	Mean	SD	Median	P10	P90
A. Contracts' characteristics						
Fixed rate mortgage contract	1,662,429	0.303	0.460	0.000	0.000	1.000
Mortgage size (log)	1,662,429	11.734	0.441	11.733	11.280	12.206
Joint mortgage	1,662,429	0.509	0.500	1.000	0.000	1.000
Interest rate actual:						
- FRM rate	504,407	5.545	0.834	5.713	4.606	6.376
- ARM rate	1,158,022	3.829	1.181	3.775	2.227	5.530
Interest rate fitted:						
- FRM rate	1,158,022	5.106	0.482	5.133	4.403	5.959
- ARM rate	504,407	4.723	1.107	5.270	2.690	5.697
Spread (1)	1,662,429	1.138	0.952	0.725	0.063	2.536
FRM risk premium (2)	1,662,429	1.172	1.055	0.938	−0.107	2.433
B. Borrowers' characteristics (3)						
Italian	1,662,429	0.893	0.294	1.000	0.500	1.000
Cohabitation (4)	1,662,429	0.206	0.405	0.000	0.000	1.000
Age (years)	1,662,429	38.165	9.302	37.000	27.500	51.000
Female	1,662,429	0.435	0.356	0.500	0.000	1.000
C. Banks' characteristics						
Supply shift factors:						
Deposit funding ratio % (5)	1,662,429	44.441	20.444	46.124	10.494	67.448
Securitization activity dummy (6)	1,662,429	0.398	0.489	0.000	0.000	1.000
Bank bond spread (7)	1,662,429	0.283	0.496	0.276	−0.390	0.960
Other characteristics:						
Leverage ratio % (8)	1,600,446	6.449	2.524	6.238	3.582	10.578
Mutual bank dummy	1,662,429	0.005	0.072	0.000	0.000	0.000
Delinquency ratio %(9)	1,662,410	3.489	2.278	3.140	0.957	8.301
Bank size (log)	1,662,429	10.215	1.436	10.144	8.230	12.174
Group dummy	1,662,429	0.918	0.275	1.000	1.000	1.000
Foreign subsidiary dummy	1,662,429	0.051	0.219	0.000	0.000	0.000
Patti Chiari (10)	1,662,429	0.632	0.482	1.000	0.000	1.000
D. Bank-borrower relationship (11)						
Distance 1 (province)	1,662,429	0.152	0.359	0.000	0.000	1.000
Distance 2 (region)	1,662,429	0.264	0.441	0.000	0.000	1.000
Distance 3 (same area)	1,662,429	0.185	0.388	0.000	0.000	1.000
Distance 4 (elsewhere)	1,662,429	0.400	0.490	0.000	0.000	1.000
Concentration index (12)	1,662,429	60.152	7.386	59.837	51.508	68.819
GDP per capita (thousands euros) (13)	1,662,429	10.190	0.236	10.273	9.742	10.387

(1) Difference between the FRM rate and the ARM rate. (2) Difference between the FRM rate and expectation of the ARM rate. The latter is based on the 1-year moving average of the 1-month interbank rate. (3) Average across individuals in the case of joint mortgages. (4) In case of joint mortgage. (5) Deposits over total liabilities. (6) Dummy that takes the value of 1 if the bank is active in the securitization market in a given quarter. (7) Difference between the cost of fixed rate bank bonds and variable rate bonds. (8) Tier1 capital over total assets. (9) Bad loans over total loans. (10) Dummy that takes the value of 1 if the bank takes part to the "Patti Chiari" initiative, whose main objective is to simplify bank-borrower relationship. (11) We control for the distance between the lending bank headquarters and household residence by four dummy variables: DIST1 is equal to 1 if borrower k has his residence in the same province where bank j has its headquarters; DIST2 is equal to 1 if (a) DIST1=0 and (b) firm k is resident in the same region, where bank j has its headquarters; DIST3 is equal to 1 if (a) DIST2=0 and (b) borrower k is resident in the same geographical area, where bank j has its headquarters; and DIST4 is equal to 1 if DIST3=0. (12) Market share of the first five banking groups in each province. (13) At the regional level. The sample period is 2004:Q1–2010:Q4.

of the time with considerable heterogeneity in the number of price adjustments (Figure A4 in the OA). This finding is robust to changes in the inaction measure, while hazard rates for keeping price unchanged are decreasing over time,

within $\pm\frac{1}{3}$ of the standard deviation of the change in the spread in the pooled data (see Figure A1 in the OA). All results go through using these measures and are reported in the OA.6.

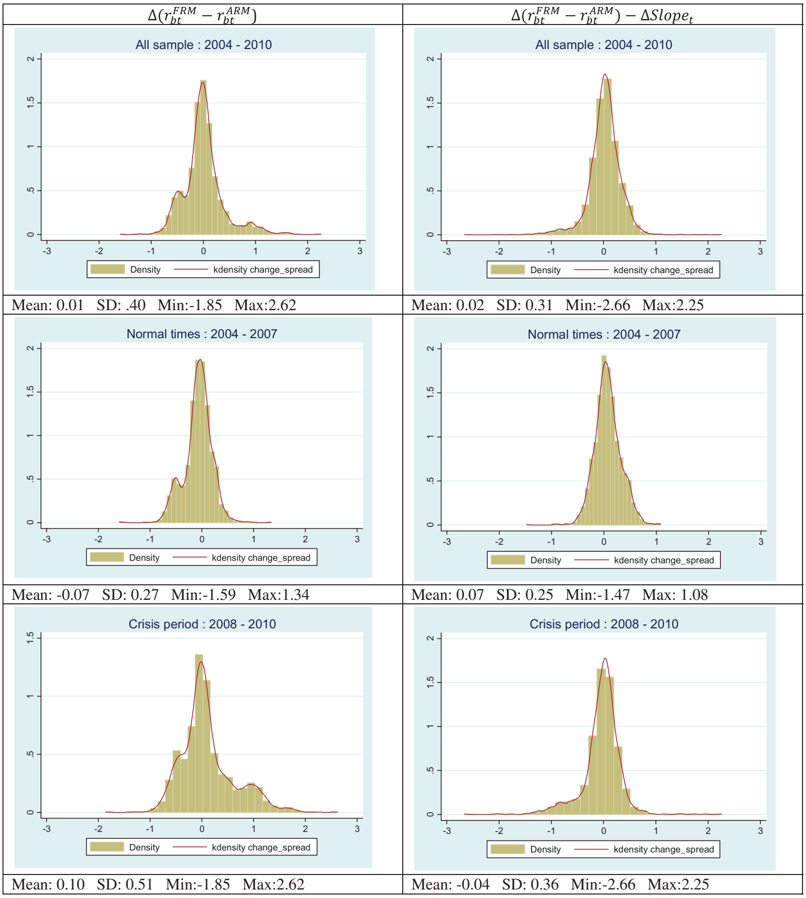

Figure 2
Distribution of the change of the spread between FRM and ARM
This figure shows the distribution of the quarterly changes in the FRM/ARM spread in the cross-section of banks for the whole sample and two subperiods. The second column shows the distribution net of the change in the slope of the yield curve (Slope), computed as the difference between the 15-year swap rate and the 1-month interbank rate.

consistently with the baseline menu cost models (Figure A2 in the OA). Last, to check the reliability of our dummy for inaction constructed from quarterly data, we have compared it with the changes in the spread computed using actual mortgage rates charged by one of the three largest Italian banks. We find that our measure closely overlaps with that from the actual mortgage rates with a correlation of 92% and shows inaction in 31% of the quarters (details in OA.5).

3.2.3 Banks' supply factors. We use three measures for the bank supply factors that should affect the relative appeal of FRM and ARM. The first is the bank bond spread - the premium the bank pays for raising long-term funding via fixed rate over variable rate bonds. Banks that pay a higher premium face a higher cost of supplying FRM and should therefore have an incentive to steer borrowers toward ARM. For most of the banks in our sample, we observe both rates; some small banks are not always active in both the fixed and variable rate bond markets. For those quarters in which these banks were inactive in a specific segment we impute the rate using the bank-specific spread (with respect to the market rate) the last time they were active in that segment. We show that results do not depend on this imputation.

