Contractual Lock-In:

Mortgage Prepayment Penalties and Mobility*

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October 31, 2023

Abstract

Do prepayment penalties lock-in borrowers, reducing mobility? Using monthly mortgage performance and borrower panel data, I study mobility before and after the expiration date of prepayment penalties. I find that borrowers increase mobility once the penalty expires, but show limited anticipatory mobility behavior as the expiration date approaches: while a penalty expiration leads to a 38% higher moving rate post-expiration relative to baseline moving rates in my sample, borrowers pre-expiration move in parallel. These expiration induced moves are to places with higher economic opportunities: the effect is just as strong for small and long distance moves and the moves are disproportionately to zipcodes with high levels of average income and high levels of upward income mobility. I then study what can explain these effects on mobility. I find that housing equity at the time the penalty expires is an important input into the mobility response of borrowers: very low and underwater LTV borrowers responses are muted, while most of the response comes from high, but < 100 LTV borrowers. I interpret this finding to be consistent with credit constraints stemming from the housing market being an important financial friction to household mobility. These results imply that mortgage contract features can interact with credit market imperfections that result in large frictions to moves that would otherwise improve economic circumstances, even when borrowers are above water on their mortgage.

*I thank Amir Sufi (chair), Michael Dinerstein, Scott Nelson, Pascal Noel, and Constantine Yannelis for their invaluable guidance and support. I am grateful to Doug Diamond, Zhiyu Fu, Peter Ganong, Benedict Guttman-Kenney, Zhiguo He, Niels Gormsen, Young Soo Jang, Ralph Koijen, Lucy Msall, Matt Notowidigdo, Simon Oh, Lubos Pastor, Luigi Zingales and participants of the Chicago Booth Student Finance PhD Brownbag, the Chicago Public Finance and Labor Lunch, the Chicago Student Applied Micro Lunch, and the Sufi PhD Working Group for their helpful comments and discussions. This research was funded in part by the John and Serena Liew Fellowship Fund at the Fama-Miller Center for Research in Finance, University of Chicago Booth School of Business. All errors are my own.

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1 Introduction

Do prepayment penalties lock people into their mortgages, reducing their mobility? Prepayment penalties are contractual fees that must be paid if a mortgage is paid off before a specified date as a way to compensate lenders for forgone interest. However, given that moving to another location typically involves selling ones home, and that, for most loans, selling ones home in the U.S. triggers a mortgage prepayment, this financial contract feature can have the real side effect of locking people into their homes and reducing mobility. Home Mortgage Disclosure Act mortgage records show that, from 2018 to 2022, on average 4-5% of all mortgage originations in the U.S. had these penalties in their contract, meaning hundreds of thousands of borrowers face being locked into their mortgages by these penalties.

Despite this potential cost, there is no empirical evidence on how borrower mobility is affected by prepayment penalties. Given the design of these penalties in practice, the size of this cost is not obvious. They are large fees, so presumably more people move after they expire. However, they are also temporary; therefore, their overall effect on mobility will not only depend on the response to their expiration, but any anticipatory response prior to expiration.

In this paper, I estimate the lock-in effect of prepayment penalties by using panel data of millions of mortgages originated and securitized during the mid 2000s housing boom. I exploit contract level variation in the length of time these penalties were in effect and a difference-in-differences framework to estimate the effect prepayment penalties have on mobility around the time they expire from a contract.

To obtain the data necessary to study mortgages with prepayment penalties during this time, I use mortgage level records from Moody's Analytics. This dataset covers the near universe of mortgages that were securitized in the private-label market, a segment of the mortgage market at this time that was disproportionately likely to have prepayment penalties. The data is collected by the loan servicers in this market and provides detailed contract terms at the time of mortgage origination such as the balance, interest rate, loan-to-value ratio, credit score and debt-to-income, as well as whether the loan had a fixed or variable rate, whether it was interest only or had a balloon payment, and most importantly, the existence and term length of any prepayment penalty. These mortgage records also come with a panel dataset that follows the performance of these mortgages until the mortgage is terminated, whether by voluntary prepayment or default.

