Identifying a Suitable Control Group Based on Microeconomic Theory: The Case of Escrows in the Subprime Market

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Abstract

We analyze the effect of a Federal Reserve Board's subprime mortgage regulation requiring escrows on the availability of mortgage credit. Due to all mortgage originators being affected by the regulation, there is no natural control group for affected markets. We use the assumption of profit maximization to construct a control group. Applying a difference-in-difference strategy to a dataset constructed using Home Mortgage Disclosure Act loan level data and USDA Rural Atlas county level data, we find no statistically or economically significant impact on the loan origination markets across the U.S., despite 202 institutions exiting the subprime mortgage market in over 200

*The views expressed are those of the authors and do not necessarily represent those of the Director of the Consumer Financial Protection Bureau nor those of the staff.

counties. These results, along with other evidence presented in the paper, strongly suggest that consumers were able to switch to similar loans originated by the exiting creditors' competitors.

1 Introduction

Our ability to evaluate the causal effects of a given change is often limited by the lack of a control group or appropriate instrumental variables. For instance, it is hard to find a government regulation that applies only to a random subset of regulated entities. Regression discontinuity techniques are usually unavailable as well: even when there is a policy discontinuity, the data for the variable underlying the discontinuity is not available or the relevant effect occurs far from the discontinuity.

We propose using restrictions derived from the assumption of profit maximization to evaluate the effects of a policy change. We are interested in how a regulation that impacts firms' fixed costs affects consumers' access to products involved. From microeconomic theory, we know that in the short run this regulation should affect consumers through the supply side only if firms exit the market, thereby decreasing competition and increasing prices.[1] If we observe several markets simultaneously, then the markets that did not experience exits constitute a control group for markets that did experience exits, controlling for all other observable characteristics of markets. Of course, it is also necessary to assume that observed exits are not correlated with unobserved market characteristics. We apply this method to analyze the effects of a 2009 Federal Reserve Board regulation that required escrow accounts

[1] However, see Chen and Riordan (2008) for a model of price increasing competition. Note that the control group remains the same even in this case.

for higher-priced mortgage loans (HPMLs; which roughly correspond to the more colloquial "subprime mortgages").

This method can be applied more broadly. The policy change does not have to be a regulation. Also, the change does not have to affect just the fixed costs of regulation. For a regulation or change that also affects marginal costs, researchers can utilize other market structure assumptions, such as Nash equilibrium. In a competitive market, a marginal cost change that affects all firms equally should be fully passed through in prices.[2] On the other hand, a monopolist passes through just some of this cost shock, depending on the shape of the demand function, which the researchers could estimate separately (for example, by examining the pass through rate of other costs). Alternatively, in many cases, at least some of the firms will be unaffected by the policy change; for example, some firms may already be in compliance with the new policy. In these cases, markets consisting of these firms can serve as a control group for a market of firms that are affected by the policy change. We would still require that the fact that a market is, say, a monopoly market, is uncorrelated with market unobservables. In many cases this will be an heroic assumption; however, in many other cases, including this one, almost all relevant market-level variables are observable.

This idea is not new. Arguably, most of the structural industrial organization literature is based on these types of assumptions (profit maximization and Nash equilibrium). However, we argue that for many purposes using microeconomic theory based assumptions to identify a control group for reduced form estimation requires fewer parametric assumptions and less time and resources (both computing and manpower) than a full-blown structural model.

[2] This, of course, depends on the exact shapes of the demand and cost functions, see Weyl and Fabinger (2013).

Furthermore, the intuition of reduced form techniques may be easier to explain to policy makers and courts.

1.1 Application

From a consumer welfare perspective, most regulations present a trade-off. On the one hand, consumers receive the benefit of regulation, whether this benefit is a protection associated with regulation or anything else (from now on, we will assume for brevity, and to coincide with our empirical application, that the benefit is a consumer protection). On the other hand, firms in the market have to incur fixed, and possibly variable, cost of regulation, causing some to exit the market, and the rest to potentially increase their prices due to a decrease in competition. Firms increasing prices may lead to fewer transactions in the regulated market (we call this effect reduced access to credit from now on). Thus, the primary tradeoff for the regulator is between reduced access to credit due to the regulation's effect on firms and offering protection to the remaining consumers.[3]

Using the method outlined above, we empirically test whether the first post-crisis subprime mortgage regulation reduced access to credit. We find no evidence of this, despite about 200 institutions ceasing to originate HPMLs around the time of the rule becoming effective. Furthermore, based on our results, similar regulations affecting mortgage creditors in the U.S. – by a fixed cost shock of a similar size (anecdotally, up to $100,000 per financial

[3]To estimate the long-term effect of the regulation on consumer welfare, one also needs to know the value that consumers derive from the protections offered. However, in many markets this will not be possible to do from the revealed preference data, as many regulations are there exactly for the products for which consumers mis-estimate benefits. In particular, we are not aware of any evidence that consumers value escrow provision when they are deciding on which mortgage loan to apply for. Thus, we abstract from examining the longer term effects of this regulation.

institution) – are unlikely to adversely affect access to credit, even if many institutions stop lending in a particular space of the mortgage market. The reason is straight-forward: other products offered by the same creditor or same products by creditors deciding to stay in the market are sufficiently good substitutes to make the effects on consumer demand negligible.

Finding of no statistically significant results raises a question of whether the control group that we are using is a valid one. Even in the case that our profit maximization assumption does not hold, we show that there is no statistically significant effect of 202 creditors exiting the HPML market – an important finding in and of itself. That said, we do believe that the profit profit maximization assumption holds, and that we actually show that there were no statistically significant effects of the rule on access to credit.

The exiting creditors are mostly small community banks and credit unions. These creditors originated just under 50 mortgage loans on average in the first quarter of 2010, and approximately 60% are supervised by either the FDIC or the NCUA. Approximately 65% of them originated only one HPML in the first quarter of 2010.

It is hard to overstate the importance of the subprime mortgage market in the United States, especially in light of the recent crisis. On the one hand, this is the only source of mortgage credit for a particularly disadvantaged segment of the population. On the other hand, as the U.S. Financial Crisis Inquiry Commission (2011) mentioned in its report, "explosion in subprime lending, [...] widespread reports of egregious and predatory lending practices, (and) dramatic increases in household mortgage debt" were, in retrospect, indicators of the upcoming crisis.

The Federal Reserve Board's (FRB) Escrow Rule became effective on April 1st 2010: any first lien HPML subject to the Truth in Lending Act, originated on this date or after,

must have an escrow account.[4] The Escrow Rule was adopted for the purposes of protecting subprime consumers by smoothing their payments and facilitating shopping for mortgages. Anderson and Dokko (2010) show that escrows decrease the incidence of foreclosure in the subprime mortgage market via smoothing out the payment stream – instead of paying property taxes and home insurance once or twice a year, consumers pay smaller amounts every month. Also, if consumers are uncertain about their mortgage terms, as Bucks and Pence (2008) find, then property taxes and home insurance become hidden fees that some consumers might discover only after consummating the transaction, resulting in suboptimal choices among creditors and consumers buying more expensive houses than they would have wanted to buy, had more information been available. The Escrow Rule was a part of the first package of mortgage rules designed to prevent another subprime crisis, together with the FRB's Ability-to-Repay Rule. The other rules in this package became effective half a year earlier than the Escrow rule, allowing us to identify the effects of the Escrow Rule separately. Incidentally, the stated reason for the delay of the Escrow Rule was to allow creditors more time for implementation (18 months overall, reflecting the non-trivial implementation requirements and thus a sizable fixed cost).

Aside from the microeconomic theory assumption of no pass-through of fixed costs (assuming profit maximization is sufficient in the short run), a potential methodological challenge is the possibility that firms exiting the HPML markets in counties of otherwise similar observed characteristics could be correlated with unobserved county characteristics.[5] Our

[4] Escrow accounts are generally used for property taxes and any creditor-required payments, for example home insurance.

[5] In the long run this regulation might prevent additional entry into the market and protect incumbents, conforming with Stigler (1971)'s view of regulation.

data allows us to control for a large number of county-level characteristics. Thus, we believe it is unlikely that there is omitted or confounding variable bias that would result in this correlation. We also do not believe that simultaneity is an issue, given the nature of the policy change.

In our evaluation of the effects of the FRB Escrow Rule, we find that while approximately 200 financial institutions exited the HPML market, we do not find a statistically or an economically significant negative effect on the overall volume of transactions. The finding suggests that not only did the remaining firms increase their volume of transactions, but also that there was enough competition in most of the markets to ensure that prices do not increase significantly despite some of the firms exiting.

Mortgage regulations that introduce a fixed cost shock of a similar magnitude to smaller creditors would arguably include most of the recent mortgage regulations passed by the Consumer Financial Protection Bureau (CFPB). Arguably, the CFPB has recently either created or re-written three regulations that affect most originators: Ability-to-Repay, which requires creditors to verify consumers' ability to repay the loan; Loan Originator Compensation, which sets limits on how financial institutions can set up their incentive schemes; and the TILA-RESPA disclosure rule that combined and simplified disclosures that a consumer receives during the origination process. While these three rules have other features, the immediate consequence is that financial institutions have to pay the implementation cost of learning the regulations, getting their systems ready, and training their personnel–all fixed costs akin to the ones that the regulation that we examine in this paper required. Our results suggest that consumers will enjoy the protections offered by these regulations (and any market externalities being corrected), without the downside of a higher price or decreased

access to credit.

1.2 Policy Background

The Board's HPML definition was designed to encompass what is commonly known as the subprime mortgage market. For first lien loans an HPML was defined as a loan with an APR of more than $APOR + 1.5\%$, where APOR is the average prime offer rate, computed weekly using data from the Freddie Mac Primary Mortgage Market Survey. In the rest of the paper we restrict the discussion to the HPML first lien loans subject to the Truth in Lending Act (which applies to roughly all residential mortgage loans).