The second measure is a proxy for banks' access to securitization. Fuster and Vickery (2015) show that the share of fixed rate mortgages is positively related to access to securitization. By allowing banks to liquidate some of their assets, securitization enhances asset allocation flexibility and so makes long-term investments more palatable. These banks should have a relative advantage in originating $FRMs$ vs $ARMs$ and should accordingly, bias their advice toward the former. We proxy access to securitization with a dummy variable equal to 1 if the bank has sold securitized loans on the market in the last quarter.

The third measure is the share of deposits to total funding. Because individual depositors face higher switching costs than institutional investors, banks that can count on core deposits can be slower and less complete in adjusting their funding to changing market conditions than banks whose liabilities consist mainly of market funding, which respond rapidly and fully to market movements (Berlin and Mester 1999). Hence, the former are less exposed to market risk and so are better able to withstand greater maturity mismatching. Being less subject to interest rate risk, banks with a large deposit base should have a relative advantage over banks with a small deposit share in issuing $FRMs$ versus $ARMs$ and may be expected to bias their advice accordingly. This is consistent with Berlin and Mester (1999) and Ivashina and Scharfstein (2010), who found that banks with better access to rate-inelastic core deposits more commonly engage in loan rate smoothing (i.e., relationship lending).

In sum, when estimating Equations 1 and 3, we expect β_3 and β_4 both to be negative if the bank supply factor considered is the bank fixed bond spread and both to be positive if it is securitization activity or the deposit ratio. Table 1, panel C, shows summary statistics of our supply factors.

Before leaving this section, we document that in periods in which banks do adjust prices, supply factors affect their pricing. To check for this, we run bank-level regressions of the FRM/ARM spread on our three bank supply factors. The results reported in Table 2 show that bank supply factors affect the FRM/ARM spread during periods in which prices are adjusted. The first three columns report regressions of the FRM/ARM spread on the supply factors inserted one at a time; all estimates control for banks fixed effects and time fixed

Table 2
Bank supply factors affect banks' pricing

Dependent variable is FRM-ARM spread (1)	(1) Cost of bond finance	(2) Securitization	(3) Deposit strength	(4) All supply factors	(5) Inaction periods
Bank bond spread (2)	0.1651***			0.1555***	−0.0001
	(0.0457)			(0.0464)	(0.0583)
Securitization activity (3)		−0.2346**		−0.224*	0.0245
		(0.1141)		(0.1301)	(0.1855)
Deposit ratio % (4)			0.0105	0.0094	0.0045
			(0.0056)	(0.0058)	(0.0061)
Additional controls (5)	Yes	Yes	Yes	Yes	Yes
Bank fixed effects (BFE)	Yes	Yes	Yes	Yes	Yes
Time fixed effects (TFE)	Yes	Yes	Yes	Yes	Yes
Observations	2,156	2,156	2,156	2,156	1,389
Adjusted R-squared	.5576	.5545	.5566	.5610	.6521
Price action (6)	Yes	Yes	Yes	Yes	No

The left-hand-side variable is the average FRM-ARM spread for a bank in a given quarter. Robust standard errors (clustered at bank level) are reported in parentheses. (2) Difference between the cost of fixed rate bank bonds and variable rate bonds. (3) Dummy equal to 1 if the bank is active in the securitization market in a given quarter and zero elsewhere. (4) Deposits over total liabilities. (5) Include (a) Provincial lending concentration measured by the market share of the top-five banking groups; (b) a dummy if the bank participates in the "Patti Chiari" initiative; and (c) GDP per capita (weighted for bank activity at the provincial level). The sample period is 2004:Q1–2010:Q4. (6) The sample is divided between periods of price action and periods of price inaction. Price inaction periods are those quarters where for bank b the change in the FRM/ ARM spread fall in the rage $\pm\frac{sd_b}{3}$, where the standard deviation is specific to each bank. Periods of price action are all the remaining. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

effects.[18] The bank bond spread and the securitization dummy predict changes in the FRM/ARM spread, the first positively and the second negatively. The impact of the deposit ratio is not statistically significant. Results are qualitatively similar when the supply factors are all entered simultaneously (third column). Obviously, there is no price response during periods of price inaction as the last column shows. Importantly, the results in Table 2 imply that the sufficient statistic property (SSP) does not hold so that the advice channel and the price channel can be separately identified. Notice that although the deposit ratio has no effect on pricing, securitization activity has a negative effect. As we will show in the next section, they affect mortgage choice through advice in the same direction, which is sufficient for the SSP to fail (see OA.2).

3.2.4 Other controls. In estimating (1) and (3), we control for characteristics of the mortgage (amount, whether it is a joint mortgage), borrower specific variables (age, gender, and dummies for Italian nationality and cohabitation) that capture part of the heterogeneity in consumer preferences; characteristics of the local market (lending concentration measured by the province market share of the top 5 banking groups), and a measure of borrower-lender relationship

[18] We also control for a measure of local market concentration, average gross domestic product (GDP) per capita (weighed for bank activity at the provincial level), and bank participation in a bank's association initiative to simplify bank-borrower relations (called Patti Chiari, see next section).

(the distance between borrower's residence and lender's headquarter). We also consider a dummy for those banks that joined the "Patti Chiari" (Clear Deals) initiative launched in 2003 by the Italian Banking Association to simplify bank-borrower relations. Panels C and D of Table 1 report the summary statistics for these variables.

4. Results

Before estimating our baseline model (1), Table 3 reports OLS estimates of various specifications of households' mortgage contract choice. Because probit

Table 3
Do lender characteristics affect mortgage choice?

	(1) Only bank fixed effects (BFE)	(2) BFE and long-term financial premium (LTFP)	(3) BFE+ LTFP + region time fixed effects (TFE)	(4) BFE+ RTFE+ borrowers' characteristics (BC)	(5) Complete model	(6) BFE and long-term financial premium (LTFP)
		LTFP = FRM risk premium (1)				LTFP = Spread (2)
Long-term financial premium (LTFP)		−0.131***	−0.066***	−0.067***	−0.067***	−0.107***
		(0.012)	(0.006)	(0.006)	(0.006)	(0.011)
Mortgage size (log)				−0.081***	−0.081***	
				(0.008)	(0.008)	
Joint mortgage				0.028***	0.028***	
				(0.003)	(0.003)	
Italian				0.046***	0.045***	
				(0.008)	(0.008)	
Cohabitation				−0.002*	−0.002*	
				(0.001)	(0.001)	
Age (years)				−0.001***	−0.001***	
				(0.0001)	(0.0001)	
Female				0.012***	0.012***	
				(0.001)	(0.001)	
Bank fixed effects (BFE)	Yes	Yes	Yes	Yes	Yes	Yes
Region-time fixed effects (RTFE)	No	No	Yes	Yes	Yes	No
Other controls (3)	No	No	No	No	Yes	No
Test of BFE joint significance (p-value)	.000	.000	.000	.000	.000	.000
Test of RTFE joint significance (p-value)	–	–	.000	.000	.000	–
Test on BC joint significance (p-value)	–	–	–	.000	.000	–
Observations	1,662,429	1,662,429	1,662,429	1,662,429	1,662,429	1,662,429
Adjusted R-squared	.0984	.1455	.3433	.3507	.3511	.1362

The table shows the parameter estimates of a linear probability model of mortgage type choice. The left-hand-side variable is a dummy = 1 if the borrower chooses an FRM and 0 otherwise. The sample period is 2004:Q1–2010:Q4. Robust standard errors (clustered at bank level) are reported in parentheses. Coefficients for dummies and fixed effects are not reported. (1) In columns 2–5, the LTFP is the difference between the FRM rate and the expected ARM rate based on borrower's actual ARM rate and one year moving average of the 1-month interbank rate. (2) In column 6, the LTFP is the difference between the FRM rate and current the ARM rate. (3) Include (a) provincial lending concentration measured by the market share of the top-five banking groups; (b) a dummy if the bank participates in the "Patti Chiari" initiative; and (c) dummies to control for the distance between the lending bank headquarters and household residence. *$p<.1$; **$p<.05$; ***$p<.01$.