To be able to study the effects prepayment penalties have on mobility, these mortgage records are in turn linked to Equifax credit bureau records. For all the borrowers who appear in the Moody's dataset, I see their credit profile from six months prior to the loan being originated to six months after the loan is terminated. Equifax uses the credit information they receive from lenders to keep the primary address of any borrower up to date on their credit file. Researchers are able to see the zipcode of this primary address, which I use to construct my measure of mobility: I define a move to be the first month Equifax no longer reports the zipcode of the mortgaged property as the primary address of the borrower.

With the mortgage and credit bureau data linked, I am able to study monthly moving rates for borrowers with different prepayment penalty lengths using a difference-in-differences specification. I exploit the staggered timing of these prepayment penalty expirations to estimate the differences in moving rates of borrowers before and after their prepayment penalty expires, relative to a control grouop of borrowers who still face the penalty at that time. My rich dataset allows me to identify the effect prepayment penalties have on mobility while non-parametrically controlling for several possible confounders, such as unobserved heterogeneity from borrowers who originate their mortgages in the same month, go to the same lender, or have similar borrower and loan characteristics at origination.

My first result identifies the effect prepayment penalties have on mobility. Post-expiration there is an immediate increase in mobility: the monthly moving rate increases by 0.10 percentage points within 2 months of the penalty expiring, peaking at near 0.20 percentage points 8 months after expiration, before slightly decreasing. On average, in the year after a prepayment penalty expires the monthly move rate is up by 0.14 percentage points. This treatment effect is economically meaningful: when facing prepayment penalties, the baseline monthly moving rate is 0.38 percentage points, implying a treatment effect of 38% .

For my estimate of the effect of prepayment penalties on mobility to be causal, two assumptions must hold: a no anticipation assumption and a parallel trends assumption. Specifically, borrowers cannot adjust their moving behavior prior to prepayment penalties expiring and, absent the prepayment penalty expiring, the moving behavior of the treated and not-yet-treated borrowers would otherwise move in parallel. I visually show suggestive evidence that both assumptions in my empirical setting are valid by plotting an event study of monthly moving rates in the year before prepayment penalties expire relative to those who still face the penalty. I find no evidencing of existing parallel trends.

While perhaps surprising, the lack of anticipation is consistent with prepayment penalties being shrouded attributes in this market (Gabaix and Laibson (2006)). It is also consistent with the structure of subprime mortgage contracts where banks, now having a financial incentive to step in and induce you to refinance after sufficient mortgage payments, provide an information shock that it is now cheaper to terminate your mortgage (Gorton (2008)).

One specific endogeneity concern is some borrowers sort into prepayment penalty term lengths based on future expected mobility, which would place an upward bias on my results. To address this concern, I complement my main difference-in-differences specification with two instrumental variable approaches that exploit features of the securitization process in the private label market and state border discontinuities in the regulation of prepayment penalty term length, respectively. The variation in prepayment penalty term length here is unlikely to be borrower driven, but rather by mortgage investor preferences for security design and shifts in regulations across state lines. I find similar results with either approach, confirming this endogenous sorting does not pollute my estimates.

With the effect of prepayment penalties on mobility established, I then study the economic consequences of these moves. While, by revealed preference, the increase in mobility itself is a sign borrowers are making themselves better off, it is also important to understand the extent to which these moves are to areas where economic activity is higher and, conversely, whether mortgage contract features such as prepayment penalties can lower standards of living by preventing moves.

To study the economic consequences of these moves, I focus on two measures: the distance of the moves and the economic characteristics of the locations moved from and moved to. The distance of moves is relevant in that longer distance moves are more likely to be job-related while short distance moves may be for non-job related reasons. Therefore, the extent to which prepayment penalties reduce long distance versus short distance mobility is informative on whether prepayment penalties only reduce mobility for moves of certain purposes. Studying the economic characteristics of the locations borrowers begin and end at is relevant to the economic consequences of these moves in that it directly measures whether borrowers are moving to areas with higher levels of economic activity. To the extent that their are placed-based effects on an individual, living in a better economic area can translate to better individual outcomes.

First, I redo my main specification to see how much short and long distance moves are being prevented by prepayment penalties. I do see by redefining my main outcome variable on mobility from whether one's zipcode of their primary address changes, to whether the first n digits of ones zipcode of their primary address changes. The first 5 digits is, naturally, my original mobility variable, while mobility defined by the first digit of ones zipcode changing is closer to changing Census divisions in the U.S., indicating a very long distance move.