From the creditor side, the rule was expected to be costly, particularly for smaller creditors. Anecdotal evidence suggested costs of upward of a $100,000 to adjust the computer systems, to train staff, and to go through all the steps necessary for implementation. All these costs are fixed and most of them are one-time sunk costs. While the Board's Ability-to-Repay Rule became effective in 2009, the Escrow Rule did not become effective until April 2010 due to "[l]arge creditors without escrow systems [asking] for 12 to 24 months to comply." The Board also received comments implicitly stating concerns regarding consumers' access to credit due to the rule: "[c]ommenters also contended that setting up an escrow infrastructure would be very expensive; creditors will either pass on these costs to consumers or decline to originate higher-priced mortgage loans (See Federal Register (2008))."

There are several methodological challenges in estimating the effect of this policy. These include the standard program evaluation problem of a lack of a natural control group when the policy being evaluated applies universally. The main data challenges are that we do not

directly observe exits and we do not know whether an exit is due to the regulation. We classify creditors as exiting if they originated at least one HPML from January 1st, 2010 until March 31st, 2010; has not originated any HPMLs from April 1st, 2010 to December 31st, 2011; and has originated at least some residential mortgage loans. We assume that every such exit is due to the regulation. The last clause in the exit definition is not to attribute any financial institutions that merged or went bankrupt to this regulation that was unlikely to cause any creditors to go bankrupt or exit the much broader residential mortgage market completely (at that point HPMLs accounted for only about 4 percent of the overall residential mortgage market). However, we conservatively count any institutions that stopped originating mortgages in a particular county overall as having exited due to the Escrow Rule.[6]

2 Empirical Strategy

2.1 Challenges

As noted before, all regulations and many major market changes affect a non-random subset of entities. Moreover, many regulations, including this one, affect all entities. At least partially due to that, many researchers revert either to pre-post identification strategies or to using control groups from related markets not subject to the regulation (for example, the small business credit card market as a control for the consumer credit card market, see Agarwal, Chomsisengphet, Mahoney, and Stroebel 2014). Generally, either of the two

[6] We define markets as counties and county equivalents, resulting in 3,273 markets. See a discussion below for more information, and see Sharfstein and Sunderam (2013) for a recent related work using the same market definition.

strategies requires much stronger assumptions on data and underlying processes than the assumptions that we make here.

2.2 Microtheoretic assumptions

The Escrow Rule's imposed a sizable one-time fixed cost on creditors that wanted to stay in the HPML market and that did not have an escrow system already in place. We assume that firms either incur this one-time cost without passing it through to the consumers or exit the HPML market. Thus, any market (county) where we observe no exits is a control market – prices should not have changed due to the Escrow Rule. For that conclusion to be true, we also assume that consumers in the HPML market do not increase their demand due to the fact that every creditor must provide an escrow. This seems like a plausible assumption: we could not find any evidence that consumers value having an escrow account when they apply for mortgages and, if anything, there is some anecdotal evidence to the contrary, biasing our results to find a negative effect where none actually exists.

An alternative specification, instead of profit maximization in the paragraph above, could compare effects between competitive and non-competitive markets. In competitive markets, many creditors would already have escrow systems implemented, and thus creditors in these markets would not be able to increase their prices in a meaningful way, since the market is sufficiently competitive. However, this specification, on top of profit maximization, assumes Nash equilibrium, and thus, for this application, we chose the less restrictive assumption of profit maximization.

2.3 Exits and correlation with unobservables

It is theoretically possible that, even conditional on the observables, probability of an exit in a market is correlated with some market-level unobservables. We believe that this is not the case in our setup. We have many county-level variables, including the future values of these variables from the point of view of the firm deciding whether to exit at the end of March 2010. Thus, we can arguably even control for expectations of creditors, as long as these expectations are rational, or at least irrational in exactly the same way.

We are not concerned about strategic timing in the form of creditors exiting the market earlier due to foreseeing the rule. Any creditor could have originated HPMLs up to the day that the rule became effective with the only potential downside of a loan not being originated by the deadline being having to price that loan a bit lower after the deadline, to escape the HPML threshold. If anything, we expect that the creditors had an incentive to accelerate originations of HPMLs that would have otherwise occurred in the weeks immediately after the effective date of the regulation. If this was indeed the case, then we should observe results akin to the post-promotion dip in sales of stockpilable products after a temporary price reduction, and that would bias us to find statistically significant adverse effects of the regulation when in fact there are none. However, even this type of strategic timing is likely to be severely limited in practice.

2.4 Not observing exits and other unobservables

Creditors do not report whether they are in the HPML market. Instead, we observe whether they made any HPMLs after the effective date of the regulation. We count creditors as

being active in the market pre-regulation if the creditor originated any HPMLs in the U.S. in either January, February, or March of 2010. We assume that a creditor exited the market post-regulation if the creditor was active pre-regulation, but had not originated any HPMLs in the 21 months immediately following the effective date of the regulation (remaining nine months of 2010 nor at any point in 2011). Given the magnitude of the rule, we do not believe, and have not heard any anecdotal evidence, that this rule would cause a creditor to exit mortgage origination overall. Thus, we do not consider creditors that exited mortgage origination overall post-regulation as exits due to the Escrow Rule. It is theoretically possible that a creditor owned two separate entities with one of them focusing solely on subprime lending. However, the data shows that the vast majority of creditors exiting the HPML market are not large enough to justify this type of corporate bifurcation and originate many non-HPMLs as well.

However, we do include creditors that stop originating mortgages in a given county overall (by, for example, closing a branch there) as an exit that could have, conceivably, been caused by the Escrow Rule. While HPMLs account for less than 4% of the overall residential mortgage market, they are likely to be more profitable and are more prevalent in some of the branches. Thus, this rule could potentially have a disproportionate effect on the business decision to keep operating a particular branch. Again, we are likely picking up at least some exits here that are not actually attributable to the Escrow Rule. Also, due to the fact that HPMLs are originated relatively infrequently by the smaller creditors, it is likely that at least some of the HPML market exits that we attribute to our rule are not actually attributable to our rule.

2.5 Difference-in-differences

At this point, we have a control group and a treatment group, and data from before and after the regulation, and thus we can apply standard difference-in-difference techniques.

Since escrow is based on property taxes, and these are determined at the county, if not the municipality level, we consider each county a separate market and use the county as our level of observation. We can evaluate the effect of the FRB Escrow Rule on access to credit by comparing origination counts before and after the policy change in the counties that experience an exit to those that do not using a difference-in-difference (DD) strategy, which is implemented in equation **1,**

$$y_{it} = f\left(\beta_{post} post_{it} + \beta_{exit} exit_{it} + \beta_{post \times exit} post_{it} \times exit_{it} + \beta_X X_{it} + \varepsilon_{it}\right) \tag{1}$$

where $post_{it}$ is an indicator for after the FRB Escrow Rule took effect, $exit_{it}$ is an indicator for being in a market that experienced a firm exit, X_{it} is a vector of covariates including a constant term, ε_{it} is an error term, and f is a function. We consider HPML counts as outcomes, and the marginal effect corresponding to $\beta_{post \times exit}$ represents the program effect.[7]

To account for the fact that the outcomes are count variables, we consider negative binomial regression model (NB):

$$\Pr\left(y_i | x_i\right) = \frac{\Gamma\left(y_i + \alpha^{-1}\right)}{y_i! \Gamma\left(\alpha^{-1}\right)} \left(\frac{\alpha^{-1}}{\alpha^{-1} + \mu_i}\right)^{\alpha^{-1}} \left(\frac{\mu_i}{\alpha^{-1} + \mu_i}\right)^{y} \tag{2}$$

$$\mu_i = \exp(\beta_{post} post_i + \beta_{exit} exit_i + \beta_{post \times exit} post_i \times exit_i + \beta_X X_i + \varepsilon_i)$$

[7] Note that $\beta_{post \times exit}$ may overstate the intent-to-treat effect of the regulation because, by construction, the "exit" group includes only markets for which the regulation was binding.

where $\exp(\varepsilon_i)$ is assumed to be distributed gamma with dispersion parameter α. As in the linear model, we take the coefficient $\beta_{post\times exit}$ to represent the program effect. Since HPML loans are relatively rare, we think that there may be an excess zeros problem, and correct for it by considering the zero-inflated negative binomial (ZINB) model as well. Suppose that there are two possible ways that we observe a zero count of the outcome variable, and let the first group, $A_i = 1$, be the group where the count is always zero and $A_i = 0$ be the group where the the count may be zero or non-zero. For $A_i = 0$, we estimate a negative binomial model as above, where

$$\Pr(y_i|x_i, A_i = 0) = \frac{\Gamma(y_i + \alpha^{-1})}{y_i!\Gamma(\alpha^{-1})}\left(\frac{\alpha^{-1}}{\alpha^{-1} + \mu_i}\right)^{\alpha^{-1}}\left(\frac{\mu_i}{\alpha^{-1} + \mu_i}\right)^{y} \tag{3}$$

$$\mu_i = \exp(\beta_{post}post_i + \beta_{exit}exit_i + \beta_{post\times exit}post_i \times exit_i + \beta_X X_i + \varepsilon_i)$$

and for $A_i = 1$ we estimate a logit model

$$\Pr(A_i = 1|post_i, exit_i, post \times exit_{i,} z_i) \tag{4}$$

$$= \frac{\exp(\gamma_{post}post_i + \gamma_{exit}exit_i + \gamma_{post\times exit}post_i \times exit_i + \gamma_Z Z_i)}{1 + \exp(\gamma_{post}post_i + \gamma_{exit}exit_i + \gamma_{post\times exit}post_i \times exit_i + \gamma_Z Z_i)}$$

where Z_i is a vector of covariates. We can interpret the marginal effect of $\gamma_{post\times exit}$ as the effect the FRB Escrow Rule had on the probability that loans of a given type are uniformly zero.

