The Effectiveness of Housing Collateral Tightening Policy[*]

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Abstract

We show that macroprudential policies can lead to adverse selection in the market for residential mortgage loans. We exploit a unique loan-level data set and a policy experiment in Singapore that differentially targets mortgage contracts for second homes. For a horizon of up to one year after the policy roll-out, we document a significant composition change towards a riskier type of borrowers that are twice more likely to become delinquent relative to a comparable non-treated cohort. Ex ante, these borrowers are not different in terms of observable characteristics, but they have lower behavioral credit scores and worse histories of credit card repayment. Consistent with the hypothesis of adverse selection towards a pool of more optimistic investors that fail to have a correct assessment of the policy impact and housing market growth, most of the effects appear to be driven by individual experiences of past price appreciation. Using a separate data set on bankruptcy proceedings, we also document a higher probability of default in the market for investment properties, occurring immediately after the time of the policy change.

1   Introduction

Macroprodential policy has become a fixture of recent policy efforts that aim to contain household leverage and reduce financial vulnerabilities. The usual assumption about the effects of measures such as upper limits on loan-to-value ratios is that they discourage speculation and reduce the likelihood of financial distress. High leverage is rightfully seen as a driver of possibly excessive exposure of the household sector to both financing liquidity and housing market risk. However, most of the research in this field has concentrated on the medium- to long-run behavior of the representative household, ignoring the role of policy measures on short-term liquidity and the changes in the composition of borrowers ensued by changes in the regulatory regime.

In this paper, we document evidence consistent with unintended consequences of a tightening policy in the mortgage market. We exploit a unique policy measure implemented in Singapore, that differentially targeted second homes. On August 31, 2010, the Monetary Authority reduced the upper limit on LTV ratios for borrowers that had at least one loan outstanding from 90 to 80 percent and raised the cash down-payment requirement from 5 to 10 percent of the collateral value[1]. We analyze the differential selection and treatment effects across synthetic cohorts of loans issued within adjacent time windows before and after the policy roll-out[2] and trace credit outcomes up to 18 months thereafter.

Our main dataset consists of a large representative sample of mortgage loan account histories, obtained from one of the largest banks in Singapore for the period between April 2010 and March 2012. We merge this sample with data on population records - which allows us to capture

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[1]Far from being a negligible part of the market, mortgages collateralized with investment property account for around a quarter of total outstanding volumes. But even beyond the immediate implications for this credit class, our results are generalizable to the financial system as a whole and highlight a phenomenon that can reasonably be expected to remain valid across jurisdictions.

[2]Although the general commitment of the Monetary Authority to more restrictive credit conditions was signaled well in advance, to the best of our knowledge, the precise form of the policy and the implementation schedules were unexpected.

a large set of demographic and financial characteristics of individuals, data on housing transactions - to understand local house price developments, as well as a comparable comprehensive sample of credit card accounts from the same bank.

Second loans have an *ex ante* better risk profile. We find that relative to first-time homebuyers, investors are older, better educated and earn higher incomes, while also having longer tenures with the bank. At the same time, loans for investment purposes are less likely to be subject to interest penalties. Moreover, following the more restrictive lending environment instituted in August 2010, LTV ratios on second loans decrease significantly by around 5 percentage points. This is especially driven by an overproportional reduction of average loan amounts, compensating for the simultaneous decrease in average home values.

Against this background, it seems puzzling to observe that overall lending in the second-loan market has largely remained unaffected and transaction activity even intensified weakly over the coming year. Instead, we report a substantial composition change towards a more risky pool of borrowers. The mortgage cohort originated immediately after the implementation of the policy is much more likely to become delinquent over the course of the next year than the comparable tranch of loans for owner-occupied property.

Importantly, this type of adverse selection is not manifest along observable characteristics, probably as a result of the bank's risk management procedures and screening efforts. However, the effect is immediately apparent in the customers' checking account and credit card data histories. The new cohort of borrowers has substantially less liquid assets than the control group, a higher *ex ante* likelihood to pay interest penalties on credit cards, and substantially lower credit scores. We favor the view that the worse credit outcomes are driven by selection effects especially because we do not see evidence for a change in financial savings or consumption behavior for these borrowers. They probably fail to have a correct assessment of either the housing market growth or the policy impact and will outstretch themselves in making the home investments.

We test this latter hypothesis by conditioning the estimated selection effect on local house price growth rates. The results confirm that most of the effect is driven by the top 25 percent

areas of the country which have experienced high rates of house price appreciation after the 2008 financial crisis. More optimistic investors likely overestimate their repayment ability, especially in an environment with tighter collateral requirements and potentially binding liquidity constraints.

This interpretation is also consistent with additional evidence of a slow learning effect. We find that the selection of riskier borrowers is concentrated in the first six months after the policy experiment, likely because both investors and financial service providers gradually realize the real and long-term implication of the government policy and update their prior on the regime shift of the policy stance and the resulting housing market price growth.

Finally, we provide external validation for our approach by using comprehensive data on bankruptcy cases and property transactions from a proprietary source. We confirm the presence of adverse selection in the market for investment properties around the time of instatement of the more restrictive lending rules and no indication of similar effects either before or more than a year thereafter.

This paper is part of recent efforts to understand the role of collateral constraints on mortgage lending and credit card debt (Qi and Yang, 2009; Mian and Sufi, 2011; Fuster and Zafar, 2015; Agarwal et al., 2015), the impact of the regulatory credit regime on household behavior (Corbae and Quintin, 2015; Agarwal and Qian, 2016), and the effectiveness of macroprudential policy (Akinci and Olmstead-Rumsey, 2015; Cerrutti, Claessens and Laeven, 2015; McDonald, 2015; Tressel and Zhang, 2016).

Our results are consistent with the implications of the Boz and Mendoza (2014) model of financial innovation and learning in an environment with binding borrowing constraint. In this setup, overborrowing can result from optimistic beliefs, even in an environment with tighter collateral restrictions. This point seems to have been recognized and anticipated by policy makers in Hong Kong. Wong, Ho and Tsang (2015) show that while the more restrictive LTV policy has been associated with binding liquidity constraints on homebuyers, the additional public mortgage insurance program was introduced to mitigate this drawback and support the effectiveness of the policy.

The rest of the paper is organized as follows. Section 2 introduces the combination of proprietary and public data sets that we use in the analysis. Section 3 explains the identification approach and reviews the different hypotheses and estimated specifications. Section 4 discusses our main results, additional tests and the external validation exercise. In Section 5, we conclude and discuss potential policy implications.

2 Data

We use a proprietary data set obtained from one of the largest banks in Singapore that serves around 80 percent of the entire population. The bank has a market share of 25% of loans outstanding and is the leading issuer of mortgage credit[3].