estimates are known to be biased when there are a large number of fixed effects (Lancaster 2000) and because in probit regressions interaction effects are not readily interpreted, given the importance of both fixed and interaction effects to our identification strategy, in the rest of the paper we estimate linear probability models and compute standard errors adjusted for clustering at the bank level. The left hand side is a dummy variable equal to 1 for FRM and 0 otherwise. The first column controls only for bank fixed effects. Systematic differences across banks can explain about 9.8% of the variance and bank fixed effects are jointly highly significant. The second column adds the long-term financial premium measured using the FRM risk premium. As expected, this variable has a negative effect on mortgage choice, and it is highly significant (p-value $< 1\%$). Interestingly, while the bank fixed effects continue to be statistically significant, when the relative price is added the explanatory power increases considerably: the model can explain about 14.6% of the variance. This is consistent with the role that theory attributes to relative prices. Economically, a 1-standard-deviation increase in the relative cost of $FRMs$ (about 1 percentage point), lowers the probability of choosing this type of contract by as much as 13.8 percentage points ($1.055\times(-0.131)$). The correlation in Column 2 between mortgage choice and relative price captures both variation over time in the relative cost of $FRMs$ common to all banks as well as variation over time specific to the bank (systematic differences in relative prices across banks are picked up by the bank fixed effects). Column 3 includes a full set of region-time dummies so that the variation in the relative price of $FRMs$ is now strictly bank specific. Notice that because the expectations about future short-term rates used to compute the average expected ARM rate are common to all individuals, they are absorbed by the region-time fixed effects; thus, the variation in the FRM risk premium reflects that in the current spread. When we rely only on this source of variation, the marginal effect on the relative price is negative and significant, and also lower (a 1-standard-deviation increase in the spread lowers the probability of choosing a FRM by 7.1 percentage points ($1.055\times(-0.067)$). This is in line with Koijen et al. (2009) that estimate an impact of 7–8 percentage points of an increase in the risk premium by 1 percentage point. Adding region-time fixed effects also improves the fit ($R^2=0.343$), suggesting relevant time-varying common variables, apart from the FRM risk premium, such as changes in the relative riskiness of the two types of mortgage contract, are captured by the time effects. Adding borrower specific controls (Column 4) and a measure of local market concentration (Column 5) brings little explanatory power and leaves the marginal effect of the relative price unchanged. Columns 6 replicates the estimates in Column 2 using the current spread as a measure of the $LTFP$. The results are very similar to those using the FRM risk premium although the latter yields a marginally better fit. Hence, in the rest of the paper, we simply take the FRM risk premium as our gauge of the $LTFP$.

Overall, this evidence assigns a key role to the relative price as a driver of mortgage contract choice. This point is made by Koijen et al. (2009). But it

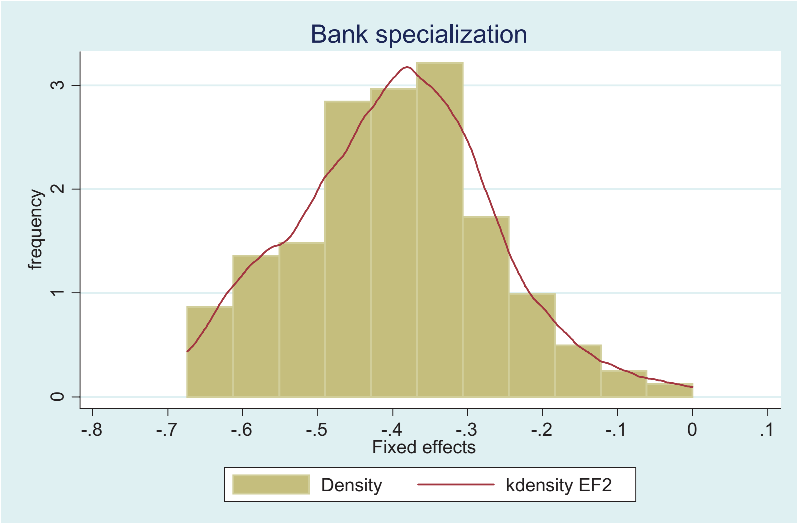

Figure 3
Pattern of bank specialization in the mortgage market
The figure shows the distribution of the bank fixed effects obtained from the regression in Table 3, Column 5. Banks in the bottom decile of the distribution (13 banks, 9.6% of the market) are defined as specialized in ARM mortgages; banks in the top decile of the distribution (13 banks accounting for 14.1% of the market) are defined as specialized in FRM.

also reveals some systematic effects of fixed characteristics of the mortgage originator. This may simply reflect sorting, or it may reflect lenders' systematic ability to shift consumer choices not via prices. To shed some light on the importance of sorting we retrieve the bank fixed effects from the estimates in Table 3, Column 5, whose distribution is shown in Figure 3. The figure suggests some heterogeneity in the pattern of banks specialization: some banks mainly originate $FRMs$, others mainly $ARMs$. The vast majority, however, tend to originate both. We then compute the means of the observable borrower characteristics for banks that tend to originate mostly $FRMs$ (the top decile of the distribution of the bank fixed effects), mostly $ARMs$ (the bottom decile of the distribution of the bank fixed effects) and of those that tend to originate both. Means and variances are reported for the whole sample and for our two subperiods (2004–2007 and 2008–2010). As can be seen from Table 4, there is no difference in the distribution (summarized by mean and standard deviation) of any observable borrower characteristic neither across the three types of banks nor over time for a given type of bank. Although sorting could of course occur as a result of unobservables, the fact that the distributions of observable borrower characteristics are so similar across banks and over time makes this a less likely possibility. Even so, in our tests we always include bank fixed effects and identify supplier nonprice effects only out of bank-specific time variation in supply factors. In Sections 4.2 and 5, we discuss sorting in greater detail.

Table 4
Borrowers' characteristics for specialized and nonspecialized banks

	Observations	Mortgage size (log)		Joint mortgage (%)		Italian (%)		Cohabitation (%)		Age (years)		Female (%)	
		Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance
All sample													
a) Banks specialized in ARM	172,026	11.724	0.258	0.518	0.249	0.907	0.076	0.212	0.166	37.899	84.677	0.441	0.124
b) Nonspecialized banks	1,349,016	11.738	0.189	0.502	0.250	0.890	0.089	0.205	0.162	37.970	85.378	0.434	0.127
c) Banks specialized in FRM	237,945	11.718	0.179	0.557	0.247	0.917	0.068	0.218	0.170	39.567	90.935	0.436	0.116
Ho: Mean (a) = Mean (c) (p-value)		(.992)		(.956)		(.980)		(.992)		(.898)		(.992)	
Ho: Var (a) = Var (c) (p-value)			(.894)		(.765)		(.804)		(.753)		(.731)		(.788)
2004–2007													
a) Banks specialized in ARM	70,632	11.672	0.301	0.529	0.249	0.891	0.088	0.220	0.171	37.441	88.586	0.439	0.120
b) Non-specialized banks	827,510	11.716	0.187	0.513	0.250	0.875	0.101	0.212	0.167	37.646	85.803	0.431	0.125
c) Banks specialized in FRM	129,234	11.693	0.181	0.562	0.246	0.898	0.083	0.224	0.174	39.010	91.050	0.434	0.116
Ho: Mean (a) = Mean (c) (p-value)		(.984)		(.974)		(.994)		(.997)		(.866)		(.996)	
Ho: Var (a) = Var (c) (p-value)			(.931)		(.767)		(.787)		(.756)		(.750)		(.779)
2008–2010													
a) Banks specialized in ARM	101,394	11.760	0.224	0.510	0.250	0.918	0.066	0.206	0.163	38.219	81.704	0.443	0.126
b) Non-specialized banks	521,506	11.773	0.191	0.487	0.250	0.914	0.069	0.193	0.155	38.492	84.181	0.440	0.131
c) Banks specialized in FRM	108,711	11.749	0.177	0.549	0.247	0.938	0.049	0.210	0.166	40.218	90.022	0.441	0.118
Ho: Mean (a) = Mean (c) (p-value)		(.992)		(.969)		(.983)		(.997)		(.828)		(.998)	
Ho: Var (a) = Var (c) (p-value)			(.852)		(.766)		(.872)		(.755)		(.720)		(.790)

The table shows the first and second moment of borrowers' observable characteristics for three types of banks. (a) Banks specialized in ARM; (b) nonspecialised banks; and (c) banks specialised in FRM. The three groups have been identified based on the method described in Figure 3. Banks in the first decile of the distribution (13 banks, 9.6% of the market) are defined as specialized in ARM mortgages; banks in the last decile of the distribution (13 banks accounting for 14.1% of the market) are defined as specialized in FRM. The others are nonspecialized. p-values of the test that the mean (or the variance) in group (a) is equal to that in group (c) are reported in parenthesis. The overall sample period is 2004:Q1–2010:Q4.