In support of the moves having economic consequences, the prepayment penalties reduce mobility in all 5 different distance based measures of mobility. All are economically meaningful: the mobility responses relative to pre-period baseline moving rates are between 38-41%.

Second, I look at how prepayment penalties reduce mobility based on zipcode-level wages earned and zipcode-level upward economic mobility, both in the zipcodes borrowers are orignally located in and the zipcodes borrower move to. I do so by sorting zipcodes by these measures, separating them into quartiles, and redoing my original event studies within these quartiles.

With respect to where borrowers are moving from, treatment effects are the same regardless of zip-code level economic activity: prepayment penalties are just as much of a friction in low economic activity areas as high economic activity areas. This result provides additional support that prepayment penalties are a financial friction to moving whose results are not being driven by the economic conditions of ones zipcode.

In contrast, treatment effects vary according to where borrowers are moving to. Overwhelming, these moves are to locations where zipcode levels of average wages and of upward economic mobility are highest. This difference is not being driven by borrowers who are at or near the margin of being in one quartile of economic activity or the other: even those in the lowest quartile of economic activity disproportionately move to the highest quartile of economic activity when prepayment penalties expire.

My final results turn to studying the mechanism behind these mobility responses. I study two main mechanisms: credit constraints and financial sophistication. Cross-sectional variation in these different sources of heterogeneity in treatment effects implies, while both may play a role in driving treatment effects or are otherwise important in the market, credit constraints drive most of the cross-sectional variation.

I proxy for credit constraints by studying the heterogeneity in mobility responses by housing equity, or loan-to-value ratios (LTV), in the month prior a borrower is deciding to move. Consistent with credit constraints such as the down payment constraints of the housing model in Stein (1995), I find an inverse-U shape in the mobility response to prepayment penalty expiration and LTV ratios. Borrowers with very low LTV ratios (< 60%) do not move more after a prepayment penalty expires: these borrowers are largely unconstrained by the prepayment penalties when deciding to move, given they have so much housing wealth, and thus the penalty does not enter into their decision making.

In contrast, borrowers with LTV ratios between 60 and 100% is where the mobility response to prepayment penalties is mostly concentrated: I find a peak response of 0.20 percentage points in the LTV range of 80-100%. These borrowers are precisely those with low enough levels of housing equity where a prepayment penalty may be binding in their housing choices, and thus we would expect treatment effects to be concentrated in this region of equity if credit constraints played a role in mobility.

Finally, I find that deeply underwater borrowers–just like those with very high levels of positive equity–do not respond to the prepayment penalties expiring. This is again consistent with credit constraints playing a role in moving: these borrowers are so constrained that the prepayment penalty is no longer marginal. Much like the literature that studies how negative equity reduces mobility, my results highlight the importance of housing equity as a source of collateral when moving, rather than simply being a wealth effect.

I then study this heterogeneity result by regional economic activity to see the extent housing equity can explain the economic consequences of these moves. I find that indeed the credit constraints story holds in both zipcodes of low and high economic activity, suggestive evidence that credit constraints not only interact with other financial frictions in a contract to prevent mobility, but prevent moves that improve the economic circumstances of borrowers.

To supplement the heterogeneity by LTV results, I also study borrower debt repayment and consumption as proxied by automobile purchases after a move. Given I see their Equifax credit records six months after the loan is terminated, I study in a short window any differences in revolving credit, such as balances from home equity lines of credit or credit card debt, and automobile purchases before and after a move. I find that moves are associated with economically meaningful pay downs of revolving debt as well as financing of automobile purchases, consistent with evidence that home sales and moves have stimulative effects (Best and Kleven (2018); Berger et al. (2020)).

In contrast to the importance of housing equity in driving heterogeneity in mobility responses to prepayment penalties, when looking at heterogeneity by credit score, credit card utilization, city-level educational attainment, and city-level fraction of individuals above the age of 54, there is limited evidence of heterogeneity in any of these variables. Given these variables have been used as cross-sectional proxies of financial sophistication in prior literature (e.g., Lusardi (2012); Agarwal et al. (2023)), I don't find much evidence that there is much *cross-sectional* evidence of financial sophistication driving my results.

1.1 Related Literature

This paper is the first to empirically document the lock-in effect prepayment penalties have on borrowers and its economic consequences. As far as I know, the mobility consequences of prepayment penalties have never been measured.