We have access to a random, representative sample of account histories for 17,197 mortgage loans, for a period between April 2010 and March 2012. For each loan, we know the creation date, the loan-to-value ratio at origination, the interest rate, as well as any interest penalty charges that the account may have been subject to.

We classify second loans by comparing the start date of each contractual agreement with the customer's lending history. If we find at least one loan outstanding, subsequent ones are indexed accordingly[4]. Most Singapore households have long-lasting relationships with their financial service providers (in our dataset, the average tenure with the bank is around 15 years; see Table 3), which alleviates any concern of systematically mis-classifying borrowers as first-time buyers or investors. Cross-checking the composition of our sample against aggregate data is difficult because of the unobserved distribution of loan types across banks. Nevertheless, in Figure 1, we show that around 20 percent of our borrowers have a second loan, which lies at

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[3]This bank has more than 4 million customers, twice the number of branches, and over four times the number of ATMs than the other major banks in Singapore. Although we do not have information on whether consumers have other banking relationships, the bank is likely the dominant service provider for our sample consumers' financial needs due to its greater convenience and comparable banking fees.

[4]A small fraction of customers have three or more mortgages. We choose to restrict our focus to just second loans, in order to not contaminate the sample with potential outliers.

the lower bound of the comparable range of 20-30 percent reported by the Monetary Authority of Singapore for the entire financial sector.

For the same time period, we obtained the histories of monthly credit card transactions and checking account balances for each individual in our sample, including details on the type of spending. Most importantly, we know whether the specific credit card account has been delinquent or is contemporaneously in delinquency (i.e. in arrears with respect to the repayment schedule). We see this indicator as a counterpart to the indicator of mortgage delinquency. Finally, we also have access to the behavioral score that the bank regularly computes for each customer in order to assess their solvency and repayment capability. We interpret this score as an overall long-term indicator of consumer risk, parallel to the more short-term variables described above[5].

The second proprietary dataset contains demographic information about Singapore residents. Based on the unique identification numbers, we match borrowers in the mortgage dataset to the population dataset to obtain information about a rich set of characteristics about each individual, including age, gender, income, nationality, ethnicity, occupation, as well as their address of residence.

We use the address of residence in the second step of our analysis, where we aim to externally validate our benchmark results against data on bankruptcy procedures.

In this case, we propose a different approach to classify individuals as investors, building on a proprietary dataset of private (non-landed, condominium) housing transactions from a local property agency. For each record, we know the exact address of the property, as well as the unique personal identification number of the seller. The latter allows us to use the population dataset to also recover their home address. By comparing the two - i.e. the address

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[5]The recording of behavioral scores only starts after our treatment period and we are therefore not able to distinguish precisely between selection and treatment effects. However, based on the observed variation for the time period we have, behavioral scores seem to be extremely persistent and only infrequently adjusted, so we prefer to analyze treatment effects by looking at an alternative set of variables such as spending patterns and checking account balances.

of the property being transacted and the residence address of the seller - we are finally able to identify investors as individuals for which they differ[6].

The last dataset comprises a representative set of records of law events in Singapore courts. They contain information on registration time, the nature of the claim, as well as the unique personal identification numbers of the defendant in each law event. By matching the law event records to the investor indicator obtained from the property transaction dataset, we are able to follow the time series of bankruptcy events and understand cohort-specific effects. Overall, this final sample comprises 94 relevant personal bankruptcy cases, covering the period between November 2009 and November 2011.

3   Methodology

Our identification approach exploits differential selection and treatment effects across synthetic cohorts of mortgage loans.

As a plausibly exogenous shift in mortgage market conditions, we use an unexpected collateral tightening policy implemented in August 2010 in Singapore. On August 31, 2010, the Monetary Authority decreased the maximum Loan-to-Value (LTV) ratio from 90 to 80 percent of the estimated property value, increased the minimum cash down-payment from 5 to 10 percent and signaled a broad contractionary future stance on mortgage market activity as forward guidance[7]. The measure was binding for all monetary and financial institutions operating in Singapore, for all types of borrowers (foreigners, nationals and permanent residents), as well as all types of properties serving as collateral (public housing and private developments).

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[6]The classification of an individual as an investor is unambiguous. However, we cannot have complete certainty that sellers which sell their owner-occupied residential unit do not also have some other unobserved piece of real estate. Nevertheless, this potential omission does not materially affect our results and would only bias the estimated coefficients towards zero.

[7]Subsequently, the Monetary Authority continued to maintain this collateral tightening stance, but no further measures were implemented until January 2011. In our empirical investigation, we carefully restrict the event and evaluation periods to only cover the relevant sub-periods and to exclude cross-contamination effects from other possible treatment events.

Importantly though, the tightening policy only applied to borrowers which had at least one mortgage loan outstanding and was specifically targeted to dampen lending activity related to property investment[8]. We exploit this difference to distinguish between a treatment group (second mortgages) and a control group (first mortgages, i.e. ones meant to finance owner-occupation).

The aggregate market share of second loans has varied between 20% and 30% during recent years. So while the Monetary Authority has targeted a sizable part of the market and the resulting effects of the policy intervention have substantial bearing on a large number of individuals, we are comfortable with considering the market for loans to first-time borrowers as a control group, plausibly unaffected by a similar tightening of collateral[9].

To capture the variation in contract and borrower characteristics around the event time, we construct synthetic origination cohorts for the 5 months before and after the implementation of the policy. The differential consideration of first- and second-loans allows us to isolate the net effect of the policy both on the composition of borrowers (a *selection* effect), and on the subsequent financial decisions that these take (a *treatment* effect).

We assess the statistical significance of the net selection effects using the following *unconditional* specification:

$$y_{i,n} = \beta_1 1_{n=1} 1_{pre} + \beta_2 1_{n=1} 1_{post} + \beta_3 1_{n=2} 1_{pre} + \beta_4 1_{n=2} 1_{post} + \varepsilon_{i,n}, \tag{1}$$

where we replace $y_{i,n}$ with any characteristic of interest, referring to borrower i and loan type $n = \{1, 2\}$. The dummy variable 1_{pre} and 1_{post} indicate whether the loan is part of the pre- or post-policy cohort.

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[8]Although there are rare exceptions, properties financed by first-time borrowers are generally owner-occupied and the ones financed by subsequent loans are held for investment purposes. In this paper, we therefore refer to second-loan borrowers as *investors*.

[9]To the best of our knowledge, we note that no other relevant initiatives were rolled out during the event window. Nevertheless, even if first-time loans were affected by other factors, e.g. developments in global markets, these matter only to the degree to which they differentially affect the two types of loans.