Table 5
Time-varying bank characteristics and mortgage choice

Dependent variable is the linear probability that the borrower chooses a FRM	(1) Baseline model including bank supply factors	(2) Sample of banks with bond spread always observed	(3) Adding nonlinear terms for LTFP	(4) Banks operating in all provinces	(5) Period before global financial crisis
LTFP	−0.0623***	−0.0625***	−0.0523***	−0.0703***	−0.0482***
	(0.0053)	(0.0053)	(0.0114)	(0.0079)	(0.0036)
$LTFP^2$			−0.0020		
			(0.0066)		
$LTFP^3$			0.0002		
			(0.0014)		
Bank bond spread	−0.0678***	−0.0633***	−0.0700***	−0.0737***	−0.0367***
	(0.0108)	(0.0106)	(0.0116)	(0.0164)	(0.0139)
Securitization activity	0.0310***	0.0221**	0.0285***	0.0337**	0.0243***
	(0.0104)	(0.0107)	(0.0102)	(0.0161)	(0.0088)
Deposit ratio %	0.0016*	0.0022**	0.0016*	0.0022**	0.0017*
	(0.0009)	(0.0009)	(0.0009)	(0.0011)	(0.0010)
Bank fixed effects (BFE)	Yes	Yes	Yes	Yes	Yes
Region-time fixed effects (RTFE)	Yes	Yes	Yes	Yes	Yes
Borrowers' characteristics (BC)	Yes	Yes	Yes	Yes	Yes
Other controls (1)	Yes	Yes	Yes	Yes	Yes
Test on BFE joint significance (p-value)	.000	.000	.000	.000	.000
Test on RTFE joint significance (p-value)	.000	.000	.000	.000	.000
Test on BC joint significance (p-value)	.000	.000	.000	.000	.000
Observations	1,662,389	1,424,059	1,662,389	957,961	862,705
Adjusted R-squared	.3557	.3652	.3566	.3779	.2851

The table shows linear probability estimates of mortgage choice. The left-hand-side variable is a dummy=1 if an FRM is chosen and 0 otherwise. Robust standard errors (clustered at bank level) are reported in parentheses. In Columns 1–4, the sample period is 2004:Q1–2010:Q4. In Column 5, the sample period is 2004:Q1–2007:Q2. Coefficients for borrowers' characteristics and fixed effects are not reported. Table 2 reports the definition of the variables. (1) Includes (a) provincial lending concentration measured by the market share of the top-five banking groups; (b) a dummy if the bank participates in the "Patti Chiari" initiative; and (c) dummies to control for the distance between the lending bank headquarters and household residence. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

4.1 Baseline model estimates

Table 5 shows the estimates of our baseline model (1). The first column uses the complete specification of Table 3 (Column 5) and adds the fixed rate bank bond spread, the securitization activity dummy and the deposit ratio as measures of time-varying banks supply factors. Not only are these variables statistically significant (the bank bond spread with p-values $< 1\%$) but also their sign is consistent with the nature of the banks' incentives that they are intended to reflect, as discussed in Section 3.2.3. A high fixed rate bond spread lowers the chances that the borrower will opt for a fixed rate mortgage, while the bank's ready access to loan securitization and its ability to rely on core deposits for funding both increases the likelihood of the borrower's taking an FRM. Because the estimates control for the relative price of FRM and ARM, these effects are additional to any effect of lender supply factors on the spread.

Taken together this evidence is consistent with the hypothesis that banks respond to changes in funding conditions both by adjusting prices and by giving biased advice.[19] The fact that customers' choice is correlated with these bank variables is also consistent with models of naive consumers, like in Ottaviani and Squintani (2006) and Kartik et al. (2007), while it tells against models of uninformed but smart customers which predict that advice will not distort choice (like in Crawford and Sobel 1982). Our results suggest that the mortgage market is more likely to be populated by genuinely naive customers than by uninformed borrowers who rationally anticipate that their bank will be offering biased advice. If that were the case, the biased advice would not be credibly transmitted and it would therefore not distort behavior.

Compared with the response to changes in relative mortgage prices the effect of lender supply factors is smaller, as one would expect, but far from negligible. Considering the results in the first column of Table 5, a 1-standard-deviation increase in the fixed rate bank bond spread lowers the probability of the borrower opting for a FRM (through the biased advice channel) by 3.4 percentage points (0.496×–0.0678), which is roughly half of the effect of an increase in the $LTFP$ of that size. A one standard deviation increase in the quarter-to-quarter variation in securitization activity increases the probability of a borrower choosing a FRM in that quarter by 1.5 percentage points; it increases by 3.3 percentage points if the quarter-to-quarter specific variation in the bank deposit ratio increases by one sample standard deviation.

In Column 2 of Table 5, we run the estimates using only the banks for which we actually observe the fixed rate bank bond spread in all relevant quarters, thus avoiding imputations. The results are unchanged. One problem is that the banks' supply factors might be capturing nonlinear effects of the relative price of $FRMs$ versus $ARMs$ in the household's decision problem. To address this concern, Column 3 adds a quadratic and a cubic term in the $LTFP$. The results do not warrant the concern: the effect of the bank supply factors is unchanged, both statistically and economically. Column 4 runs the model only for the banks present in all provinces. We do this to assess possible biases due to sorting (see the next section). The results are unchanged, qualitatively and quantitatively.[20] Finally, Column 5 restricts the sample to the 2004–2007 period. We do not observe the exact maturity of the mortgage, so one may worry that banks adjust their pricing of shorter versus longer maturity mortgages (to manage their interest rate risk) when their supply factors change, but this might not get picked up in the rate imputation. Because during the 2004–2007 period the 10- to 20-year Italian yield curve is flat and the premium is contained (less

[19] The correlation could reflect reverse causality: that is, banks faced with a stronger demand for $FRMs$ securitize more and try to attract more deposits. We have two answers to this observation. First, a current shift in the relative demand for $FRMs$ is unlikely to be able to cause a response in securitization and in the deposit base in the same quarter; second, and most importantly, reverse causality cannot explain the effect of the bank bond spread. An increase in the relative demand for $FRMs$ would trigger an increase in the issues of fixed rate bonds (to match maturities) and presumably an increase in the bond spread, giving rise to a *positive* correlation between FRM share and bond spread. This is the opposite of what we find.

[20] Our results are unchanged if we use a more flexible specification allowing for all borrower observables as well as the time fixed effects to be interacted with the FRM-ARM spread.

than 40 basis points), we would expect that if this effect is present it plays a minor role. As can be seen results are unaffected. All supply shifters retain sign, size and significance very close to the estimates in the whole sample.