A similar analysis can be performed at the firm level in order to estimate the effects of the policy HPML volumes of exiting and non-exiting firms. The specifications are completely analogous to the county level analysis, except the level of observation is the firm. Note, that

we cannot use the same type of theory-identified control groups for firm-level analysis, and thus the identification there is based solely on observables and not on the profit maximization assumption.

3 Data

We analyze the 2010 and 2011 Home Mortgage Disclosure Act (HMDA) loan level files for originations data and the USDA Rural Atlas for county level characteristics. The HMDA definition of HPML changed in the last quarter of 2009: loans applied for prior to September 1, 2009 but originated before the end of 2009 are classified as HPML based on the spread of their APR over the "comparable treasury security" utilizing the "Treasury Securities of Comparable Maturity under Regulation C" table, action taken, lock-in date, APR, term (loan maturity), and lien status, and loans with application dates of September 1, 2009 or later or originated in 2010 or later are defined as HPML based on the spread of their APR over Average Prime Offer Rate. Moreover, in the last quarter of 2009, financial institutions were free to report to HMDA either according to the old regime or according to the new regime. Consequently, 2009 HPMLs are not comparable to 2010 or later HPMLs, so our sample period begins January 1, 2010, as illustrated in Figure 1. Since our period prior to the implementation of the FRB Escrow Rule is restricted to the first quarter of 2010, we restrict our post-period to the first quarter of 2011 in order to avoid any seasonal effects.

Since we do not directly observe whether firms are equipped to provide escrow accounts, we proxy for exits by identifying firms that exit the HPML market after the policy change. We classify a firm as active in the HPML market if it originates at least 1 HPML loan in

the sample period prior to April 1, 2010. A firm is considered to have exited the HPML market if it originated HPMLs before April 1, 2010 but it did not originate HPMLs for 21 months (until the end of 2011) after the rule was implemented. Counties are considered to be exposed to an exit if a firm that was active in that county prior to April 1, 2010 exited the HPML market but did not stop originating mortgages overall (we will refer to first-lien loans subject to the Truth in Lending Act as the overall mortgage market).[8]

The process of constructing the final county level mortgage dataset with county level characteristics and exit definitions is illustrated in Figure 2. We begin by collapsing the HMDA loan level data into three distinct datasets: a firm level HMDA dataset, a firm-by-county level dataset, and a county level dataset. Each observation in the firm level dataset represents a distinct creditor, either in the period before or after the policy change. From this dataset we generate firm exits. We then merge this data by firm to the firm-by-county level dataset, where observations are defined by firm and county and whether or they appear in the pre-period or the post-period. This allows us to define county exposure to exits. We then merge the county level exits by county to the county level dataset, which is defined by county by month. Finally, the county-level characteristics are merged on by county using the data from the USDA Rural Atlas.[9]

[8] We recognize that this definition of exit introduces measurement error in the the definition of counties that are exposed to a policy change, and may attenuate the results. We expect to address this in the future by computing the bounds on the loans following Bollinger (1996).

[9] The Dodd-Frank Wall Street and Consumer Protection Act of 2010 (Dodd-Frank Act) became effective on July 21, 2011. This resulted in changes in regulatory authority over HMDA reporters: all institutions with assets greater than or equal to $10 billion were assigned to the newly created Consumer Financial Protection Bureau, and all institutions below $10 billion in assets that were formerly supervised by the Office of Thrift Supervision were transferred to the Office of the Comptroller of the Currency. Consequently, HMDA agency codes and respondent IDs were changed as of the 2011 reporting year. To assure that we have consistent firm definitions across the 2010 and 2011 reporting years, we made use of a crosswalk between the 2010 and 2011 panel created in the process of conducting impact analyses for the Dodd-Frank Act Title XIV rulemakings at the Consumer Financial Protection Bureau. HMDA reporters that changed identifiers due to the Dodd-Frank Act were matched to their 2011 reporting identifiers, and nonmatching HMDA reporters

There are limitations to the HMDA data, insofar as it does not cover all mortgage loans. By construction, HMDA may disproportionately exclude certain types of loans. Institutions that do not have a home or branch office in a Metropolitan Statistical Area (MSA) or do not receive applications for, originate, or purchase loans that correspond to property located in an MSA in the previous year are exempt from reporting, which may result in undercounts of loans in rural areas. Non-profit institutions and non-depository institutions with less than $10 million in assets the previous year or that originate 100 or less home purchase loans, including refinances of home purchase loans, are also exempt from reporting, so small creditors may also be underrepresented. Based on the CFPB's matching of HMDA data with data from Call Reports and projecting mortgage originations out of sample, we believe that HMDA captures at least 95 percent of the mortgage market and about 80 percent of the market in the more rural (non-MSA) counties. Our methods should capture exits of HMDA reporters, and to the extent that exits are not counted because of non-reporting, this should result in downward bias of our estimates due to mismeasurement of a binary variable, as addressed in Aigner (1973) and Bollinger (1996).

Table 1 summarizes the characteristics of the 246 counties that experience an exit and the 3,023 counties that do not experience an exit.[10] The counties that experience an exit are similar in average household size, the fraction of the population that is age 65 and older, and the fraction of housing that is owner occupied (approximately 70%). The counties that experience an exit are on average more populous, with average populations of 478,772 versus 167,677 in counties that do not experience an exit, so the fact that there are more HPMLs

were investigated manually. We are grateful to Brian Bucks for creating this crosswalk and sharing it with us.

[10] We count any county equivalents as counties.

on average in counties that experience an exit is not surprising. While apriori one might expect more exits in rural counties where smaller creditors that do not already have escrow capabilities are likely to be better represented, this is not necessarily the case in equilibrium. Less rural markets are generally more competitive, and the smaller creditors not being able to originate escrows seem to be in more danger of exit in these larger and denser markets.

Counties that experience an exit have a slightly higher fraction of minority residents and college educated residents. This is unsurprising because 76% of counties that experience an exit are in metro areas versus 32% counties that do not experience an exit. The average HPML and the overall residential mortgage market lending volume is higher in counties that experience an exit, and the percentage of mortgages that are HPMLs is 2.5% and 4.4% respectively. Table 2 summarizes the characteristics of the 202 firms that exited the market and the 6,489 firms that do not.[11] In the first quarter of 2010, exiting firms originated 378 of 50,437 HPMLs originated and 9,718 of 1,328,118 of mortgages originated. Exiting firms had a 2010 median asset size of $218 million compared to a median asset size of $207 million for non-exiting firms. Unsurprisingly, exiting firms are also more likely to have only originated one HPML in the pre-period: 65% versus 11%.

4 Results

To put the results below in perspective, if the access to credit hypothesis provided by the creditors in their comments is correct, then we would expect to see a significant reduction in HPMLs due to the rule. In contrast, if every creditor perfectly adjusts to the rule and

[11] Of the 202 exiting firms, 196 either have less than $2 billion in assets or originate less than 500 loans in 2010, which may classify them as small creditors.

keeps the same practices or there is no market effect despite some creditors exiting, then we would expect no discernible effects in either the HPML market. We show below that we see the second case – no significant effect in the HPML market.

As can be seen from figures 3 and 4, prior to the introduction of the FRB Escrow Rule, originations of HPMLs and mortgages in counties that experienced an exit track the originations of counties that did not experience an exit. In quantifying the effect of the rule, we first consider the effect of the rule on HPML originations in the first row of Table 3. Given the fact that the outcome is count data, our preferred specification is the negative binomial regression presented in column 2 because it makes percentage changes in loan volumes accessible and comparable across outcomes and treatment status. Each model includes controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

From the first row of Panel A of Table 3, we see that HPML volumes do not change significantly in response to the implementation of the escrow rule in counties that experience an exit from the HPML market, and the second row shows that HPML purchases were not adversely affected either. As mentioned above, HMDA reporting criteria may lead to the misclassification of counties that experienced an exit as counties that did not experience an exit, which would bias the results towards zero. Since the coefficients on HPML originations are positive, constructing lower bounds, as constructed by Aigner (1973), would not reverse the directionality of the result. The firms that exited the HPML market may have been less likely to sell the HPML they originated: column 2 of row 1 of Panel B shows that the number of HPML held in portfolio in counties that experienced an exit declined by approximately

8%. Since consumers typically do not have a choice over whether their loans are sold or not, this should have no impact on demand for credit. Therefore, consumer access to credit was not adversely affected by access the fixed implementation costs associated with the Escrow Rule.

One concern is that there may be a disparate impact on a particular group of consumers. In order to explore this, we return to the loan file and consider loans that are made to consumers in low income groups and minority groups. The results are displayed in Table 4, and show that neither borrowers with reported income below their county median nor in the bottom 40% of the national household income distribution have their HPML loan volumes significantly affected by the Escrow Rule. The Rule also does not have a significant adverse effect on black or hispanic borrowers. Another group that we consider separately are consumers that are borrowing loans secured by property in non-metro counties. Anecdotally, rural lending may differ from metro area lending because there might be fewer lenders available or a higher proportion of loans that must be held on portfolio because the property they are collateralized **by** makes it difficult to sell the loans on the secondary market. Table 5 presents the NBRM results stratified by metro area status. Contrary to the anecdotal argument, it appears from columns 1 and 3 that there is no significant effect of the Escrow Rule on HPML originations in either metro or non-metro areas, and that the significant 16% reduction in portfolio HPML occurs in metro areas while there is no significant decrease in portfolio HPML in non-metro areas.