To understand the net *conditional* effects of the policy on loan performance (and specifically on the post-origination delinquency rate), we propose the following loan-level linear-probability model:

$$1_{p_{i,t,n}>0} = \tau_t + \xi X_{i,t,n} + \beta_1 1_{n=1} 1_{pre} + \beta_2 1_{n=1} 1_{post} + \beta_3 1_{n=2} 1_{pre} + \beta_4 1_{n=2} 1_{post} + \varepsilon_{i,t,n}, \quad (2)$$

where $p_{i,t,n}$ are loan penalties paid by borrower i on loan n in period t. In this estimation, we also control for time fixed effects τ_t and loan-specific variables $X_{i,t,n}$ such as the loan amount, and the income, age, race, sex, and marital status of the borrower.

Both equations (1) and (2) insure that even if there may be any parallel transmission of policy changes or other unobserved factors to loan issuance or mortgage repayment during the event window, the combination of time fixed effects and origination cohort fixed effects isolate those influences and allow for a clean identification of the effects of the policy.

In the latter part of our analysis, we exploit the rich credit checking account and credit card histories of borrowers from different mortgage issuance cohorts. This allows us to model selection and treatment effects jointly. We propose the following specification, estimated in a panel dataset of account × month observations:

$$y_{i,t,n} = \delta_t + \alpha 1_{post} + \underbrace{\beta 1_{n=2}}_{\text{Characteristics}} + \underbrace{\gamma 1_{post} 1_{n=2}}_{\text{Selection effect}} + \underbrace{\tau 1_{post} 1_{n=2} 1_{obs}}_{\text{Treatment effect}} + \varepsilon_{i,t,n}, \quad (3)$$

where we replace $y_{i,t}$ with variables capturing the financial behavior of borrower i in month t. In addition to the terms that are already part of equations (1) and (2), we introduce the dummy variable $1_{\text{post-policy cohort, second loan, post-policy observation}}$. It takes the value of 1 if the respective account × month observation stems from a second-loan borrower that has originated at least one loan during the post-policy treatment period and if - at the same time - the specific month of the observation falls within the subsequent post-policy evaluation period, i.e. between January and June 2011.

The comprehensive formulation in equation (3) also includes time fixed effects, which elimi-

nate any common variation across all checking and credit card accounts, either due to aggregate factors or any idiosyncratic time variation in bank policy.

In order to dig deeper into the mechanisms likely to generate adverse selection among property investors, we condition the coefficient β_4 on factors likely to be correlated with the strength of their investment motive. Plausibly, one such factor is the experienced post-crisis house price growth, that investors might interpret as a signal for more value appreciation potential down the road. Let $1_{d,price}$ be an indicator variables taking the value of 1 for the districts with the 25 percent highest level of average house price growth between 2008 and 2010[10]. We propose the following variation to equation (2):

$$1_{p_{i,t,n}>0} = \tau_t + \beta X_{i,t,n} + \beta_1 1_{n=1} 1_{pre} + \beta_2 1_{n=1} 1_{post} + \beta_3 1_{n=2} 1_{pre} + \beta_4 1_{n=2} 1_{post} + \delta 1_{d,price}$$

$$+ 1_{d,price}(\gamma_1 1_{n=1} 1_{pre} + \gamma_2 1_{n=1} 1_{post} + \gamma_3 1_{n=2} 1_{pre} + \gamma_4 1_{n=2} 1_{post}) + \varepsilon_{i,t,n},$$

where d is the postal district where the borrower resides. Ideally, we would want to consider local house price variation within the area where the property serving as collateral is located, but we do not have access to the address of the property. Nevertheless, the interpretation of the result is only slightly altered when we use this alternative proxy: observed house price appreciation around the borrower's location is probably used as a signal extraction tool when forming expectations; on the other side, optimism about the local market does also translate into an increased investment motive and the willingness to take on additional leverage.

Beyond immediate effect on the post-policy cohort during a 5-month treatment window - where identification is unambiguous - we want to understand whether the mechanisms that we uncover are affecting the mortgage market in a more persistent way. The main problem hereby is that statistical inference becomes less powerful once we go beyond January 2011 because of confounding influences from subsequent rounds of collateral tightening policy, as

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[10]Postal districts are the most granular geographical segmentation for which we are able to calculate meaningful house price indexes. Some postal districts are only sparsely populated and are predominantly dedicated to industrial or commercial real estate. In our analysis, we consider a set of 27 districts, widely dispersed across the country.

well as alternative behavioral and anticipation effects that may affect first-time buyers and investors asymmetrically. Moreover, these two different factors have opposing implications on any results obtained from this extended analysis: (i) the January 2011 round of cooling measures can potentially bias the respective coefficients upwards due to further restrictions on the available menu of contractable LTV ratios, (ii) anticipation effects and more intense screening can at the same time affect the market for first-time loans, biasing the coefficients towards zero. Nevertheless, we expect the latter factor to only play a modest role, especially if the estimated effects remain statistically significant for a prolonged period of time. To capture such effects, we propose the following specification:

$$
1_{p_{i,t,n}>0} = \tau_t + \beta X_{i,t,n} + \delta_1 1_{n=2} + \sum_{j=1}^{4} \left(\gamma_j 1_{cohort=j} + \delta_j 1_{cohort=j} 1_{n=2} \right) + \varepsilon_{i,t}, \tag{4}
$$

where we add two more synthetic cohorts to the analysis, one covering the period February 2011-June 2011 and the other one July 2011 - November 2011[11].

Finally, we provide external validation for our results by using aggregate bankruptcy records that cover the entire financial sector in Singapore. We attach a dummy variable to each individual case, indicating whether the defendant is a property investor. In each period, we then calculate the likelihood that the bankruptcy procedure was initiated against an investor as opposed to a first-time buyer. Concretely, we base our statistical inference on the following specification:

$$
1_{investor,i,t} = \sum_{j=1}^{5} \gamma_j 1_{cohort=j} + \varepsilon_{i,t}, \tag{5}
$$

where i indexes the court case, t is the specific month in which the bankruptcy order was issued. Compared to equation (4), we also include an additional synthetic cohort for the period November 2009 - April 2010 as the omitted reference group.

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[11] One additional caveat hereby is that we have to expand the evaluation period to cover the entire available sample for each cohort and this leads to different numbers of cohort × month observations. We have experimented with different ways to deal with this problem and conclude that it does not have any material impact on the results.