It is worth emphasizing the thought experiment that underlies the identification of biased advice in our estimates. Take the effect of the fixed rate bank bond spread. The estimate of this coefficient results from comparing the choices of customers at a given bank in a given quarter facing a given FRM spread with the choices of the customers of the *same* bank in a different quarter, possibly facing a different FRM spread and observing that customers that choose the contract in a quarter in which the bank faces a higher cost to attract long-term funding tend (once the component of the costs common to all banks is filtered out) to opt for fixed rate mortgages. In making this comparison, we take into account the fact that the pools of customers in different quarters may have different observable characteristics, and we interpret the result of the comparison as evidence that banks bias advice to distort the mortgage contract choices of their customers to their own advantage. That is when the cost of long-term funding increases relative to short-term funding, the bank tends to recommend $ARMs$ so as to reduce exposure to interest rate risk. This interpretation rests on the identifying assumption that the variation in the unobservable characteristics of the pools of borrowers from one quarter to the next is not correlated with the quarterly change in the fixed rate bank bond spread. A similar argument applies to the deposit ratio and to securitization activity. That is borrowers do not sort into banks according to *time-varying* supply factors.[21] As this is the key identification assumption in our empirical model, we discuss it further in the next section.

4.2 Sorting

Unobserved heterogeneity due to sorting of customers by time invariant bank characteristics is inconsequential for the estimates, because this is accounted for by the bank fixed effects. Furthermore, as shown in Table 4, when we split banks according to pattern of specialization in $FRMs$ or $ARMs$, we find that the mean and variance of our six household-specific characteristics do not vary across subsamples, which suggests that even sorting by stable characteristics is unlikely to play a role.

However, there could be sorting by the *time-varying* component of the bank supply factors, a possibility that we have excluded by imposing it as our identifying assumption. For this assumption to fail and for our results accordingly to be driven by sorting depending on time-varying supply factors, the distribution of risk aversion (or other borrowers characteristics that affect mortgage choice) would have to react to quarterly changes in supply factors because borrowers choose the bank where to apply for a mortgage conditioning

[21] Note that the theoretical model does not allow for sorting, because all banks face the same pool of borrowers.

Table 6
A test for the presence of "dynamic" sorting

Explanatory variables	Mortgage size (log)	Joint mortgage	Italian	Cohabitation	Age	Female
	Dependent variables					
Bank bond spread	0.0005	0.0012	−0.0079	0.0034	−0.1227	−0.0020
	(0.0035)	(0.0023)	(0.0056)	(0.0023)	(0.0774)	(0.0013)
Deposit ratio	0.0003	−0.0001	−0.0002	−0.0001	−0.0014	0.0000
	(0.0003)	(0.0002)	(0.0005)	(0.0004)	(0.0091)	(0.0001)
Securitization activity	0.0079	0.0090	−0.0016	−0.0058	−0.2730	0.0035
	(0.0109)	(0.0070)	(0.0014)	(0.0065)	(0.4049)	(0.0031)
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-time effects	Yes	Yes	Yes	Yes	Yes	Yes
F-test on joint significance of bank-specific characteristics (p-value)	.4833	.2710	.2414	.4817	.4556	.4250
Observations	1,662,429	1,662,429	1,662,429	1,662,429	1,662,429	1,662,429
R-squared	.0427	.0199	.0604	.0135	.0333	.0022

The table reports the results of regressions of customers' observable characteristics on time-varying bank-specific characteristics, controlling for bank, time, and province fixed effects. The sample period is 2004:Q1–2010:Q4. Robust standard errors (clustered at the bank level) are reported in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

on the supply factors. This does not seem like a plausible mechanism, if only because customers have limited access to banks' balance sheet data. In other words, our key identification assumption is that the composition of borrowers at a given bank does not vary with its balance sheet. To further strengthen this assumption, we look for evidence of this kind of sorting in our sample. Table 6 seeks to explain household-specific observable characteristics at a given bank using our three time-varying supply factors (while controlling for bank and region-time fixed effects). No coefficient is significant.

One critique of our check is that it is based on few observables and sorting may be due to unobserved heterogeneity, in particular differences in risk aversion. We will face this issue upfront in Section 5 by showing direct evidence that a measure of risk aversion does not correlate with bank supply factors and by running panel data regressions that control for borrower unobserved heterogeneity. Here, we notice that the evidence so far suggests that it unlikely that this mechanism drives the result.

Taken together, the evidence in Tables 4 and 6 suggests that different banks face a similar pool of borrowers that does not change with balance-sheet variables. As we discuss in Section 5, this helps to address two potential alternative explanations of our findings.

4.3 Results with price inaction

Table 7 reports the estimates of model (3) which adds to the baseline model (1) interaction terms between the three bank supply factors and a dummy equal to 1 if in a given quarter the bank kept the FRM/ARM spread unchanged. The model predicts greater reliance on advice—hence a greater direct effect of bank supply factors in household contract choice—in periods of price inaction. Table 7 uses our reference measure to define price inaction. In all

Table 7
The role of price inaction

Main definition of price inaction (threshold ($\pm\frac{sd_b}{3}$)				
	(1) Baseline model including bank supply factors	(2) Sample of banks with bond spread always observed	(3) Adding nonlinear terms for LTFP	(4) Banks operating in all provinces
LTFP	−0.0599***	−0.0596***	−0.0510***	−0.0560***
	(0.0048)	(0.0048)	(0.0112)	(0.0074)
LTFP^2			−0.0008	
			(0.0065)	
LTFP^3			0.0002	
			(0.0014)	
Bank bond spread	−0.0555***	−0.0521***	−0.0581***	−0.0636***
	(0.0106)	(0.0103)	(0.0113)	(0.0152)
Securitization activity	0.0302***	0.0226**	0.0278***	0.0311*
	(0.0103)	(0.0106)	(0.0101)	(0.0159)
Deposit ratio %	0.0014*	0.0019**	0.0013*	0.0017*
	(0.0009)	(0.0010)	(0.0008)	(0.0009)
D_{ib}(1)	−0.0188	−0.0333	−0.0187	−0.0266
	(0.0263)	(0.0277)	(0.0267)	(0.0276)
Bank bond spread * D_{ib}	−0.0583***	−0.0571***	−0.0571***	−0.0614***
	(0.0128)	(0.0134)	(0.0127)	(0.0191)
Securitization activity * D_{ib}	0.0319**	0.0390**	0.0321**	0.0323*
	(0.0148)	(0.0162)	(0.0148)	(0.0185)
Deposit ratio % * D_{ib}	0.0004	0.0004	0.0005	0.0007
	(0.0005)	(0.0005)	(0.0005)	(0.0006)
Bank fixed effects (BFE)	Yes	Yes	Yes	Yes
Region-time fixed effects (RTFE)	Yes	Yes	Yes	No
Borrowers' characteristics (BC)	Yes	Yes	Yes	Yes
Other controls (2)	Yes	Yes	Yes	Yes
Observations	1,662,389	1,424,059	1,662,389	957,961
Adjusted R-squared	.358	.367	.359	.381

The table shows linear probability estimates of mortgage choice. The left-hand-side variable is a dummy=1 if an FRM is chosen and zero otherwise. The sample period is 2004:Q1–2010:Q4. Robust standard errors (clustered at the bank level) are reported in parentheses. Coefficients for borrowers' characteristics and fixed effects are not reported. Table 2 reports the definition of the variables. (1) Price inaction: dummy D_{ib} =1 in quarters where bank b the change in the FRM/ARM spread fall in the rage $\pm\frac{sd_b}{3}$, where the standard deviation is specific to each bank. (2) Include (a) provincial lending concentration measured by the market share of the top-five banking groups; (b) a dummy if the bank participates in the "Patti Chiari" initiative; and (c) dummies to control for the distance between the lending bank headquarters and household residence. *$p<.1$; **$p<.05$; ***$p<.01$.

specifications the interaction with the price inaction dummy has the same sign as that of the specific supply factor—thus reinforcing its effect—and is statistically significant in most of the cases. The effect is particularly strong for the fixed rate bank bond spread and the securitization activity: in quarters in which the bank does not adjust the FRM spread ($D_{bt}=1$), the effect roughly doubles. For the deposit ratio the differences in marginal effects between times of price inaction and the overall average for all quarters are more limited and not statistically significant.