In Table 6 we consider the effect of the policy on average loan size and spread over APOR. Focusing on the results for the specification in column 4, which includes controls for borrower and county level characteristics, we see that average loan amounts decrease by approximately

$1,300 in response to the implementation of the Escrow Rule, and this results is significant at the 10% level. This could be an indication of consumers and creditors adjusting to the rule by ensuring that consumers take out smaller loans that would have rates that could fall under the HPML limit.

In row 2 we see that when we restrict attention to HPML, the spread over APOR increases by 9.8 basis points, which is statistically significant at the 10% level, but is not a large economic impact. There is also no discernible effect of this price increase on demand, indicating that while an individual creditor's demand might be somewhat elastic, the overall demand for mortgages is relatively inelastic. A more meaningful outcome is the effect of the Escrow Rule on average APRs relative to APOR, but due to HMDA reporting criteria, we only observe spreads that are at least 1.5 over APOR. Consequently, we attempt to capture the effect of the rule on APR for both HPML and non-HPML in row 3 by running a tobit model where the spread over APOR is censored at 1.5 for all non-HPML, and find that the result is negative and insignificant.

Table 7 documents the policy's effect on firms that exited the HPML market. Controlling for pre-period originations in column 1 and pre-period originations and firm characteristics in column, the OLS estimates of the effect of the program on loan volumes at these firms are significantly negative for all mortgages and all purchase mortgages (in contrast to refinances or home improvements). The point estimates in the NBRM specifications in column 2 and 4 are insignificantly negative for mortgages overall. This is consistent with the substitution of HPMLs from non-exiting lenders dominating the extent to which borrowers switch from HPMLs to non-HPMLs at exiting lenders. In turn, this suggests that consumers are able to switch creditors when the products that they demand are not offered by a given creditor.

5 Robustness Checks

5.1 Alternate Definitions of Exit

We also consider alternative definition of exit: a county is considered to be exposed to an exit if a firm that originated any HPMLs in the county prior to the FRB Escrow Rule exits the HPML market nationally. This differs from the primary definition used in this paper because it also includes exits by firms that exit mortgage lending entirely, and we believe that these firms may have been more likely to exit for non-Escrow Rule related reasons. Nevertheless, the results from this analysis are qualitatively similar to those presented for our preferred exit definition, and can be found in Table 8.

As we mentioned above, the definition of HPML changed in the last quarter of 2009: the threshold APR changed from 3 over a treasury security of comparable maturity to 1.5 over APOR. Figure 7 plots the two thresholds daily from 2006 through 2011 for 30 year fixed rate mortgages. In 2009 the new definition of HPML tends to be a subset of the previous definition of HPML, so using the old definition to define HPMLs prior to the policy change should result in an undercount of HPML originations, but may still capture HPML originators that we did not observe in the 2010Q1 pre-period. When we extend the pre-period to include January through December of 2009, we find that our count of exiting firms increases from 202 to 270. The results of a replication of our main estimates with this extended pre-period definition of exit are displayed in Table 10. Similar to our findings in Table 3, we find that the point estimates for the effects of the Escrow Rule on HPM originations and HPM purchase originations are positive and insignificant in the NBRM and ZINB specifications in columns 2 and 3.

5.2 Propensity Score Matching

We match counties based on pre-policy change loan characteristics and county level characteristics, using both nearest neighbor matching with 2 neighbors and Epachnikov kernel matching. Then we compare originations in January, February, and March of 2011 for counties that experience an exit with originations in their matched counties. For example, the Washington, DC area county Prince George's County, MD experienced an exit and is matched with Bristol County, MA, which is part of the metropolitan statistical area that includes Providence, RI and did not experience an exit. The results are presented in Table 11. Analogous to our negative binomial regression model difference-in-differences results in Table 3, our estimates do not reject the hypothesis that there is no change in HPML originations in affected counties due to the FRB Escrow Rule. We also present balancing test results for both the nearest neighbor and Epachnikov kernel matching in Table 12. Note that we fail to reject that the treatment and matched control groups are not balanced for all characteristics, except for the proportion of a county that is of hispanic origin in the metric that uses Epachnikov kernel weighting.

5.3 Synthetic Control Groups

Since there are over 3,000 counties, we reduce the dimensionality of the construction of synthetic control groups by first k-median clustering counties based on their characteristics and their patterns of HPML and overall mortgage originations prior to the policy change, and then construct synthetic control groups for counties using the methods described in Abadie and Gardeazabal (2003) to weight counties that have not experienced an exit. Due

to computational limitations, we restrict the number of comparison counties to 100 or less. The results are presented graphically in Figure 6 and Figure 8, for HPMLs and overall residential mortgages respectively. Each gray line represents the difference in originations between a county that experiences an exit and its synthetic counterfactual county. The maroon lines represent the mean of the estimated differences for all counties, and the dashed lines represent the upper and lower bounds of the 95% confidence interval of the mean. The difference between the counties that experienced an exit and their synthetic controls is indistinguishable from zero for all periods after the introduction of the FRB Escrow Rule, which is consistent with our results using other methods.

For illustrative purposes, we have presented the relative weights of the donor counties for the synthetic control group for Prince George's County, MD in Table 13. While synthetic Prince George's County is a weighted average of 4 counties, the donor pool actually consisted of 49 counties, the remainder of which were weighted 0 in the synthetic control group. Table 14 presents the values of HPML and overall residential mortgage origination counts in the months preceding the policy change for Prince George's County and its synthetic control group. Note that HPML originations do not differ between the synthetic and control group by more than 0.96% for the 3 months preceding the policy change.

5.4 Census Tract Level Analysis

We also replicate the analysis at the Census tract level, and using the methodology above find that the Escrow Rule is associated with a 53% decline in HPML originations and a 3.2% decline in overall residential mortgage origination volume. We present the results in Table

9. Note that this finding is consistent with a hypothesis of creditors either originating loans just under the HPML limit of 1.5% over APOR instead of originating loans over the limit or with a hypothesis of consumers switching to cheaper creditors.

We believe that the small size of Census tracts mechanically results in large percentage changes in originations being associated with a tract experiencing an exit as well as in in severe classification error for our exit definition, which will bias any results downwards. We find that only 1,072 of the 65,166 observed Census tracts are classified as experiencing an exit. We also find that 99.7% of census tracts had single-digit HPML originations in the three months immediately preceding the regulation's effective date, including the 59.3% of tracts that had 0 HPML originations over the period. Moreover, 454 out of the 1072 tracts had just one HPML origination and 302 out of 1072 tracts had just two HPML originations. Note that the way that we construct our exit definition forces the tract to have at least one HPML origination in the pre period. However, if we do not see any HPML originations in our three month comparison period post the effective date, there is a high likelihood that this is due to chance, thus the fact that we constrain the pre period to have at least one origination necessarily biases our results.

In addition to the data issues, anecdotally, most of the larger creditors do not use different price sheets for different locations in the same state, with notable exceptions of California and Texas, where a creditor might use two different price sheets for the whole state. Thus, we find it hard to believe that many creditors would customize their pricing by census tract. Furthermore, customizing pricing by census tracts might raise (potentially unwarranted) redlining concerns from the regulators. Even further, for the purposes of bank merger review, the FRB generally defines markets as either counties or groups of counties, suggesting that

the census tract definition might be too fine of a grid.

Note that for this robustness check we could utilize more controls than the USDA data for each county. We merged our dataset with information from the Census Bureau on each of the census tracts, effectively recreating the data process described above. We also used some of the USDA county-level controls. The overall population of these 1,072 tracts was 327,204 and 4,953 total HPMLs were made in these counties in the first 3 months of 2010.

5.5 Counterfactual Implementation Dates

As a validation test, we replicate our analysis using counterfactual quarters of implementation, both using alternative dates to construct the exit definitions as well as using alternative dates for the policy change on the counties that are classified as exiting in the main analysis. The results that adjust the exit definition differ from those that we find when comparing the first quarters of 2010 and 2011, which suggests that our analysis is not just capturing year-over-year trends.

The results from the analysis of changing the implementation date for the same set of counties that experienced an exit generates statistically insignificant results with point estimates that are small and have negative signs for the effect of the counterfactual policy change on overall residential mortgage originations. Results from a representative counterfactual date experiment using the second quarter of 2010 and 2011 are presented in Table 15.

5.6 Entry

Theoretically, exiting firms might simply be replaced by firms that enter the HPML market in the county. If this were the case then the rule would effectively have no effect on market composition. To test whether exiting firms are replaced by entering firms, we run a probit regression of whether a county experiences firm entry on an indicator for experiencing an exit. A specification that include controls for county-level characteristics, including January 2010 HPML and overall mortgage volumes produces a statistically insignificant estimated coefficient of -0.104 (0.118) on the exit indicator variable. This shows that firm exit is not offset by firm entry.

5.7 State-Specific Effects

Industry participants have stated that some states have more onerous escrow processes than other, and often cite Pennsylvania as an example of a state with complex requirements. More involved escrow procedures should be associated with larger fixed costs in implementing an escrow system, so we would expect that the effects of the Escrow Rule to be larger in these states. We test this by adding an indicator variable for being in Pennsylviania as well as interactions of this indicator variable with being in a county that experiences an exit and being in a county that experiences an exit in the post-period. The results are presented in Table 16: relative to other counties that experience exits, Pennsylvanian **counties** that experience an exit experience an approximately 31.4% larger decline in HPMLs, from the result in the second row of column 2. However, overall lending is not adversely affected in these counties: overall residential mortgage originations actually increase by approximately

5 percent, as seen in the fourth row of column 2. This finding is consistent with a hypothesis of creditors either originating loans just under the HPML limit of 1.5% over APOR instead of originating loans over the limit or with a hypothesis of consumers switching to cheaper creditors.