4 Results

4.1 Aggregate dynamics and policy context

Collateral tightening policy has become a standard macroprudential tool, aimed at cooling the housing market and soothing vulnerabilities both in the household sector and among financial intermediaries. The assumption about the underlying transmission mechanism is that by discouraging high-LTV mortgage lending, risky and over-optimistic borrowers drop from the market and aggregate housing demand decreases. Second loans are the natural first policy target in this case, allowing the monetary authority to explicitly address the root of leveraged speculative behavior, without directly affecting housing affordability for the bulk of the population.

The effectiveness of such measures is difficult to assess. Figure 1 shows that house price inflation was more muted in the wake of the policy roll-out in August 2010, but prices continued to rise by 10%-15% per year until stabilizing in late 2013. The number of transactions followed similar patterns.

The collateral tightening policy came into place at a time of intense mortgage market activity, with growth rates above 20 percent and a continued upwards trend for almost two years. While the growth in mortgage lending immediately stabilized around the timing of the policy, the market continued to expand robustly. Credit growth finally abated two years later and house prices decreased across all sub-regions, supported by a series of subsequent policy measures targeting various segments of the economy.

Interestingly, activity in the market for second loans has also remained unaffected by the policy measure and, if anything, followed a slight upward trend. Before turning to our analysis of this puzzle, we review a set of stylized facts and ask how first and second mortgages differ in terms of loan and borrower characteristics.

4.2 First vs. second loans

Mortgage loans are predominantly meant to finance residential owner-occupation, while second loans are mostly taken out for the purpose of financing property investment. In Table 3, we compare the features of the average mortgage contracts for these two categories of loans.

Second-loan properties are generally more expensive by a factor of around 50 percent and they are much more likely to be private developments[12]. Investment property is generally sought to have both high income-generating capacity and robust store of value. In a market dominated by public housing, investors reasonably prefer expensive and more prestigious objects such as condominium apartments, which they finance with loans that have a slightly shorter duration on average and lower LTV ratios. Interest rate spreads appear broadly similar across the two loan types, probably indicating that banks trade off the higher general risk of leveraged real estate investment with the improvements in the quality of the collateral and the better expected solvency of the borrowers.

Especially along this latter dimension, we find remarkable differences. Relative to first-time home-buyers, investors in the property market are significantly older, predominantly better educated males and they earn higher incomes. Since we only capture the income of the household head and investors are also much more likely to be married, the overall disposable resources are understated when simply looking at income differences. The wealth gap between the two groups is thus probably even larger. The investors' relationship with the bank has also been ongoing for a significantly longer time and they are also more likely to be domestic nationals, which further substantiates the likelihood of an a priori better risk profile[13].

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[12]In Singapore, 80 percent of the housing stock is accounted for by public construction projects leased by the *Housing Development Board* (HDB), a government-owned agency. Because of specific public-scheme arrangements which greatly facilitate the access to public housing, traditional bank mortgages are more likely to finance property purchases outside the HDB system.

[13]We do not aim for a definitive view on the relative creditworthiness of owner-occupants versus property investors. Below, we provide suggestive evidence that investors are indeed less likely to become delinquent or to face payment arrears on credit card debt, but our data does not allow for (and our identification approach does not rest on) a precise household-level solvency analysis. Instead, we exploit differences *within* the group of second-loan borrowers, controlling for variation in their general characteristics.

4.3 Adverse selection

Figure 2 summarizes our benchmark result. In Panel A, we report the likelihood of mortgage loans originated in different cohorts to be subject to interest penalties due to late payment of monthly mortgage installments. The observation window is given by the 6 months between January 2011 and June 2011. Consistent with the fact that second-loan borrowers generally face more robust financial conditions, we find that their pre policy likelihood of delinquency is negligible. Instead, first-loan home-buyers seem more exposed to negative shocks, with an estimated rate of delinquency around 2.3 percent.

After the announcement and roll-out of the collateral tightening policy in August 2010, the rate at which first-loan home-buyers face mortgage payment penalties remains unchanged[14]. However, the investor loan cohort originated immediately after the policy change is much more likely to be subject to interest penalties, indicating the presence of adverse selection in the mortgage market.

The likelihood of delinquency for this latter cohort even becomes double in magnitude, relative to the sample of first-time loans originated during the same period, suggesting that around 5 percent of the investors that entered the market immediately after the change in policy have experienced payment difficulties during the first year of mortgage repayment. Second loans are a priori safer and aggregate conditions do not seem to changed in any material way over the observation period - if anything, comparable loans for owner-occupied housing are *less* likely to be delinquent. We therefore attribute the observed effect to a change in the composition of borrowers towards more risky and more optimistic ones, that are willing to bet on the housing market even in the face of adverse conditions and even against an explicit mandate of the monetary authority to implement measures aimed to temperate the previously very high rates of house price appreciation.

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[14]This is reinforcing for our identification approach, but only material in determining its empirical power. Even if the policy would have influenced the composition of first-time borrowers, e.g. through announcement or attention effects, our method isolates this component and we only interpret the net relative effect on second-loan cohorts.

Observable characteristics

In Table 3, we show that the precise type of adverse selection cannot be pinned down solely by looking at observable characteristics. In the last three columns of this table, we report the features of the average mortgage contracts originated in the post-policy cohort and the net selection effect caused by the tightening of collateral requirements.

We find that the preference for private private property has decreased substantially for first-time borrowers and it has increased slightly for investors. Since this period was one in which households were finding it increasingly difficult to afford private condominium apartments (the most prestigious and continuously appreciating properties on the market), it is natural for investors to step in relatively more forcefully - especially those that are more driven by speculative motives, i.e. more optimistic about the future potential for price gains.

In order to gain access to credit, investors were willing to pay significantly higher interest rate premia, despite the fact that, as a direct consequence of the policy measure, the new loans had substantially lower LTV ratios. In turn, the fact that the bank was charging higher interest rates is suggesting that some of the composition change was indeed apparent during the loan pricing and risk management process.

However, the estimated magnitude of 13 basis points seems insufficient, in view of the sizable change in the composition of borrowers documented in Figure 2. All observable differences between investors in the two cohorts are economically and statistically insignificant. Based on purely observable characteristics, the bank's risk management would not have been able to detect the changes in the risk profiles of borrowers[15].

Unobservable characteristics

Instead, in Panel B of Figure 2 we document substantial selection effects for the post-policy investor cohort when analyzing their histories of credit card repayment behavior. Consistent

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[15]The estimated directions of the coefficients suggest that investors in the treatment cohort have slightly lower income, are less well educated and less likely to be active in professional occupations.

with our benchmark results obtained from mortgage performance data, the credit card delinquency rate of investors from the treatment cohort is found to be larger than the delinquency rate of comparable first-time borrowers.