As was observed in Section 2, studying the effect of bank supply factors under price inaction not only permits a valid test of a relevant implication of the biased advice model even when only one supply factor is available but overcomes two potential objections. The first is that supply factors may become significant only because the relative mortgage price is measured with error. The rate on the mortgage not chosen is imputed, so this may be a concern. The bias arises because supply factors are correlated with the relative mortgage price (measured with error), so price constancy breaks this correlation and allows a neat identification of the nonprice channel. The second objection is that this procedure might omit demand controls that also affect relative mortgage prices. Because the omitted controls end up in the error term, they will bias the coefficient of relative price; and because the relative price and supply factors are correlated, the latter's effect may become significant, independently of distorted advice. Though the region-time fixed effects should capture these demand shifts, it is still questionable whether they capture them all. Under price inaction, this source of bias is eliminated so if supply factors nevertheless still affect mortgage choice, this is clearly due a supplier nonprice effect. These results are confirmed if we use the more stringent definition of price inaction (OA, Table A4).

Hence, on this ground too we conclude that the evidence is consistent with a significant role of nonprice effects when households choose between $FRMs$ and $ARMs$.

4.4 Financial sophistication and mortgage complaints

The model in Section 1 predicts that banks supply factors bias the mortgage choice of unsophisticated customers more than those of sophisticated borrowers. To test this implication, we estimate model (1) separately for samples of sophisticated and unsophisticated customers. To proxy for borrowers' sophistication, we rely on variation in education across provinces. We use the province share of households with a bachelor's degree as our proxy for financial sophistication and run regressions for sophisticated and unsophisticated borrowers by focusing on residents in provinces in the top 25% and bottom 25% of the distribution across provinces of the financial education index, respectively.[22]

The left panel of Table 8 shows the estimates for the two samples. Sophisticated and unsophisticated borrowers equally respond to the relative price of FRM and ARM. But the two groups show substantially different responses to the bank supply factors. Unsophisticated borrowers display a 56% stronger negative response to increases in the fixed rate bond spread and the difference between the two groups is statistically significant (the test for the difference in shown in the third column). The overall response of mortgage choice to the securitization activity indicator and to the core deposit ratio is positive for both groups but is more than twice as large for the unsophisticated

[22] To ensure enough variability because we use province means, we split the distribution in financial education by the top and bottom 25%. Results (unreported) are similar if we split between the top and bottom 5% and are qualitatively similar if we proxy sophistication with the size of the loan and distinguish between experienced borrowers (who have borrowed from some banks in the past) and inexperienced borrowers (who are applying for a loan for the first time).

Table 8
Different degree of client sophistication and mortgage complaints

Dependent variable is the probability that the borrower chooses a FRM	*A. Test based on different degree of client sophistication*			*B. Test based on different number of mortgage complaints among banks*		
	(a) Sophisticated borrowers	(b) Unsophisticated borrowers	Difference |b-a| H0: |b-a| >0	(c) Low complaint banks	(d) High complaint banks	Difference |c-d| H0: |c-d| >0
Long-term financial premium	−0.0680***	−0.0630***	0.005	−0.0418***	−0.0619***	0.020**
(*LTFP*)	(0.0075)	(0.0064)	(0.010)	(0.0039)	(0.0082)	(0.009)
Bank bond spread	−0.0501***	−0.0780***	0.028*	−0.0520***	−0.0860***	0.034**
	(0.0167)	(0.0103)	(0.020)	(0.0112)	(0.0160)	(0.020)
Securitization activity	0.0438***	0.1186***	0.075**	0.0208**	0.0443***	0.024**
	(0.0175)	(0.0365)	(0.040)	(0.0090)	(0.0110)	(0.014)
Deposit ratio %	0.0011	0.0023***	0.0012*	0.0006	0.0023***	0.002*
	(0.0008)	(0.0004)	(0.0009)	(0.0010)	(0.0008)	(0.001)
Bank fixed effects (BFE)	Yes	Yes		Yes	Yes	
Region-time fixed effects (RTFE)	Yes	Yes		Yes	Yes	
Borrowers' characteristics (BC)	Yes	Yes		Yes	Yes	
Other controls (1)				Yes	Yes	
Observations	328,900	338,603		896,523	765,866	
Adjusted R-squared	.3273	.3811		.3740	.3410	

The table shows linear probability estimates of mortgage choice. The left-hand-side variable is a dummy=1 if an FRM is chosen and 0 otherwise. In the first test reported on left-hand-side (panel A) sophisticated borrowers are defined as borrowers in provinces in the first 20% of the distribution of the education index. Unsophisticated borrowers are those in provinces in the last 20% of the distribution of the education index. The education index is based on the share of households reporting to have obtained a bachelor's degree. In the second test reported on the right side (panel B) high (low) complaint bank are those with a number of mortgage complaints received by the received by the Banking and Financial Ombudsman (Arbitro Bancario Finanziario, ABF) over total bank mortgage clients at that bank above (below) the median. The sample period is 2004:Q1–2010:Q4. Robust standard errors (clustered at the bank level) are reported in parentheses. Coefficients for borrowers' characteristics and fixed effects are not reported. Table 2 reports the definition of the variables. (1) Include (a) provincial lending concentration measured by the market share of the top-five banking groups; (2) a dummy if the bank participates in the "Patti Chiari" initiative; and (3) dummies to control for the distance between the lending bank headquarters and household residence. *$p < .1$; **$p < .05$; ***$p < .01$.

borrowers. This evidence is consistent with the prediction of the biased advice model.

To test further the distorted advice hypothesis, we have gathered data on complaints against banks filed by borrowers to the Banking and Financial Ombudsman (Arbitro Bancario Finanziario, ABF) in Italy. The ABF is an out-of-court settlement scheme for disputes between customers and banks and other financial intermediaries. Households submit over 90% of the complaints. Among them we have only selected complaints related to mortgages. To allow for a delay between the mortgage origination and the submission of a complaint we focus on complaints submitted over the period 2011–2015. The Ombudsman reports the identifier of the bank the complaint refers to. For each bank, we have then calculated two indexes of complaints: (1) number of mortgage complaints received by the ABF over total bank mortgage clients at that bank (computed from our Credit Register data) and (2) number of complaints accepted by the ABF over total bank mortgage clients. We have then run our basic specification of mortgage choice splitting the sample into high (above median) versus low complaint banks (below median) and test whether the correlation between mortgage choice and supply factors is larger in the sample of banks that received the most complaints, as one would expected if biased advice drives complaints. The right panel of Table 8 shows the estimates when we split based on complaints received. Results are the same if we split on complaints accepted. As can be seen, bank supply shifters have a significantly larger effect, both statistically and economically, among high-complaints banks.

Overall, we take the results in Tables 8 as additional evidence hinting at a role of biased advice in mortgage contract choice.

5. Alternative Explanations and Concerns

A first possible alternative nonprice channel that could explain our findings is rationing rather than advice. Suppose banks target a desired FRM share $\overline{s}$ that depends on supply factors (higher for banks with larger core deposits, easier access to securitization and a smaller bond spread). If the actual share is below target, the bank turns down applicants who opt for ARM and grant mortgages only to those who choose FRM; and conversely if the share is above target. This could explain our findings. Supply factors will affect the probability of observing a given mortgage choice. Rationing, and thus the effects of supply factors, will be more severe at times of price inaction and these effects may be stronger for unsophisticated borrowers if they face higher search costs, so that they are more likely to take the contract offered rather than move to another bank and keep searching. However, rationing implies sorting which should presumably be visible even on observable features, but in our data this does not seem to occur as discussed in Section 4.2.

A second nonprice channel turns on the difference between advice (a signal sent to a group of customers at the banks premises) and advertising (a signal sent to the general public, being clients or not). If some banks invest in advertising a particular financial product, they will tend to sell more of it, even in the absence of advice (Gurun, Matvos, and Seru 2016). If the difference in advertisement levels is correlated with balance sheets, then our results may not be interpreted as advice. While some advertising might be present in our sample, if this is to be the key driver of the result we should observe at least some sorting. By definition, advertising affects a vast pool of potential borrowers *before* they self-select into a given bank. A bank pushing $ARMs$ over $FRMs$ through advertisement would end up attracting a disproportionately large number of borrowers with a preference for the advertised mortgage contract. But as we have seen, the data display little evidence of sorting, at least on our observables.