6 Consumer Financial Protection Bureau's Escrow Rule

The Dodd-Frank Act required the Bureau to pass another Escrow Rule, that was finalized in January 2013 and became effective in June 2013. The market affected was still the HPML market. The rule required five years of mandatory escrow provision, as opposed to one year as in the FRB's rule. Congress also wanted the CFPB to define small creditors that operate in rural or underserved markets (also to be defined by the CFPB). These creditors would be exempt: they would not have to provide an escrow while originating an HPML. Congress specifically cited concerns regarding access to credit in these areas as the motivation for this exemption. Of course, this also implies that these consumers would not receive the protections of the rule. The CFPB designated over 1,600 counties across the U.S. as rural or underserved (in other words, around half of all the U.S. counties), and small creditors that operate in these counties receive certain exemptions from many of the newly released mortgage rules, including the Escrow Rule.

Our loan-level mortgage data comes from HMDA, a dataset that is collected on a yearly basis. The dataset that will include time before and after the CFPB's Escrow Rule became effective will not be available until early Fall of 2014, and even then matching it to all the other datasets used in this study (including previous years' HMDA) will take months. How-

ever, we and/or other researchers will be able to double-check the results in this study. We can apply similar estimation techniques to see whether the exempted counties experienced more loan origination than the control counties defined as above. Moreover, we can use more standard regression discontinuity techniques to validate that result and to see whether microtheoretic-based control groups lead to roughly the same results as more standard techniques when these more standard techniques are available. These techniques would certainly be available in this case: small creditors are defined as the ones that originated under 500 loans in the previous year (2012). However, even at the end of 2012 it would have been hard to guess that the January 2013 rule would prescribe exactly 500 as the limit (in particular, the proposed version from several years before the rule had 100 portfolio loans as the proposed limit). Similarly, there are discontinuities in terms of which counties were designated as either rural or underserved that can be exploited in the same manner.

7 Conclusion

We provide empirical estimates of the effect of a regulation that imposes a fixed cost on firms (creditors) on the volume of the affected transactions (availability of credit). We find no evidence that the introduction of the FRB Escrow Rule affected access to credit, suggesting that there is a sufficient amount of firms in the subprime mortgage market to compensate for the volume lost by the creditors exiting the market. These findings are applicable to the range of policies that impose fixed costs on firms, and suggest that concerns about the effect of these types of policies on transaction volume may not be warranted.

Standard microeconomics and market microstructure logic can account for this, apriori

somewhat surprising (especially taken into account the aforementioned creditor comments and approximately 200 creditors exiting), result. The cost of implementing an escrow system is effectively a fixed cost. Thus, if a creditor stays in the market, either because she already had the system implemented or because she incurred the fixed cost to implement it because of the regulation, there is no direct effect on this creditor's pricing decision. The only potential effect is the indirect one – if some of the competitors exit, then competition becomes softer, increasing the incentive to raise the price. The main message to the regulators here is that there is a difference between access to credit from a particular firm and access to credit overall, and this difference is particularly significant in this setting.

Even for the 246 counties that experienced an exit, the market was close to competitive by examining, for example, the Hirsch-Herfindahl Index.[12] The extent of the price increase depends on how many creditors are left and on how differentiated the remaining creditors are. The markets where we would expect an effect of an overall volume of transactions would be the ones where an exit would somehow result in a systemic shift in the nature of market competition, such as going from a close to perfectly competitive market to four active firms or less. Once microeconomic theory had helped us identify the treatment group, the result was almost a foregone conclusion given the low market concentration even after the implementation of the rule.

We hope that this type of a microeconomic theory based identification will help future researchers solve similar problems. We hope that non-academic researchers use this identification strategy as well. Our application alone suggests that any regulation imposing even

[12] Only 39 out of the 246 counties had an HPML market HHI of over 2,500. A graph of the HHIs for counties that experience and do not experience an exit is in Figure 9.

a sizable fixed cost on mortgage creditors in the United States is unlikely to lower consumer welfare, at least not through the fixed cost shock and any exits associated with that shock.