Similarly, we document a significant negative selection effect in terms of the behavioral scores of the investor cohort. In the wake of the collateral tightening policy, the composition of the customer base shifts towards borrowers with higher risk and likely lower loan repayment ability. Since the mortgage and credit card accounts are usually not jointly analyzed in the price setting or risk management decisions (at least not in a systematic customer-by-customer way), it seems natural to conclude that banks are partially oblivious of these composition effects.

Table 4 confirms that investors in the treatment cohort also have lower checking account balances and lower total spending, which is consistent with the fact that the pool of borrowers shifts from the generally less risky profile of typical property investors to considerably riskier borrowers which are more illiquid and seemingly substantially less likely to cope with negative shocks.

4.4   Selection vs. treatment effects

A slightly different (and weakly complementary) alternative to the pure adverse selection mechanism described above is that the collateral tightening policy itself leads people to become more illiquid and face financing constraints in the future. If borrowers overstretch and struggle to meet the higher downpayment, this may lead them to fall behind with subsequent payments and consume less.

In the bottom part of Table 4, we test this hypothesis and confidently conclude that we see no supportive evidence for this type of treatment effects on either checking account balances of credit card behavior. The estimated coefficients are statistically insignificant and, while we indeed observe a slight decrease in investor's checking account balances, their consumption of some goods categories seems to be increasing.

4.5 The role of price expectations

To better understand the change in the composition of borrowers that ensues as a result of the decrease in contractable LTV ratios, we exploit the regional house price heterogeneity across Singapore districts.

Table 6 first documents the robustness of our main identification approach to a wide set of loan-level controls, such as the total value of the mortgage contract, and the income and demographic profile of the borrower. In a second step, we interact the coefficient on the post-policy cohort dummy with the district-level house price growth rate between 2009 and 2011[16]. The reasoning behind this modeling choice is that we expect the most optimistic investors to try to benefit from the momentum and slow reversal properties of residential property prices, possibly extrapolating past trends and trusting their repayment ability even in the face of tighter collateral requirements.

While this can only be suggestive of the underlying factors that have attracted relatively more risky investors to the market, the results robustly confirm that most of the effect that we observe in our benchmark specification is concentrated in areas that have experienced relatively higher house price growth rates in the past. This evidence is entirely consistent with the hypothesis that, during the period immediately following the monetary authority's change in policy stance, more optimistic and possibly overconfident investors adversely selected into the newly available relatively more restrictive mortgage contracts.

4.6 Persistence of selection effects

Our event window of 5 months after the policy roll-out is purposefully trimmed to avoid any confounding effects from simultaneous aggregate developments or subsequent rounds of policy adjustments.

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[16]During this period, residential market developments were highly heterogeneous in Singapore, with growth concentrated in more desirable areas close to the central business district, as well as around new residential developments and in the landed sector (e.g. detached, terraced houses).

Nevertheless, in order to understand the likely persistence of the selection mechanism documented above, we also estimate a more comprehensive specification, in which we trace the performance of loan cohorts originated in the more distant future relative to the timing of the original policy.

Figure 5 reports the estimated coefficients for each loan cohort, alongside 95 percent confidence intervals. When we look at delinquency rates on both on mortgage loans and credit card debt, the results imply the selection of riskier borrowers is concentrated immediately after the first policy experiment in August 2010. This evidence is consistent with a slow learning effect: many investors (especially the more optimistic ones) did probably not realize the real and long-term implication of the government policy; however, after the second LTV increase is put in effect in January 2010, most people update their prior on a likely regime shift of the policy scheme, as well as the resulting housing market price growth.

As further validation of our identification approach, we find that marginal selection effects are indistinguishable from zero both before the policy roll-out, as well as 1-2 years later, after sufficient time has passed for borrowers and lenders to absorb and interpret the new policy environment.

4.7   Insights from bankruptcy proceedings

Finally, we exploit time variation in personal bankruptcy proceedings to provide external validity for our results. In Table 7, we estimate a linear probability model where the dependent variable indicates whether the respective defendant is also a property investor. The right-hand side variables are modeled as time dummies which take the value of 1 if the observation period lies within specific cohort groups, designed to match the same structure as in our benchmark loan-level analysis.

The results indicate a significantly higher likelihood of property investors to declare bankruptcy in the period approximately 6-10 months after the policy change. This is consistent with the

timing that we have identified as part of our benchmark analysis[17].

Since we do not observe significant differences in investor bankruptcy either before the policy change or 1 year thereafter, this is further reinforcing our hypothesis of significant adverse selection in the market for residential investment properties, immediately after the implementation of the collateral tightening policy.

5 Conclusions

This paper shows that macroprudential policy aimed at tightening housing collateral requirements can lead to adverse selection from a riskier pool of borrowers that are more optimistic and choose to take the risk of a liquidity crunch to bet on the housing market. Relative to a comparable non-treated cohort, they have a higher ex ante likelihood of credit card delinquency and lower liquid savings. This phenomenon can alter the transmission mechanism that policy makers usually assume and deteriorate the effectiveness of ad-hoc regulatory measures meant to dampen leverage growth or deter speculation in the housing market. An example of a possibler counter-balancing measure is the introduction of the mortgage insurance program in Hong Kong, which was found to mitigate liquidity constraints for indebted households.

The overall market impact of this type of adverse selection remain unquantified in this paper, since the data only cover a limited part of the Singapore mortgage credit landscape. Nevertheless, anecdotal evidence shows that after the initial round of cooling measures, the Monetary Authority further tightened the housing collateral regime and complemented this with a set of aggressive fiscal disincentives aimed at curbing speculation.

While our results suggest that adverse selection may have delayed the transmission mechanism, possibly driven by the slow adjustment of household expectations, the relative contribution of alternative causes remains an open question.

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[17]We also find a weak build-up of vulnerabilities in the sector immediately after the policy change, but this does not appear to be statistically significant, probably also as a consequence of the overall lower power of the estimation within this relatively lower sample.