Mortgage Lock-In This paper contributes to the growing literature studying how frictions stemming from mortgages and the mortgage market can affect mobility. Previous work has focused on how the amount of housing equity, in particular having negative equity, decreases mobility (Genesove and Mayer (1997, 2001); Chan (2001); Engelhardt (2003); Andersson and Mayock (2014); Foote (2016); Brown and Matsa (2020); Gopalan et al. (2021); Bernstein and Struyven (2022)). Another strand of this literature has studied how the differences between ones contractual mortgage rate and the ongoing market mortgage rate affects mobility (Quigley (1987); Fonseca and Liu (2023)). Ferreira et al. (2010) looks at both of these frictions, as well as the lock-in effects of property taxes. My paper studies a different mortgage related friction that can also potentially reduce mobility.

Mobility This paper also contributes to the economics literature on mobility (see Jia et al. (2023) for a review). (Deryugina et al. (2018); Groen et al. (2020); Nakamura et al. (2022)) are three papers that study migration induced by natural disasters to study the effects such migration has on employment and earnings. Keys et al. (2022) uses a movers analysis and credit bureau records to study the financial consequences of mobility and finds that moves lead to a reduction in financial distress. My paper also studies the labor and credit market implications of these moves, but with a different source of variation: changes to the cost of moving induced by features of ones mortgage.

In other work, Bergman et al. (2023) finds that alleviating credit constraints for a population of renters only has a limited impact on mobility, while alleviating informational constraints about the process of moving had a relatively large impact on mobility. My paper provides evidence that, at least for homeowners, mortgage frictions can interact with credit constraints enough to impact mobility.

Prepayment Penalties There is also a literature in real estate and finance that studies prepayment penalties. The theoretical literature typically studies the welfare consequences and optimality of prepayment penalties in a mortgage contract. The first paper to do so is Dunn and Spatt (1985), where they study the risk-sharing benefits of these penalties; in later work, Chari and Jagannathan (1989) expands this model to account for adverse selection on the borrower side. A more recent model is that of Mayer et al. (2013) that shows prepayment penalties can provide efficiency gains in a dynamic model of refinancing with costly default. In all these models, a known cost is the prevention of ex-post beneficial housing turnover and mobility; my paper contributes to this work empirically by estimating this effect.

There is also an empirical literature that studies prepayment penalties, mostly focused on prepayment and default behavior as well as their effect on credit access. Beltratti et al. (2017) finds that a 2007 mortgage system reform in Italy that banned prepayment penalties led to less prepayments and worse mortgage pricing, mostly through the contracts chosen after the ban. I contribute to this literature by being the first to study how prepayment penalties impact mobility. Rose (2012) finds that loans with prepayment penalties lead to higher default, but only in the non-bank mortgage market. Rose (2013) finds geographic variation in prepayment penalties correlates with lower prepayment rates in that area. My paper is the first to show the effect prepayment penalties have on mobility.

Transaction Costs This paper also contributions to the literature that studies how transaction costs related to housing can impact mobility and housing markets more generally. Most of the work in this literature uses variation in transaction taxes (Van Ommeren and Van Leuvensteijn (2005); Cunningham and Engelhardt (2008); Ferreira (2010); Dachis et al. (2012); Besley et al. (2014); Slemrod et al. (2017); Best and Kleven (2018)). All these papers are focused on transaction costs stemming from the housing market, while I contribute to this literature by studying a transaction cost related to the mortgage market. I also contribute to this literature by showing directly the potentially amplifying effect leverage can have on the mobility response of transaction fees, as theorized in Best and Kleven (2018).

The rest of the paper is outlined as follows. Section 2 provides background on the time period and segment of the mortgage market studied in this paper. Section 3 details the data used. Section 4 explains the empirical specifications used to estimate the effect prepayment penalties have on mobility and section 5 provides the estimates. Section 6 goes over what mechanisms could be driving these effects. Section 7 concludes.

2 Background

I study the effect prepayment penalties have on mobility by looking at mortgages originated and securitized in the 2000s private label securitization (PLS) market. This section provides additional information on what the PLS market was like and how salient prepayment penalties were in this market.

2.1 2000s Private Label Securitization Market

To understand why the U.S. mortgage market of the 2000s is a useful setting for studying the effect of prepayment penalties on mobility, it's important to understand the institutional details of the PLS market.