8 Tables and Figures

Figure 1: Timeline of FRB Escrow Rule and Sample Period

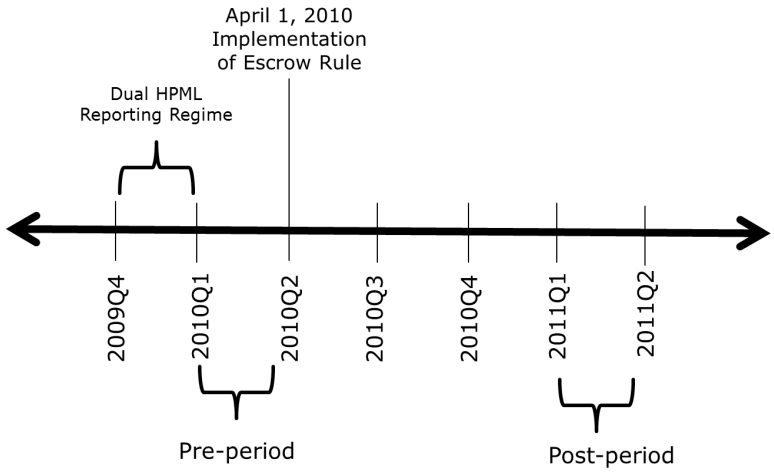

Figure 2: Data Construction Process

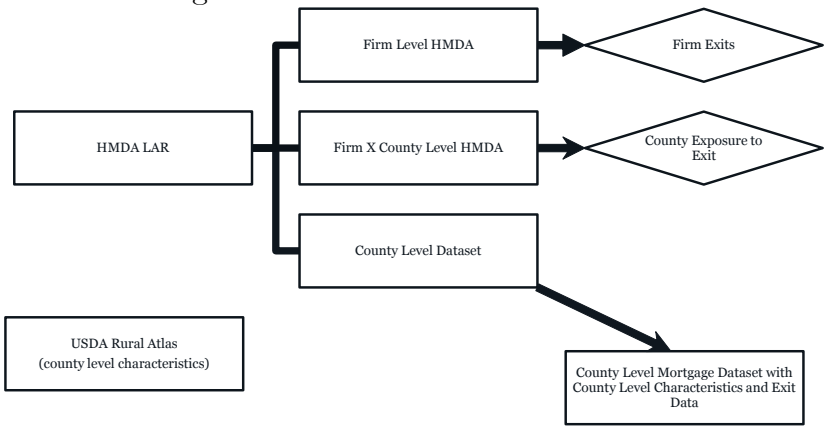

Figure 3: Mean HPML Originations by County Exit Experience

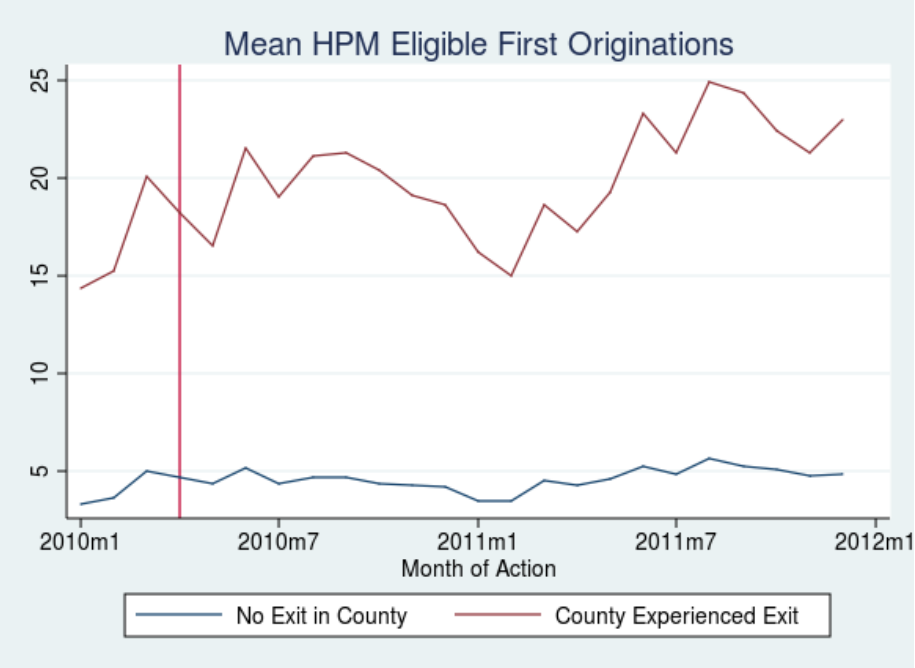

Figure 4: Mean Eligible First Originations by County Exit Experience

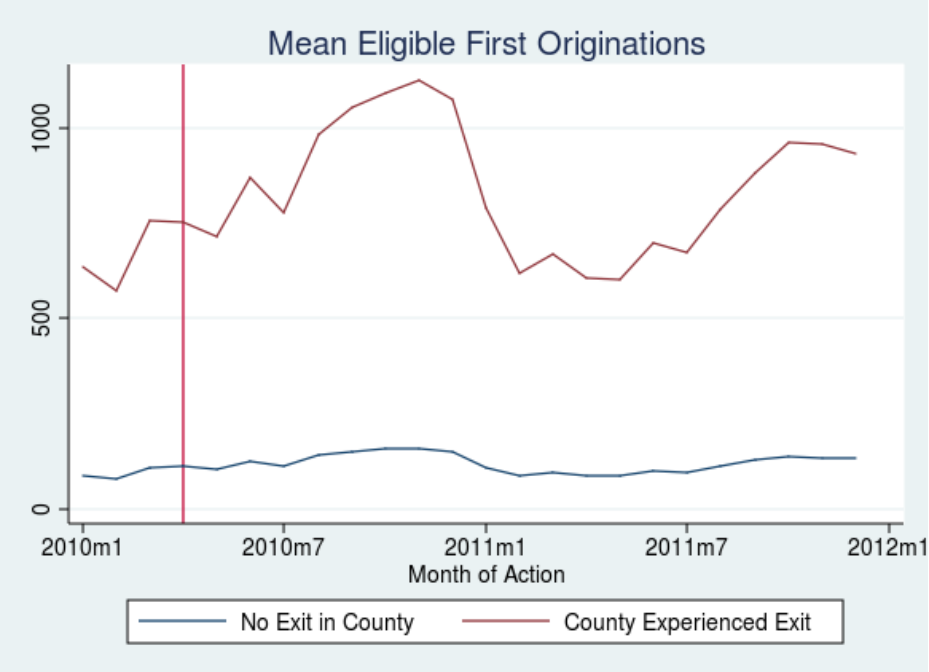

Figure 5: Mortgage Originations by Exiting Firms and Non-Exiting Firms

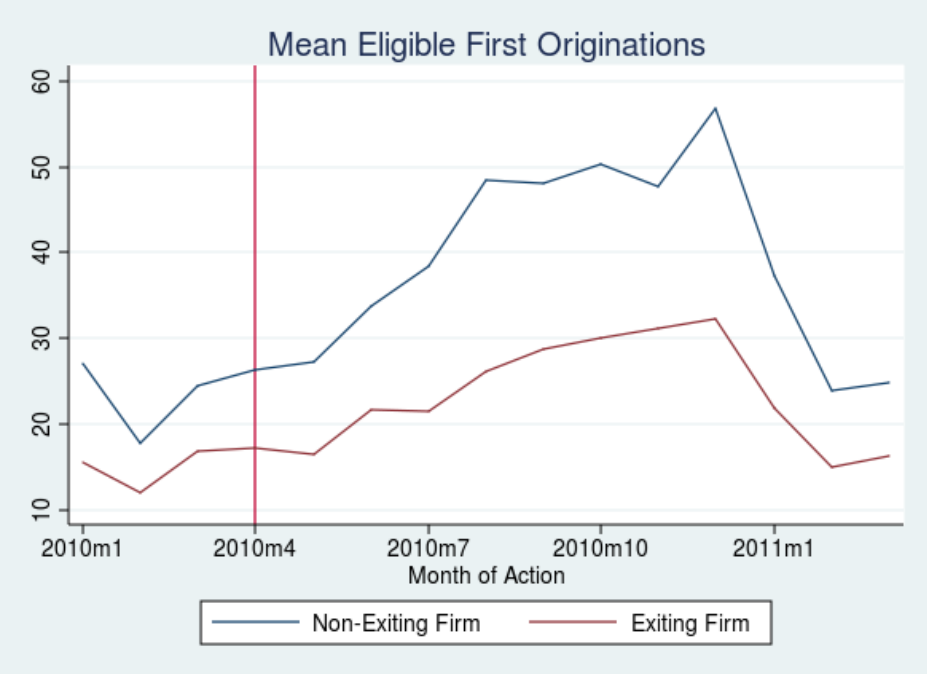

Figure 6: Synthetic Control Estimates of HPML Originations

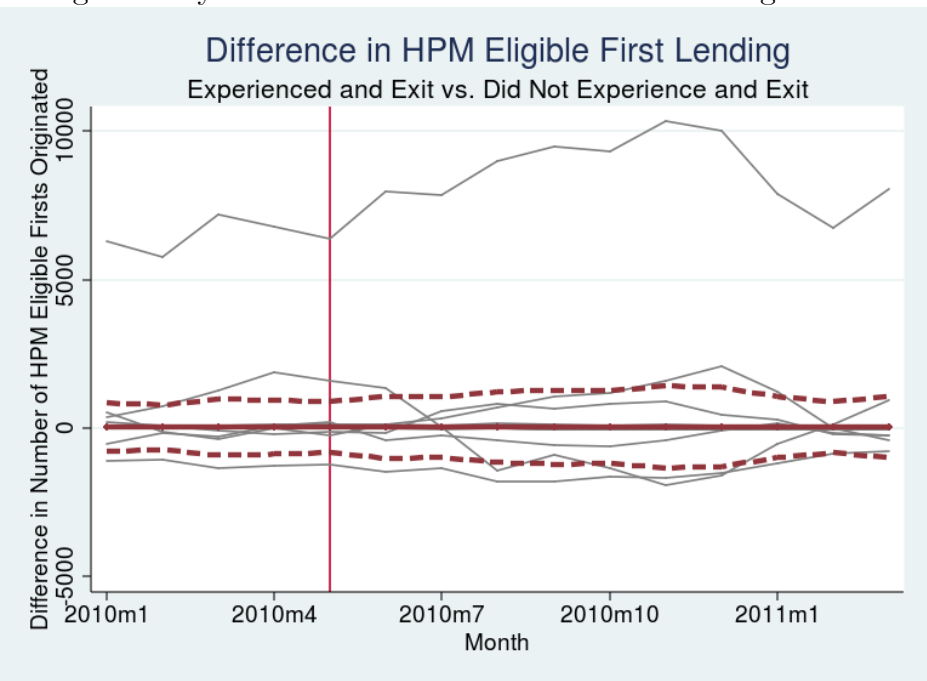

Figure 7: HPML APR Thresholds for 30 Year Fixed Rate Mortgages

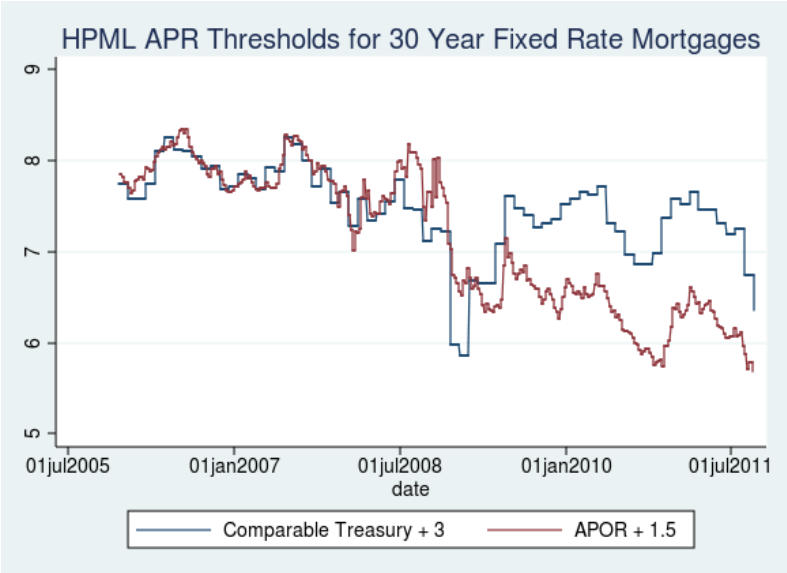

Figure 8: Synthetic Control Estimates of Mortgage Originations

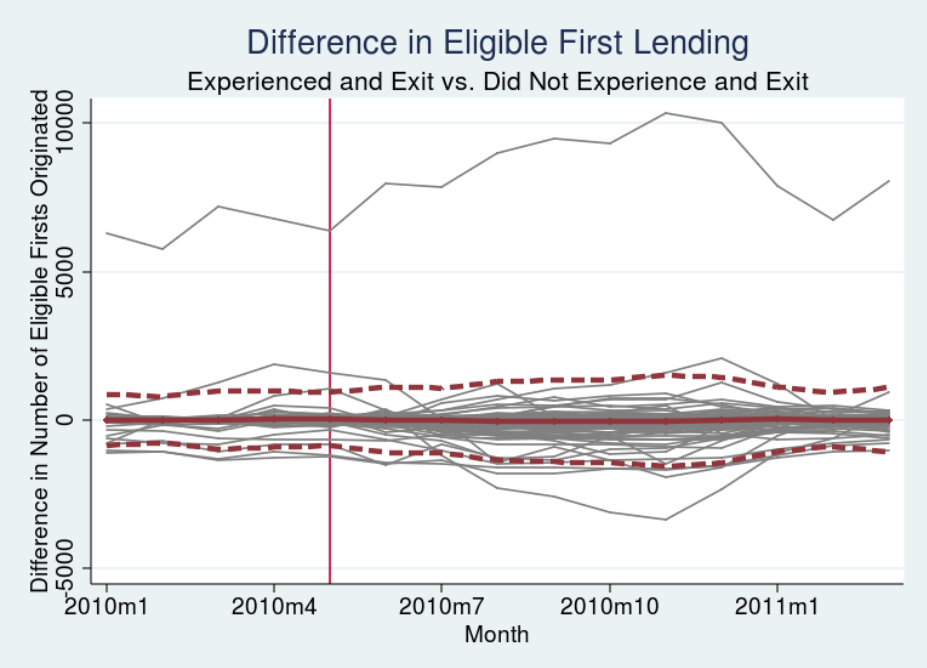

Figure 9: Histogram of HHI in HPML Market by County Exit Status

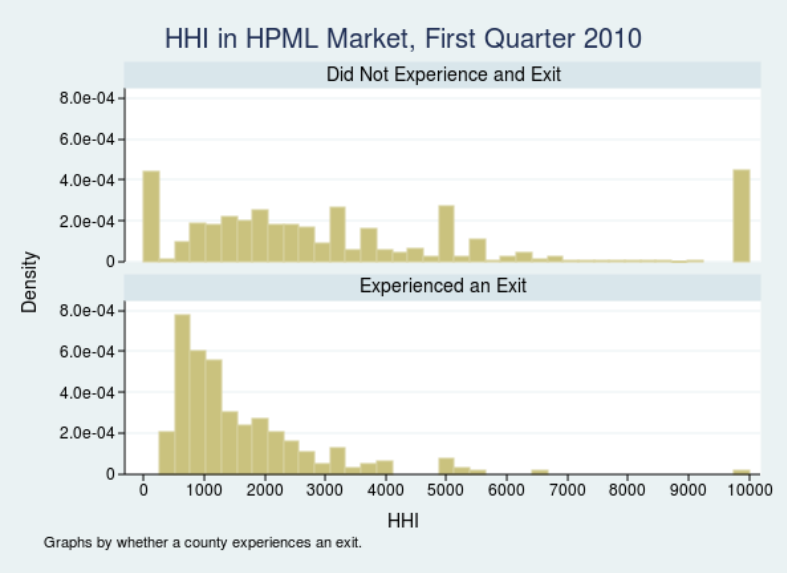

Table 1: County Level Means

Variable	All Counties	Exiting Counties	Non-Exiting Counties
Count of Counties	3,269	246	3,023
Number of HPM Eligible Firsts	4.97	16.53	4.02
Number of Eligible Firsts	134.72	654.42	92.43
Total Population, 2010	191,103	478,772	167,678
Total Number of Households	70,928	175,241	62,433
Average household Size, 2007-2011	2.53	2.59	2.53
Median household income, 2011	43,979	50,084	43,487
Percent of Owner Occupied Housing, 2007-2011	72.99	69.12	73.31
Percent of population 65 or older, 2010	15.80	13.93	15.95
Race, 2010			
White	76.37	67.27	77.11
African-American	8.57	12.02	8.29
Asian	1.15	2.90	1.01
Native American	1.79	0.68	1.88
Hispanic	10.49	15.29	10.10
Multiple Race	2.02	2.44	1.98
Education, Adults 25 and Over			
No High School	16.71	15.55	16.80
High School Only	35.02	30.84	35.36
Some College	28.90	28.12	28.97
College or Higher	19.37	25.49	18.87
Percent of Counties			
Metro Area (UIC Codes 1,2)	35.42%	75.61%	32.15%
Persistent Poverty (2000)	12.25%	6.72%	12.70%
High Outmigration (2008)	22.69%	3.25%	24.30%
Low Levels of Education (2000)	19.74%	10.50%	20.50%
Retirement Destination (2000)	14.04%	18.07%	13.70%

HMDA 2010Q1 and 2011Q1 and USDA Rural Atlas.
Definitions for county-level characteristics available at `http://www.ers.usda.gov/data-products/atlas-of-rural-and-small-town-america/documentation.aspx`
Unweighted means of monthly observations prior to the FRB Escrow Rule unless otherwise indicated.

Table 2: Firm Level Means

Variable	All Firms	Exiting Firms	Non-Exiting Firms
Counts	6,691	202	6,489
Annual Applications	1,950.78	454.76	1,997.35
Pre-Period Origination Counts			
HPM Eligible First	7.49	1.74	7.67
Eligible First	198.18	47.77	202.86
HPM Eligible First Purchase	2.94	0.62	3.01
Purchase Eligible First	69.75	16.84	71.39
Originated 1 HPM in the Pre-Period	12.27%	64.85%	10.63%
Assets in thousands (excludes non-banks)	21,880,627	6,792,503	22,350,314
Regulator (Using 2010 Codes)	207,358	218,429	207051
Office of the Comptroller of the Currency	12.63%	19.31%	12.42%
Federal Reserve Board	7.62%	6.44%	7.66%
Federal Deposit Insurance Corporation	35.79%	37.62%	35.74%
Office of Thrift Supervision	7.26%	5.45%	7.32%
National Credit Union Administration	25.33%	22.77%	25.41%
Department of Housing and Urban Development	11.36%	8.42%	11.45%
Region			
Northeast	16.98%	23.76%	16.77%
Midwest	31.89%	21.29%	32.22%
South	35.84%	40.10%	35.71%
West	14.45%	12.38%	14.52%
Not a US State	0.84%	2.48%	0.79%

HMDA 2010Q1.
Unweighted means of monthly observations prior to the FRB Escrow Rule unless otherwise indicated.

Table 3: Effect of the FRB Escrow Rule on Loan Counts