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Figure 1
Aggregate market developments

This figure describes the evolution of the residential real estate and credit markets, during and after the event window corresponding to the August 2010 tightening of collateral requirements. In the top panel, we report the SPI hedonic house price index and the overall number of residential property transactions, based on data from the Urban Redevelopment Authority. Both variables are normalized to have a value of 1 in the third quarter of 2010. In the bottom left panel, we plot the average yearly growth rate of the total value of outstanding residential mortgage loans issues, as reported by the Monetary Authority. In the bottom right panel, we use our proprietary mortgage loan dataset and plot the fraction of second loans in overall loan issuance, aggregated at monthly frequency. The darker shaded time interval indicates the post-policy treatment cohort. The intervals marked in lighter shade indicate the treatment evaluation periods and pre-policy control periods, respectively.
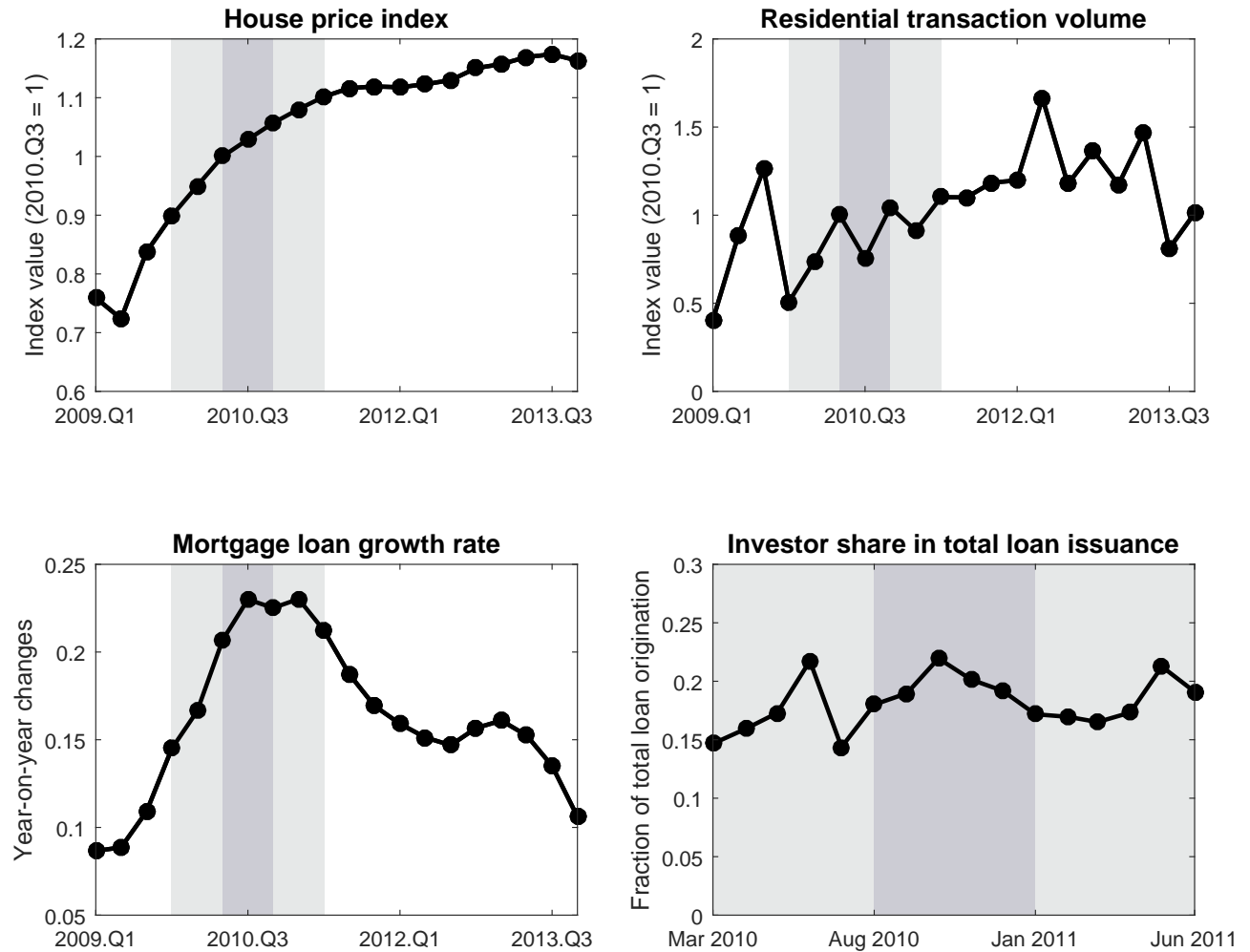
Figure 2
Policy effects across mortgage loan cohorts

In Panel A, this figure reports average incidences of mortgage penalties for synthetic loan cohorts pre- and post-policy change. The indicator variable takes the value of one if the respective loan has been subject to interest rate penalties at least once during the observation window between February and June 2011. In the left part of Panel B, we report average incidences of credit card penalties between April 2010 and January 2011. The corresponding indicator variable takes the value of one if the credit card account has been subject to interest rate penalties at least once during this pre-policy observation window. We assign borrowers to the same synthetic mortgage origination cohorts as above and calculate average likelihoods of credit card interest penalties within each bin defined by the combination of loan cohort and loan type. In the right part of Panel B, we report the corresponding average behavioural credit scores, computed and recorded by the bank for each individual credit card account. Since credit scores are unit-free, we normalize them by subtracting the full-sample cross-sectional mean and dividing by the standard deviation. Vertical bars indicate 90 percent confidence intervals based on within-cohort standard errors.