Mortgage securitization in the U.S. largely works through the government. Specifically, since the early 1980s, mortgage securitization would operate through government sponsored enterprises (GSEs), such as Fannie Mae or Freddie Mac, that would purchase loans from lenders and then proceed to pool and tranche thousands of loans together into a mortgage-backed security, selling them to investors.

Given the government presence, there were many regulations on what mortgages could be securitized by government agencies. The first common standard is that of credit quality: the government typically only securitized higher credit score borrowers, with a securitization cut-off of a 620 FICO credit score being a common rule of thumb (Keys et al. (2010)). Another rule is that of the size of the loan: the agencies have a loan balance limit where they will not securitize mortgages above that limit.[1]

Due to the government regulations limiting mortgage securitization to high credit borrowers with smaller mortgage balances, there was growing interest in the 1990s for the private sector to securitize mortgages from either low credit score borrowers, larger mortgages, or both (McConnell and Buser (2011)). While slowly growing through this decade and into the early 2000s, the PLS market takes off in late 2003, with the share of privately securitized mortgages going from 16% of total mortgage originations in 2002 to 46% in 2006 (Mian and Sufi (2022)).

Many of the regulations that applied to mortgages sold to the government agency-led securitization market did not apply to the private label market. The main rule for the purpose of this paper is that, while prepayment penalties were virtually banned from the government agency securitization market, they were fully allowed in mortgages that were sold in the private label market.

The prevalence of prepayment penalties in this market can be seen in Figure A1. While even in the early 2000s prepayment penalties were common in this market, averaging just over 30% from 2000 to 2002, the percent of PLS mortgages with a prepayment penalty increases to over 50% from 2005 to 2006. The figure also provides suggestive evidence of lender-driven variation in the prevalence of these prepayment penalties: the surge in prepayment penalties begins in August 2003, the exact month noted in Justiniano et al. (2022) and Mian and Sufi (2022) where a credit supply expansion from the PLS market begins.

The combination of the growing presence of prepayment penalties in the PLS market along with its surge in the mid-2000s results in millions of mortgages originated with such penalties.

[1] Loan limits vary by location and year; latest limits can be found on the FHFA website here.

Given my dataset covers the near universe of PLS mortgages, providing detailed contract terms at origination and monthly performance data after origination, the market at this time allows for an ideal setting to study the effect prepayment penalties have on mobility in a precise, high frequency manner.

2.2 How Aware were Borrowers of Prepayment Penalties?

Given I use variation in prepayment penalty expiration dates to identify their effect on mobility, I now will discuss suggestive evidence that these contract terms were not well understood by borrowers at this time. At the peak of the 2000s housing boom, there was much blame placed on the non-standard attributes of the subprime mortgages originated during this time leading to the many defaults we saw once the housing crash occurred. The extent to which these loan attributes casually led to defaults has been discussed and debated extensively in the literature (e.g., Gerardi et al. (2008); Palmer (2023)). Regardless, the subprime crisis led to U.S. policy makers and legislators making many changes to how mortgages were originated, one of many coming from how prepayment penalties were seen as abusive and predatory.[2]

One of the initiatives of the federal government in response to this backlash of the non-standard attributes in subprime mortgages was to investigate the extent to which these attributes were easily disclosed to borrowers. Figure A2 shows the results from a randomized-control trial that was run by the Federal Trade Commission to test how well borrowers understood particular mortgage features via the disclosure form one is provided when taking out a mortgage. While many features were not well-understood by borrowers, prepayment penalties were particularly confusing for borrowers: 68% of respondents could not say whether they had a prepayment penalty in their contract, while 95% of respondents were did not know what the penalty amount would be if charged. This is not particularly surprising when looking at the typical disclosure forms used at this time; Figure A3 shows the only lines that discuss ones prepayment penalty in such forms. Nowhere does it say definitively if one's mortgage has a prepayment penalty, only two boxes that state that either one doesn't have a penalty or that they "might" have one.