		(1) OLS	(2) NBRM	(3) ZINB Negative Binomial Equation	(4) ZINB Zero Inflation Equation
Panel A: All Eligible First Loans					
HPM Eligible Firsts		0.273	0.0123	0.0121	1.105
		(0.645)	(0.0370)	(0.0364)	(0.943)
R2/Chi2		0.674	3,584		2,324
HPM Eligible First Purchases		0.271	0.0147	0.0298	0.448
		(0.391)	(0.0501)	(0.0497)	(0.562)
R2/Chi2		0.525	2,575		1,324
N		18,828	18,828		18,828
DoF		27	27		27
Panel B: Portfolio Loans					
HPM Eligible Firsts		-1.663***	-0.0791*	-0.0919**	0.194
		(0.508)	(0.0447)	(0.0452)	(0.444)
R2/Chi2		0.370	2,736		1,518
Eligible Firsts		42.55***	0.0349*	0.0420**	21.17
		(6.968)	(0.0194)	(0.0192)	(13.45)
R2/Chi2		0.817	8,499		7,848
	18,792	18,792		18,792	
	27	27		27	

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 4: Effect of FRB Escrow Rule for Select Borrower Groups

	(1) HPM Eligible Firsts	(2) Eligible Firsts
Income Measures		
Income Below County Median	-0.0296	-0.0180
	(0.0488)	(0.0146)
Observations	18,292	18,292
Chi2	2,942	11,166
Household Income In Lowest 40% Nationally	-0.0360	-0.0188
	(0.0496)	(0.0162)
Observations	18,292	18,292
Chi2	2,377	6,200
Race		
Black	-0.0180	0.0345
	(0.129)	(0.0350)
Observations	18,292	18,292
Chi2	2,648	6,839
Hispanic	0.0225	-0.0120
	(0.0893)	(0.0288)
Observations	18,292	18,292
Chi2	2,141	9,387

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 5: Effect of the FRB Escrow Rule on Metro and Non-Metro Areas

	In a Metro Area		Not in a Metro Area	
	(1)	(2)	(3)	(4)
	HPM	Eligible Firsts	HPM	Eligible Firsts
All Loans	-0.131	0.00321	0.151	0.0397
	(0.106)	(0.00884)	(0.0928)	(0.0353)
N	6,540	6,540	12,288	12,288
Chi-Squared	534.8	2658	1,733	4,213
Portfolio Loans	-0.157***	0.0191	0.164	-0.0334
	(0.0494)	(0.0202)	(0.104)	(0.0521)
N	6,540	6,540	12,252	12,252
Chi-Squared	539.5	2,637	1,652	3,270

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 6: Effect of FRB Escrow Rule on Loan Size and Spread over APOR

	(1)	(2)	(3)	(4)
Loan Size in Thousands (OLS)	-0.937	-1.997*	-0.259	-1.311*
	(1.252)	(1.136)	(1.040)	(0.791)
Observations	3,548,995	3,365,563	3,526,965	3,344,297
R-squared	0.020	0.333	0.195	0.429
Spread over APOR	0.0623	0.0911	0.0573	0.0975*
Restricted to HRM (OLS)	(0.0515)	(0.0556)	(0.0548)	(0.0563)
Observations	144,817	125,887	142,196	123,675
R-squared	0.002	0.101	0.021	0.110
Spread over APOR	-0.0727	-0.0149	-0.134	-0.0945
With Non-HPM Set to Zero (Tobit)	(0.127)	(0.144)	(0.131)	(0.145)
R-Squared	0.00514	0.0685	0.0503	0.0966
	3,548,995	3,365,563	3,526,965	3,344,297
Borrower Level Controls		X		X
County-Level Controls			X	X

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
Borrower-level controls include reported income, whether a co-borrower is present, minority group, loan purpose, and type of lender. County-level controls include race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 7: Effects of the FRB Escrow Rule on Exiting Firm Loan Volumes

	Pre-Policy Change HPML and Eligible First Controls		Log Asset, Regulator, and Pre-Policy Change HPML and Eligible First Controls	
	(1)	(2)	(3)	(4)
	OLS	NBRM	OLS	NBRM
Eligible Firsts	-17.07***	-0.0612	-13.85**	-0.0344
	(4.910)	(0.0573)	(5.469)	(0.0443)
R2/Chi2	86,527	86,527	76,167	76,167
N	0.934	3,558	6,024	6,024
Purchase Eligible Firsts	-3.927***	-0.157**	-2.305**	-0.0385
	(1.070)	(0.0699)	(1.153)	(0.0438)
N	86,527	86,527	76,167	76,167
R2/Chi2	0.892	908.9	0.906	2,580

Standard errors in parentheses. Standard errors are clustered at the firm-level.
HMDA and HMDA panel data.
Sample restricted Jan 2010 to April 2014.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.