Panel A

Panel B

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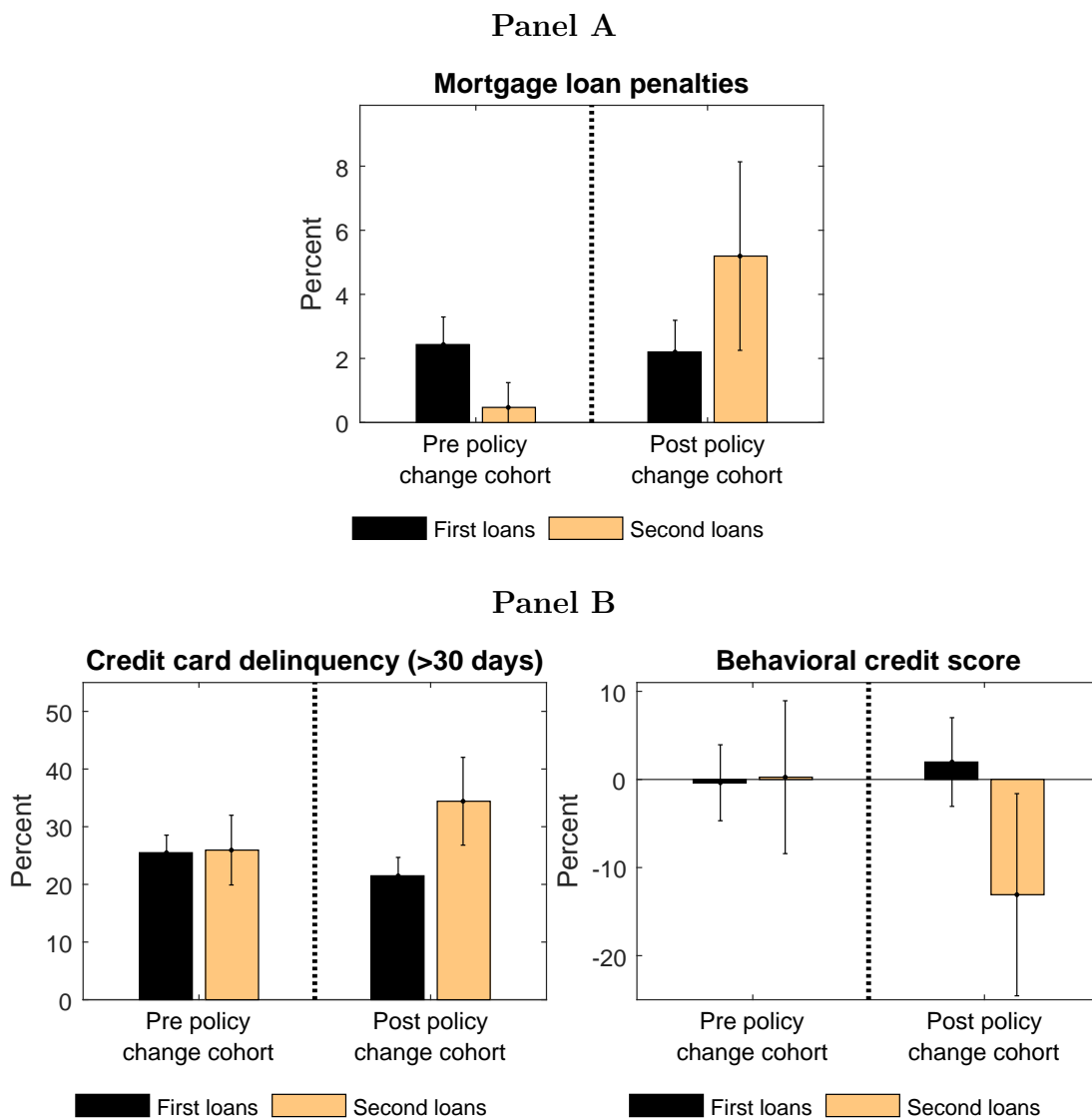
Table 1
Summary statistics

This table reports summary statistics on selected loan-level variables and demographic characteristics, from our merged sample of mortgage loans and individual population records.

	Mean	Percentile				
		5th	25th	50th	75th	95th
Loan amount *('000s)*	$626.81	$116.90	$268.00	$454.00	$750.00	$1,616.00
Property value *('000s)*	$1,075.45	$285.00	$422.00	$780.00	$1,244.00	$2,800.00
Mortgage interest rate spread *(percent)*	1.64	1.03	1.24	1.56	1.99	2.49
LTV ratio *(percent)*	69	33	60	80	80	90
Borrower income per year *('000s)*	$138.29	$29.06	$52.86	$84.00	$148.90	$403.25
Borrower age *(years)*	41	30	35	40	46	57
Length of tenure with the bank *(years)*	15	4	12	13	21	23
Credit utilization rate	0.12	0.00	0.02	0.06	0.14	0.50
Behavioural credit score *(units)*	16,735	14,040	16,093	17,160	17,733	18,000

Table 2

Selection effects across mortgage loan cohorts

This table reports average loan-level variables and frequency of demographic characteristics, assigning borrowers to 2 synthetic mortgage origination cohorts: the first from April to August 2010 (pre-policy) and the second from September 2010 to January 2011 (post-policy). Within each cohort, we divide the sample in first and second loans.

	Pre-policy cohort		Post-policy cohort	
	1st loan	2nd loan	1st loan	2nd loan
Loan and property characteristics				
LTV ratio *(percent)*	68.14	66.97	69.66	62.77
Mortgage interest rate spread *(percent)*	1.70	1.69	1.42	1.56
Private property *(share)*	0.46	0.62	0.35	0.63
Property value *('000s)*	$1,021.15	$1,489.06	$905.88	$1,344.28
Loan maturity *(years)*	25.06	24.44	26.95	25.37
Observed borrower characteristics				
Average age *(years)*	41.19	44.48	39.44	43.96
Average income per year *('000s)*	$140.67	$182.90	$103.43	$144.46
Length of tenure with the bank *(years)*	14.73	16.08	13.87	16.32
Foreign national *(share)*	0.30	0.23	0.32	0.21
Male *(share)*	0.76	0.83	0.75	0.81
Married *(share)*	0.58	0.70	0.53	0.71
Professional occupations *(share)*	0.52	0.51	0.50	0.44
Administrative occupations *(share)*	0.21	0.28	0.19	0.26
Graduate and postgraduate education *(share)*	0.72	0.83	0.67	0.75
Unobserved borrower behavior				
Credit card debt	$469.60	$589.19	$327.82	$765.71
Delinquency *(>30 days, frequency)*	0.25	0.26	0.21	0.34
Behavioural credit score *(units)*	752.24	762.61	762.61	543.57

Table 3
Selection effects across mortgage loan cohorts

This table reports selection effects on selected loan-level variables and demographic characteristics, from our merged sample of mortgage loans and individual population records. We assign borrowers to 2 synthetic mortgage origination cohorts: the first from April to August 2010 (pre-policy) and the second from September 2010 to January 2011 (post-policy). Within each cohort, we divide the sample in first and second loans. We report estimated selection effects, as captured by the β_4 coefficient from the following regression:

$$y_{i,n} = \beta_1 + \beta_2 1_{n=2} + \beta_3 1_{post} + \beta_4 1_{n=2} 1_{post} + \varepsilon_{i,n},$$

where we replace $y_{i,n}$ with any characteristic of interest, referring to borrower i and loan type $n = \{1, 2\}$. The dummy variables 1_{pre} and 1_{post} indicate whether the loan is part of the pre- or post-policy cohort. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Selection effect 2nd loan
Loan and property characteristics	
LTV ratio *(percent)*	-5.74***
Mortgage interest rate spread *(percent)*	0.13*
Private property *(share)*	0.13**
Property value *('000s)*	-$23.75
Loan maturity *(years)*	-0.77
Observed borrower characteristics	
Average age *(years)*	1.11
Average income per year *('000s)*	-$1.72
Length of tenure with the bank *(years)*	0.98
Foreign national *(share)*	-0.03
Male *(share)*	-0.01
Married *(share)*	0.05
Professional occupations *(share)*	-0.04
Administrative occupations *(share)*	0.00
Graduate and postgraduate education *(share)*	-0.03
Unobserved borrower behavior	
Credit card debt	$318.29
Delinquency *(>30 days, frequency)*	0.12**
Behavioural credit score *(units)*	-257.00*