Obviously, it is well possible that while we find no sorting on observables, borrowers sort on unobservables, implying that the evidence in Table 6 may not be enough to conclusively reject rationing or advertisement as possible explanation. This issue is relevant particularly in light of the fact that, though the borrower characteristics that we observe all help explain mortgage choice, and should thus affect selection if present, the set of borrower's variables in our data set is quite limited. We try to address this issue in two ways.

First, we use data from the Bank of Italy Survey of Household Income and Wealth (SHIW), which contains the identifier of the bank they obtained the mortgage from. We then match these data with the average annual value of the bank supply shifters. Importantly, SHIW also has a measure of individual risk aversion elicited in the same way like in the Survey of Consumer Finances;[23] hence, we can run an ordered logit regression of the borrower risk aversion on the bank supply shifters. Sorting induced by advertising or rationing would imply a correlation with the borrower risk aversion, arguably the key preference parameter that according to theory predicts mortgage choice. We find no effect as the first two columns of Table 9 show. One concern with this risk aversion indicator is that it is poorly measured and the lack of correlation with banks supply shifters reflects poor measurement (inflating standard errors) rather than absence of selection. To assess whether this is the case we run regressions of mortgage type choice on risk aversion and a set of other controls. We find that more risk averse households are significantly more likely to choose a FRM, suggesting that measured risk aversion is not just noise (Table A5 in OA). The SHIW contains a large number of observables characteristics, so we have expanded the test and have checked whether the supply shifters correlate with each of a larger set of individual characteristics[24] that may potentially matter for mortgage choice. In all cases, an F test reveals no correlation (Table A6 in OA). Yet, while this evidence lowers the chances that the nonprice channel that we document reflects advertising or rationing, it cannot rule it out conclusively: selection may still occur on variables that we do not observe.

To further address this issue, we use a subsample of borrowers in our data set that at a different point in time obtain another mortgage either from the same or another bank. This sample (15% of the cases, of which 13.7% taking a second mortgage and 1.7% also a third mortgage) is a panel. This allows us to replicate our estimates inserting borrower fixed effects, which by definition capture any time invariant borrower characteristic relevant for mortgage contract choice, including the borrower risk aversion. Advertising and rationing both induce selection which if present should result in a correlation between the supply factors and the fixed effects. To test this implication we have estimated our main specification on the panel sample first without and then adding individual fixed effects. The first two columns of Table 10 show these estimates. Two facts emerge. First, the results hold also in the panel, and estimates are very close to those in the whole sample. Second adding the fixed effects increases the fit of the

[23] Individuals report their preference for risk-return combinations ranging from very high risk and very high return to no-risk and low return, with two intermediate combinations in between. Hence, the indicator takes four values increasing in risk aversion.

[24] The list includes individual wealth, individual income, the consumption-to-income ratio, education, family size, and overdraft facilities.

Table 9
Test for the presence of advertisement and rationing effects

	Dependent variable is individual risk aversion (1)		Dependent variable is bank rejection rate (2)	
Explanatory variables:	(1) Baseline	(2) Adding bank and time fixed effects	(3) Baseline	(4) With interaction terms
Bank bond spread	0.0243	−0.0413	−0.1594	−0.1759
	(0.0692)	(0.0797)	(0.1472)	(0.1525)
Securitization activity	−0.0088	−0.2765	0.2963	0.2604
	(0.1422)	(0.2549)	(0.3841)	(0.3767)
Deposit ratio %	−0.0025	−0.0141	0.0091	0.0100
	(0.0037)	(0.0099)	(0.0165)	(0.0170)
D_{ib}(3)				0.0520
				(0.5758)
Bank bond spread * D_{ib}				0.0673
				(0.2288)
Securitization activity * D_{ib}				0.0862
				(0.2814)
Deposit ratio % * D_{ib}				−0.0022
				(0.0091)
Bank fixed effects (BFE)	No	Yes	Yes	Yes
Time fixed effects (TFE)	No	Yes	Yes	Yes
F-test on joint significance of bank-specific characteristics (p-value)	.8623	.3502	.5364	.9217
Estimator	Ordered logit	Ordered logit	OLS	OLS
Observations	3,023	3,023	3,023	3,023
Pseudo/Adjusted R-squared (4)	.0010	.0596	.461	.460

The sample period is 2004:Q1–2010:Q4. Robust standard errors (clustered at the bank level) are reported in parentheses. (1) The individual risk aversion indicator takes four values increasing in risk aversion. Individuals report their preference for risk-return combinations ranging from very high risk and very high return to no-risk and low return with two intermediate combinations in between. In particular, we have considered the answers to the following question. "In managing your financial investments, would you say you have a preference for investments that offer: 1) **very high returns**, but with a **high risk** of losing part of the capital; 2) a **good return**, but also a **fair degree of protection** for the invested capital; 3) a **fair return**, with a **good degree of protection** for the invested capital. 4) **low returns**, with **no risk** of losing the invested capital.". We have considered only individuals taking a mortgage in a given years and matched the database with average bank-specific characteristics of their financial intermediary. (2) The left-hand-side variable is given by the number of rejected applications over the total number of loan applications received by a bank in a given quarter. Table 2 reports the definition of the variables. (3) Price inaction: in panel A, dummy $D_{ib}=1$ in quarters where bank b the change in the FRM/ARM spread fall in the rage $\pm\frac{sd_b}{3}$ where the standard deviation is specific to each bank. (4) Pseudo R-squared for the first two columns and adjusted R-squared for the last two columns. $*p<.1$; $**p<.05$; $***p<.01$.

regression and fixed effects are jointly statistically significant (p-value of the F-test: .000), implying that fixed effects capture relevant characteristics; however, this leaves the parameters on the supply factors unchanged - a symptom of lack of correlation between unobserved heterogeneity and supply factors and thus of sorting on *all* unobservable fixed characteristics. We cannot reject the null that the effect of each of the three supply shifters is the same in the first column. Formally, when we regress the estimated fixed effects on the supply factors we find no correlation (Column 3, p-value of the F-test .75).

Concerning rationing, we propose also a more direct test. We have obtained from the Credit Registry maintained by the Bank of Italy, data for each bank

Table 10
Subsample of borrowers with multiple mortgages

	Dependent variable is a dummy=1 if a FRM is chosen and 0 otherwise		Dep. variable: Borrowers' fixed effects
	(1) Baseline model (Table 5, Column 1)	(2) With borrowers' fixed effects	(3) Test for correlation of BOFE on supply factors
LTFP	−0.0519***	−0.0528**	
	(0.0108)	(0.0230)	
Bank bond spread	−0.0618***	−0.0635***	0.0096
	(0.0114)	(0.0144)	(0.0088)
Securitization activity	0.0239*	0.0198*	−0.0033
	(0.0128)	(0.0114)	(0.0140)
Deposit ratio %	0.0011***	0.0021**	−0.0005
	(0.0003)	(0.0010)	(0.0011)
Bank fixed effects	Yes	Yes	Yes
Region-time fixed effects	Yes	Yes	Yes
Borrowers' characteristics	Yes	Yes	No
Borrowers' fixed effects (BOFE)	No	Yes	No
Other controls (1)	Yes	Yes	No
Test on joint significance of BOFE (p-value)	–	.000	–
Test on joint significance of bank characteristics (p-value)	.000	.000	.751
Observations	253,763	253,763	253,763
Adjusted R-squared	.328	.342	.142

The first two columns of the table show linear probability estimates of mortgage choice for a subsample of borrowers that have more than one mortgage. The left-hand-side variable is a dummy=1 if an FRM is chosen and zero otherwise. The third column of the table shows an OLS regression of the borrowers' fixed effects on bank-specific characteristics and other controls. Robust standard errors (clustered at the bank level) are reported in parentheses. The sample period is 2004:Q1–2010:Q4. Coefficients for borrowers' characteristics and fixed effects are not reported. Table 2 reports the definition of the variables. (1) Include (a) provincial lending concentration measured by the market share of the top-five banking groups; (b) a dummy if the bank participates in the "Patti Chiari" initiative; and (c) dummies to control for the distance between the lending bank headquarters and household residence. *$p < .1$; **$p < .05$; ***$p < .01$.