Table 8: Alternate Exit Definition: Effect of FRB Escrow Rule on Loan Counts

	(1)	(2)	(3)	(4)
	OLS	NBRM	ZINB Negative Binomial Equation	ZINB Zero Inflation Equation
HPM Eligible Firsts	0.0938	-0.000168	0.00222	1.503**
	(0.583)	(0.0346)	(0.0342)	(0.707)
R2/Chi2	0.675	3,569		2,312
HPM Eligible First Purchases	0.198	0.00191	0.0213	0.527
	(0.352)	(0.0461)	(0.0462)	(0.469)
R2/Chi2	0.526	2,565		1,321
N	18,828	18,828		18,828
DoF	27	27		27

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 9: Effect of the FRB Escrow Rule on Census Tract Level Loan Counts

	(1) OLS	(2) NBRM	(3) ZINB Negative Binomial Equation	(4) ZINB Zero Inflation Equation
HPM Eligible Firsts	-0.339***	-0.760***	-0.578***	20.16
	(0.0142)	(0.0341)	(0.0419)	(0)
R2/Chi2	0.596***	-0.0326***	-0.0383***	2.230
	(0.156)	(0.0110)	(0.0108)	(1.947)
Eligible Firsts	0.596***	-0.0326***	-0.0383***	2.230
	(0.156)	(0.0110)	(0.0108)	(1.947)
R2/Chi2	0.429	121,930		106,589
N	761,043	761,043		761,043
DoF	27	27		27

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with 2008-2012 American Community Survey 5 Year Estimates.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 10: Effect of the FRB Escrow Rule on Loan Counts, Pre-Period Extended to January 1, 2009

	(1) OLS	(2) NBRM	(3) ZINB Negative Binomial Equation	(4) ZINB Zero Inflation Equation
HPM Eligible Firsts	-0.0399	0.0160	0.0362	0.307*
	(0.120)	(0.0237)	(0.0259)	(0.174)
R2/Chi2	0.674	3,580		2,321
HPM Eligible First Purchases	0.0795	0.00857	0.0300	0.0806
	(0.0726)	(0.0333)	(0.0396)	(0.167)
R2/Chi2	0.524	2,584		1,319
N	18,832	18,832		18,832
DoF	27	27		27

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market. Exit defined over January 2009 to December 2010.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 11: Propensity Score Matching Average Treatment on the Treated Effect on Originations

	HPM		Eligible Firsts	
	(1) Nearest Neighbor	(2) Epanechnikov	(3) Nearest Neighbor	(4) Epanechnikov
January 2011	-0.2080	-0.5126	33.4622	-102.4664
	(2.0926)	(2.3100)	(109.7574)	(123.8112)
Feburary 2011	0.5210	0.5000	15.8592	-86.2773
	(1.7797)	(1.9657)	(85.7686)	(97.0348)
March 2011	1.0042	0.8151	29.3298	-67.7773
	(2.2253)	(2.4534)	(93.5711)	(104.8320)

HMDA 2010Q1 and 2011Q1 and USDA Rural Atlas.
Definitions for county-level characteristics available at `http://www.ers.usda.gov/data-products/atlas-of-rural-and-small-town-america/documentation.aspx`
Propensity score matching on county characteristics and HPM and eligible first originations in the first 3 months of 2010.
Point estimates report county-level estimates of the effect of the FRB Escrow Rule on originations of HPM eligible first or eligible first mortgages.

Table 12: Balancing Tests for Propensity Score Matching

	Nearest Neighbor Matching				Epachnikov Kernel			
	treated	Control	t-Statistic	p>|t|	treated	Control	t-Statistic	p>|t|
Total Population, 2010	4.90E+05	4.60E+05	0.36	0.717	4.90E+05	4.40E+05	0.74	0.457
Average Household Size, 2007-2011	2.5743	2.5913	-0.85	0.394	2.5743	2.5972	-1.04	0.297
Median Household Income, 2011	50084	52168	-1.5	0.134	50084	50686	-0.46	0.648
Percent of Owner Occupied Housing, 2007-2011	13.92	13.554	1.19	0.234	13.92	13.354	1.8	0.073
Percent of Population 65 or Older, 2010	68.941	68.777	0.19	0.849	68.941	68.587	0.41	0.685
Race, 2010								
African-American	12.418	12.076	0.26	0.795	12.418	11.849	0.44	0.66
Asian	2.9913	3.7372	-1.7	0.089	2.9913	3.1653	-0.45	0.655
Native American	0.7034	0.6399	0.29	0.769	0.70334	0.5900	0.56	0.576
Hispanic	12.464	12.886	-0.3	0.766	12.464	13.602	-0.76	0.446
Multiple Race	2.4107	2.5502	-1.19	0.235	2.4107	2.5287	-1	0.316
Education, Adults 25 and Over								
No High School	14.948	14.809	0.25	0.806	14.948	15.031	-0.14	0.886
Some College	28.387	28.313	0.17	0.869	28.387	28.635	-0.56	0.578
College	25.725	26.575	-0.79	0.433	25.725	25.741	-0.02	0.988
HPM Eligible First Originations								
January 2010	14.693	14.252	0.23	0.82	14.693	14.263	0.22	0.825
February 2010	15.618	14.462	0.6	0.552	15.618	14.824	0.4	0.687
March 2010	20.517	18.479	0.82	0.41	20.517	19.218	0.52	0.602
Eligible First Originations								
January 2010	656.14	726.8	-0.63	0.529	656.14	627.98	0.28	0.782
February 2010	589.73	643.95	-0.55	0.586	589.73	564.96	0.27	0.786
March 2010	779.32	857.55	-0.6	0.547	779.32	754.04	0.21	0.831
Urban Influence Code								
In large metro area of 1+ million residents	0.37395	0.39496	-0.47	0.638	0.37395	0.37605	-0.05	0.962
In small metro area of less than 1 million residents	0.37815	0.34874	0.67	0.506	0.37815	0.37815	0	1
Micropolitan area adjacent to large metro area	0.03361	0.05462	-1.12	0.265	0.03361	0.04202	-0.48	0.632
Noncore adjacent to large metro area	0.01681	0.02101	-0.34	0.737	0.01681	0.02101	-0.34	0.737

Micropolitan area adjacent to small metro area	0.08824	0.05882	1.23	0.22	0.08824	0.07143	0.68	0.5
Noncore adjacent to small metro area and contains a town of at least 2,500 residents	0.06303	0.07983	-0.71	0.478	0.06303	0.06723	-0.19	0.853
Noncore adjacent to small metro area and does not contain a town of at least 2,500 residents	0.0042	0	1	0.318	0.0042	0	1	0.318
Micropolitan area not adjacent to a metro area	0.0084	0.0042	0.58	0.563	0.0084	0.0042	0.58	0.563
Noncore adjacent to micro area and contains a town of at least 2,500 residents	0.0084	0.02101	-1.14	0.254	0.0084	0.01681	-0.82	0.412
Noncore adjacent to micro area and does not contain a town of at least 2,500 residents	0.0042	0	1	0.318	0.0042	0	1	0.318
Noncore not adjacent to metro or micro area and contains a town of at least 2,500 residents	0.01261	0.01261	0	1	0.01261	0.01471	-0.2	0.844
Noncore not adjacent to metro or micro area and does not contain a town of at least 2,500 residents	0.0084	0.0042	0.58	0.563	0.0084	0.0084	0	1

Common support includes 78 treated counties and 3060 untreated counties.

Table 13: Prince George's County, Maryland Synthetic Control Group

FIPS	County Name	State	CBSA	Unit Weight
6029	Kern County	CA	Bakersfield, CA	0.102
12095	Orange County	FL	Orlando, FL	0.312
13121	Coweta County	GA	Atlanta-Sandy Springs-Marietta, GA	0.127
24031	Montgomery County	MD	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.209

Prince George's County, Maryland (FIPS 24033) experiences an exit from the HPM market. Based on a donor pool of 49 counties assigned by k-median clustering on first quarter 2010 HPM and eligible first originations, race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 14: Prince George's County, Maryland Synthetic Control Group Predictor Balance

	Treated	Control	Percent Deviation
HPM Eligible First Originations			
January 2010	22	21.998	0.01%
February 2010	20	20.020	0.10%
March 2010	22	20.191	-0.96%
Eligible First Originations			
January 2010	1258	1236.786	1.69%
February 2010	975	998.703	-2.43%
March 2010	1437	1419.371	1.23%

Prince George's County, Maryland (FIPS 24033) experiences an exit from the HPM market.
Based on a donor pool of 49 counties assigned by k-median clustering on first quarter 2010 HPM and eligible first originations, race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 15: Alternate Quarter 2 Implementation Date Counterfactual Experiment on Loan Counts

	(1) OLS	(2) NBRM	(3) ZINB Negative Binomial Equation	(4) ZINB Zero Inflation Equation
HPM Eligible Firsts	-0.350	-0.0721	-0.0724*	-0.249
	(1.013)	(0.0473)	(0.0404)	(0.583)
R2/Chi2	0.689	3,357		2,449
Eligible Firsts	-66.43**	-0.0516	-0.0242	6.967***
	(26.60)	(0.0377)	(0.0293)	(1.945)
R2/Chi2	0.859	13,744		12,917
HPM Eligible First Purchases	1.318*	-0.0225	-0.0335	-0.230
	(0.718)	(0.0667)	(0.0648)	(0.398)
R2/Chi2	0.577	2,854		1,901
Eligible First Purchases	22.57***	-0.00738	-0.0192	-0.768
	(7.219)	(0.0236)	(0.0245)	(0.860)
R2/Chi2	0.907	13,754		13,505
N	18,822	18,822		18,822
DoF	27	27		27

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q2 and 2011Q2.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible firstmarket but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

Table 16: Effect of the FRB Escrow Rule on Loan Counts, With Pennsylvania Specific Effects

	(1) OLS	(2) NBRM	(3) ZINB Negative Binomial Equation	(4) ZINB Zero Inflation Equation
HPM Eligible Firsts	0.539	0.0346	0.0359	1.131
	(0.671)	(0.0376)	(0.0372)	(0.961)
PA HPM Eligible Firsts	-5.276***	-0.377***	-0.369***	
	(1.507)	(0.0934)	(0.0919)	
R2/Chi2	0.676	3,806		2,560
Eligible Firsts	31.91***	-0.00225	0.00199	19.34***
	(8.982)	(0.0119)	(0.0113)	(1.079)
PA Eligible Firsts	45.78	0.0493*	0.0499*	
	(36.80)	(0.0268)	(0.0265)	
R2/Chi2	0.899	12,958		12,989
N	18,828	18,828		18,828
DoF	27	27		27

* p<0.1, ** p<0.05, ***p<0.01.
Standard errors in parentheses. Standard errors are clustered at the county-level.
HMDA merged with USDA Rural Atlas.
Sample restricted to 2010Q1 and 2011Q1.
Exit defined as experiencing a pre-period HPM origination from a firm that exited the HPM eligible first market but that did not exit the eligible first market.
All models include controls for race, education, urban influence codes, median household income, average household size, total population, proportion of population age 65 and older, and proportion of owner occupied households.

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