Table 4
Individual financial behavior: selection vs. treatment effects

This table reports estimated coefficients from the following specification:

$$y_{i,t,n} = \delta_t + \alpha 1_{post} + \underbrace{\beta 1_{n=2}}_{\text{Characteristics}} + \underbrace{\gamma 1_{post} 1_{n=2}}_{\text{Selection effect}} + \underbrace{\tau 1_{post} 1_{n=2} 1_{obs}}_{\text{Treatment effect}} + \varepsilon_{i,t,n},$$

where $y_{i,t,n}$ are respective dependent variables, observed for customer i in month t. δ_t are time fixed effects and 1_{obs} indicates that the time period is part of the observation window between February and June 2011. All variables are normalized by subtracting the mean and dividing by the standard deviation. Spending categories include payments with both credit and debit cards. In Panel B, we include borrower fixed effects and restrict the sample to second loans originated during the post-policy period. Standard errors are reported in parentheses under the coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A

	Checking account	Total spending	Dining out	Services	Durable goods
Characteristics	0.28***	0.23***	0.10***	0.12***	0.07***
Selection effect	-0.05***	-0.12***	-0.10***	-0.05**	-0.02
Treatment effect	-0.05	0.01	0.10	0.00	-0.01
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	230,730	230,730	230,730	230,730	230,730
Adjusted R^2	0.028	0.010	0.002	0.007	0.001

Panel B

	Checking account	Total spending	Dining out	Services	Durable goods
Treatment effect	0.03	0.04	0.10*	0.05**	-0.01
Borrower fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	6,690	6,690	6,690	6,690	6,690
Adjusted R^2	0.481	0.303	0.125	0.199	0.116

Table 5

The persistence of selection effects

This table reports estimated δ_j coefficients from the following panel specification:

$$1_{p_{i,t,n}>0} = \tau_t + \beta X_{i,t,n} + \delta_1 1_{n=2} + \sum_{j=1}^{4} \left(\gamma_j 1_{cohort=j} + \delta_j 1_{cohort=j} 1_{n=2}\right) + \varepsilon_{i,t},$$

where $1_{p_{i,t,n}} > 0$ are respective indicators of credit payment difficulties by borrower i in period t, $n = \{0,1\}$ indexes first and second loans, τ_t are time fixed effects, and $X_{i,t,n}$ is a vector of individual characteristics. In the left panel, the coefficients δ_j are obtained as interaction effects between cohort dummies and an indicator variable which takes the value of 1 if the observation corresponds to a second loan. In the right panel, the latter variable takes a value of 1 if the respective borrower has a second loan outstanding. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Mortgage loan penalties	Credit card delinquency
Log average borrower's income	0.000	0.001
	(0.001)	(0.009)
Log loan amount	0.003	-0.001
	(0.002)	(0.012)
Mortgage interest rate	0.005***	0.026*
	(0.002)	(0.014)
Apr 2010 - Aug 2010 (Pre-policy)	0.000	0.027
	(0.004)	(0.019)
Sep 2010 - Jan 2011 (Post-policy)	0.003	-0.022
	(0.002)	(0.022)
Feb 2011 - Jun 2011	-0.003	0.015
	(0.002)	(0.024)
Jul 2011 - Nov 2011	0.000	-0.012
	(0.003)	(0.026)
Second-loan borrower	-0.004***	0.034
	(0.001)	(0.036)
Apr 2010 - Aug 2010 (Pre-policy, interaction)	0.002	-0.033
	(0.004)	(0.048)
Sep 2010 - Jan 2011 (Post-policy, interaction)	0.021*	0.098*
	(0.011)	(0.054)
Feb 2011 - Jun 2011 (interaction)	0.009*	-0.079
	(0.005)	(0.050)
Jul 2011 - Nov 2011 (interaction)	0.000	-0.066
	(0.003)	(0.052)
Time fixed effects	Yes	Yes
Borrower characteristics	Yes	Yes
Number of observations	66,952	119,024
Adjusted R^2	0.004	0.019

Table 6

The role of price expectations and borrower financial conditions

This table reports estimated coefficients from our benchmark regression specification. The corresponding delinquency indicator variable takes the value of one if the mortgage account has been subject to interest rate penalties at least once during the post-policy observation window between February 2011 and June 2011. The house price indicator $1_{d,price}$ takes the value of 1 for the districts with the 25 percent highest level of average house price growth between 2008 and 2010. Standard errors are reported in parentheses under the coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Pre-policy control cohort	-0.001	-0.003	-0.016	-0.003	-0.003
	(0.008)	(0.008)	(0.062)	(0.008)	(0.008)
Post-policy treatment cohort	0.030***	0.011	-0.039	0.007	0.019*
	(0.009)	(0.010)	(0.065)	(0.010)	(0.010)
District-level house prices		0.002	0.002	0.000	-0.001
		(0.003)	(0.003)	(0.002)	(0.003)
Pre-policy (interaction)		0.012	0.013	0.011	0.012
		(0.011)	(0.011)	(0.011)	(0.011)
Post-policy (interaction)		0.042***	0.043***	0.062***	0.012
		(0.012)	(0.012)	(0.012)	(0.012)
Length of tenure			0.001		
			(0.001)		
Pre-policy (interaction)			0.202		
			(0.326)		
Post-policy (interaction)			0.702		
			(0.431)		
Credit card utilization rate				0.076***	
				(0.005)	
Pre-policy (interaction)				-0.051**	
				(0.024)	
Post-policy (interaction)				0.339***	
				(0.028)	
Behavioural score					-0.011***
					(0.001)
Pre-policy (interaction)					0.006
					(0.003)
Post-policy (interaction)					-0.054***
					(0.004)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Borrower characteristics	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	16,043	16,043	16,043	16,043	15,944
Adjusted R^2	0.005	0.006	0.006	0.031	0.037

Table 7

Bankruptcy rates of property investors

This table reports estimated γ_j coefficients from the following regression specification:

$$1_{investor,i,t} = \sum_{j=1}^{5} \gamma_j 1_{cohort=j} + \varepsilon_{i,t},$$

where i indexes the court case, t is the specific month in which the bankruptcy order was issued and $1_{investor,i,t}$ is an indicator variable which takes the value of 1 if the defendant is a property investor. We use the merged sample of bankruptcy procedures, population records and transaction-level data. We identify investors as sellers of property that are not owner-occupants, i.e. the address of the property is different from the address of residence. We define time cohorts consistently with the grouping in our benchmark specification. Standard errors are reported in parentheses under the coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Mortgage bankruptcy
Apr 2010 - Aug 2010	0.07
	(0.13)
Sep 2010 - Jan 2011	0.15
	(0.12)
Feb 2011 - Jun 2011	0.19*
	(0.10)
Jul 2011 - Nov 2011	0.01
	(0.15)
No. of obs.	94
Adjusted R^2	0.008