Section 3 provides evidence using my sample that is consistent with prepayment penalties

[2] Here's an article from the *New York Times* that explains this line of argument.

not being salient to borrowers. While there are systematic differences between borrowers of different penalty terms, the selection across prepayment penalty terms is not monotonic and not very large, suggesting any selection is modest. As well, much of the variation is driven by one's lender or the MBS deal one's mortgage is pooled into, as well as other mortgage contract terms that have also been documented to not be well understood by borrowers, again suggestive that the prepayment penalty length in a mortgage is lender, and not borrower, driven. This is consistent with research that has shown borrowers don't understand non-standard mortgage features well (Bucks and Pence (2008)), do not shop effectively for mortgage products (Woodward and Hall (2012); Bhutta et al. (2021)), and do not anticipate bankruptcy flag removals (Gross et al. (2020)). As well, there is previous work empirically documenting the extent of shrouded attributes in the credit market (Agarwal et al. (2017); Ru and Schoar (2023)). Given the prevalence of such shrouded attributes in credit markets and the institutional details of prepayment penalties laid out above, it is plausible that prepayment penalties themselves were also likely to be initially shrouded in the closing documents of a mortgage loan. This is also consistent with earlier research that shows anti-predatory laws at the local and state level, at worst, did not seem to hurt subprime borrowers (Bostic et al. (2008)).

3 Data

This sections details the data used in this project, including the loan-level data from mortgage servicing records, the borrower-level from credit bureau records, and regional-level data such as zip-code level median incomes from the IRS Statistics of Income division and CBSA-level educational attainment and age composition from Census records.

3.1 Mortgage Data

To study mortgages with prepayment penalties, I use mortgage servicing records of mortgages that were originated and securitized in the private label securitization market from June 2005 to December 2007.

The mortgage records come from Moody's Analytics.[3] It covers more than 90 percent of

[3]Readers may be more familiar with the name of the company Blackbox Logic, the company who originally

the universe of mortgages that were privately securitized, a segment of the market disproportionately likely to have prepayment penalties, making it useful for this analysis. It contains information about the contract at origination, such as the interest rate, whether it is a fixed rate or adjustable rate mortgage, the balance of the loan, the FICO credit score of the borrower, the LTV and combined LTV (CLTV) at origniation, and the existence of a prepayment penalty and for how many months it is in effect. It also provides monthly performance data for the loan, including the payment status: whether the loan is current on payments, is delinquent, or is being voluntarily prepaid.

I limit my sample to be first liens mortgages on properties of single-family residence and where the borrower reports will be owner-occupied. One concern is the private label securitization market had a non-trivial amount of speculators during this time period. Indeed, (Haughwout et al. (2011)) shows that up to 50% of the originated mortgages reported to be for owner-occupancy were likely to be for speculation in 2006, in the middle of my sample. I will discuss how I handle this in more detail below when discussing the credit bureau data I use.

3.2 Credit Bureau Data

In order to study how prepayment penalties affect mobility, these mortgage records have been linked to Equifax credit bureau records, one of the three major bureaus in the U.S.

Being a credit bureau, Equifax is a particularly useful and credible data source to study mobility. As detailed in (DeWaard et al. (2019)), Equifax receives consumer reports from lenders on their borrowers every month, with many of these reports containing up to date informaiton on a borrower's address. Equifax then uses this information and runs it through their own proprietary algorithm to decide whether a primary address should be updated. Several papers in recent years have used credit bureau data to study mobility (e.g., Molloy and Shan (2013); Gopalan et al. (2021); Fonseca and Liu (2023)).

I define a move to be the first month where the zipcode of the mortgaged property provided in the mortgage records no longer appears in Equifax. This accounts for spurious "moves"

collected and organized this dataset. Moody's bought the company in 2014 and has collected and maintained the mortgage data since.

where the Equifax provided zipcode changes but returns to the original address. This can occur in the short windows when borrowers are moving into and out of the mortgaged property, since it may take time for borrowers to update all of their information across various credit accounts.

3.2.1 Using Credit Bureau Data to Account for Speculators

As mentioned in the previous subsection, this time period was known to have a non-trivial amount of speculators in the mortgage market. Given I am studying how prepayment penalties lock-in borrowers to their homes, it is important to study a sample of borrowers who reside in the mortgaged property.

To ensure my sample does not have speculators, I remove any borrowers who Equifax reports as ever having 3 or more first-lien mortgages. This is because there can only be one first liens per property, and hence having more than two implies having a property that is not primarily for owner occupancy (Haughwout et al. (2011)).[4] Related to this issue of potential misreporting, I also limit my sample of borrowers to those whose zipcode of the mortgaged property matches the zipcode provided by Equifax.