and every quarter on *a*) the total number of loan applications and *b*) the number of applications that have been rejected. We have then computed a rejection rate as *b/a* and regressed it against our bank-specific supply factors and their interactions with the inaction dummy. The results presented in the last two columns of Table 9 indicate that the rejection rate is not correlated with our measures of supply factors (Column 3) and that this lack of correlation is there even during periods of price inaction (Column 4).[25]

A final issue regards the effects of competition. The model assumes that each bank is a monopolist in its local market. Provided banks retain some market power qualitative results on advice do not depend on this assumption. Yet quantitative results may depend on market power. To test whether competition disciplines banks and affects the extent of biased advice, we run our basic

[25] Italy does not have a preapproval process; the loan officer collects the applications and provides advice. The approval is decided later by a different bank department. This timing separates the advice from the rationing decision, making the test meaningful. In fact, if no distorted advice is present and rationing is used instead, one should find a correlation between rationing and banks supply shifters.

specification splitting the sample between provinces with “higher competition” (those provinces where the market share of the first five banking group is below the median) and provinces with “lower competition.”The results show that borrowers in less competitive provinces display a stronger negative response to an increase in the fixed rate bond spread and a stronger positive response to an increase in securitization activity and in the core deposit ratio. However, such differences are not statistically significant (Table A7 in OA).

6. Conclusion

In this paper we use a novel methodology to detect the presence of biased financial advice from banks to households choosing a mortgage. We show that in a simple model of mortgage choice in which the lender can set the price and also give the customer advice, the relative prices of fixed rate and adjustable rate mortgages are generally not a sufficient statistic for the choice. Banks that face a mixed pool of sophisticated and unsophisticated borrowers will respond to changes in the cost and availability of funding by adjusting prices and by providing advice to steer borrowers toward the choices most advantageous to the bank. Hence, supply shocks affect borrowers’ mortgage choices not only through prices but also directly, insofar as they proxy for unobservable advice. Thus, they actually reveal the existence of such advice.

We find evidence that is consistent with this prediction. Time-varying measures of the bank’s incentive to steer households toward adjustable rate mortgages, such as the cost of long-term funding, affect household choice even when controlling for the relative price at origination of the two types of mortgage. As the model predicts, the effect of this distortion is stronger in periods when banks do not adjust the relative price of their mortgages. In addition, and again consistent with the model, nonprice supply side effects on borrowers’ choice are stronger in the case of unsophisticated borrowers, who should theoretically be more responsive to the bank’s advice. Other mechanisms of nonprice responses, such as rationing and advertising, can potentially explain these findings. While we cannot rule them out conclusively, several pieces of evidence make these alternative channels less likely than distorted advice. In particular, contrary to what rationing and advertising imply, we find no evidence of sorting, at least on the set of observables in our data and time-invariant unobservables.

While this paper shows evidence of nonprice distortions on mortgage choice it is silent on their welfare cost, who bears it when borrowers differ in sophistication and what policies are most effective at limiting banks incentives to distort mortgage choice. In a complementary paper, Guiso et al. (2018) build on our evidence to precisely address these questions. They set up and estimate a structural model of the mortgage market in which banks set prices and can steer the choices of heterogenous borrowers. The model estimates the fraction of unsophisticated borrowers at 48% and sets the welfare loss from distorting choices at a sizable 11% of the annual mortgage repayment, borne largely by the unsophisticated borrowers. Interestingly, while the cost of the distortion is relevant, their policy simulation implies that severely restricting advice would carry even larger costs, as the information value of the advice far exceeds the cost of the distortion.

Appendix A. Proofs

In this appendix we prove the propositions that characterize the model solution. In what follows we adopt the convention $m=1$ if the choice is ARM and $m=0$ if the choice is FRM.

Proposition 1: In the absence of advice, households' mortgage choice is independent of bank supply factors conditional on the relative prices of ARM and FRM. In particular, $E(m|\phi)=E(m|\phi,\theta)$, where m denotes mortgage choice.

Proof If there is no advice the equilibrium household decision rule as a function of risk aversion and supply factors is

$$m(\gamma)=\begin{cases}1 & \text{if } \phi(\theta)>\gamma\\ 0 & \text{if } \phi(\theta)\leq\gamma\end{cases}$$

so that $E(m|\phi)=G(\phi)E(m|\gamma>\phi)+(1-G(\phi))E(m|\gamma\leq\phi)=G(\phi)=E(m|\phi,\theta)$.

Proposition 2: If the model does not satisfy the SSP, household choices depend on the factors θ even controlling for prices. In other words, $E(m|\phi,\theta)\neq E(m|\phi)$.

Proof With advice, the household decision rule becomes

$$m(\gamma)=\begin{cases}1 & \text{if } \phi(\theta)-\alpha(\theta)>\gamma\\ 0 & \text{if } \phi(\theta)-\alpha(\theta)\leq\gamma,\end{cases}$$

and $E(m|\phi)=E_\theta\left\{G(\phi-\alpha(\theta))E(m|\gamma>\phi)+(1-G(\phi-\alpha(\theta)))E(m|\gamma\leq\phi)\right\}=E_\theta\{G(\phi-\alpha(\theta))\}$. By a similar calculation, $E(m|\phi,\theta)=G(\phi-\alpha(\theta))$. If the two coincide, it must be that $\alpha(\theta)$ is deterministic given ϕ, otherwise it is not possible for the expectation of $\alpha(\theta)$ to coincide with each of its realizations. Hence, a deterministic function must link ϕ to α, so that the SSP must be satisfied.

Proposition 3: Under price rigidity, $E(m|,\phi,\theta)\neq E(m|\phi)$.Moreover, price rigidities may amplify the effects of supply factors because advice substitutes for pricing in distorting demand.

Proof If the SSP does not hold, the result is proved by the last proposition which holds for general degrees of flexibility. Now suppose SSP holds. Under price rigidity, there exists a subset of the supply factor space Θ such that the bank does not adjust the price. Call this subset Θ^I. Now if a bank starts with price ϕ and gets two draws of supply factors $\theta_1, \theta_2 \in \Theta^I$ with $\theta_1 \neq \theta_2$, it must be that $E(m|\phi,\theta_1) = G(\phi - \alpha(\theta_1)) \neq G(\phi - \alpha(\theta_2)) = E(m|\phi,\theta_2)$. Because $E(m|\phi) = E_\theta(E(m|\phi,\theta))$ and the same expectation cannot be associated with two different realizations, we must have $E(m|\phi) \neq E(m|\phi,\theta)$.

To see the amplification under price rigidity, note that the ARM share, in case of rigidities, is $x = G(\phi_0 - \alpha(\theta))$ so that $\frac{\partial x}{\partial \theta_i} = -g(\phi_0 - \alpha(\theta))\frac{\partial \alpha}{\partial \theta_i}$ and the marginal effect depends on the shape of the distribution and the bank's payoff function. If there is some complementarity for the bank between prices and advice and if the distribution of risk aversion does not increase too rapidly in $\alpha - \phi$ the marginal effect is greater under price rigidity. For example, $v_{\alpha\phi} > 0$ and $g(\cdot)$ uniform are sufficient conditions for this result to be true.[26]

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[26] The result cannot be established generally, because the presence of rigidities changes the optimal choice of the bank, moving the position of the marginal borrower (i.e., indifference between ARM and FRM) over the support of the distribution of risk aversion. This implies that the marginal effect of supply factors on advice depends on the distribution of risk aversion. Generally, under $v_{\alpha\phi} > 0$, we need fixed costs that are high enough in order to argue that the marginal effect is not greater under price rigidity: if it is not, this means that the distortion under price rigidity differs very substantially from that under price flexibility. When this is true, the marginal profitability of a change in prices must be higher, and high fixed costs are necessary for this to happen.

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