Finally, the fact that I measure mobility using Equifax's primary residence variable, which is verified by Equifax itself, is in itself a safe-guard against picking up speculative activity, given any sale for speculation purposes not involving one's main home won't result in a change of primary address, regardless of their stated intention in the mortgage records.

3.3 Other Data Sources

In addition to individual mortgage and borrower data, I use two main regional economic datasets, both at the zipcode level.

I use zip-level average wages and adjusted gross income from the Internal Revenue Service's Statistics of Income Division. I also use zip-level average measures of upward income mobility from Opportunity Atlas, as discussed in Chetty et al. (2022a,b). Both of these are used to measure the regional economic activity of where borrowers are moving from and where they are moving to, in order to study the extent to which these moves have economic consequences.

[4] I allow two first-liens to allow for borrowers who started a mortgage at a new property while terminating the mortgage of an old property.

I also use core based-statistical area (CBSA) characteristics from the U.S. Census to proxy for regional levels of financial sophistication, in particular the share in a CBSA with at least a college degree and the share in a CBSA that are at least 55 years old.

3.4 Summary Statistics

Table 1 provides summary statistics on the borrowers' Equifax predicted income, credit score, and mortgage balance, combined loan-value ratio, and interest rate at origination. Consistent with this sample largely representing the subprime mortgage market, these borrowers have low income and credit scores and face high LTV ratios and interest rates.

Table 2 shows summary statistics of monthly mortgage performance when borrowers are under these penalties. Again, consistent with this being the subprime mortgage market, we see many instances of mortgage prepayment and default. As well, we see that the monthly moving rate is 38 basis points a month.

The final two rows provide statistics on debt repayment behavior and automobile purchase behavior for these borrowers when under a prepayment penalty. We see that 2-3% of borrower-months are associated with a car purchase, and that the average borrower is taking on debt rather quickly, with 6-12% growth in revolving balances every 3 months. I will refer back to these variables when studying the role credit constraints play in the mobility responses to prepayment penalties.

When focusing on differences across prepayment penalty lengths, it is clear that there are systematic differences across observable borrower characteristics. This motivates the difference-in-differences specification I use to study the effect prepayment penalties have on mobility, comparing changes in mobility across borrowers (different term lengths) and across time (before and after expiration). As robustness, in some specifications I also flexibly control for many borrower observables with fixed effects specifications.

While there are systematic differences across these observable characteristics, these variables do not do a great job of explaining the variation in prepayment penalty term length we se in the data. To see this, Table 3 presents adjusted R^2's of the following regressions:

$$PrepaymentPenaltyLength_b = \alpha_{x(b)} + \epsilon_b$$

where $\alpha_{x(b)}$ is a set of fixed effects for various variables I observe at origination. Borrower and loan characteristics such as income, credit score, loan balance, interest rate, and LTV ratio do not explain much of the variation we see in prepayment penalty length: the best fit is the fixed effects for interest rates, where the adjusted R^2 is still only 0.05. In contrast, fixed effects for variables such as which lender or loan servicer you got your loan from and the MBS pool your mortgage is placed into strongly predict prepayment penalty length. The MBS pool fixed effects specification does particularly well, alone being able to explain a quarter of the variation in penalty length.

4 Empirical Specification

To study how prepayment penalty expiration affects mobility, I exploit contract-level variation in expiration date of these penalties, where the expiration date is the function of the age of the loan. Using a difference-in-differences framework, I estimate monthly moving rates for borrowers over the life of the loan, comparing those whose penalty expires relatively early in the life of the loan versus borrowers who penalty expires relatively later.

As highlighted by several recent papers, staggered difference-in-difference specifications can be biased if treatment effects are heterogeneous and dynamic (Cengiz et al. (2019); Goodman-Bacon (2021); Sun and Abraham (2021); Baker et al. (2022); Borusyak et al. (2023)). Given my setting is vulnerable to this bias, I estimate all effects using the local projection difference-in-differences specification (LP-DiD) introduced in Dube et al. (2023), which corrects for this bias.[5] The event-study version is

[5] Figure B1 shows that this new approach to difference-in-differences gives similar results to Callaway and Sant'Anna (2021) (aka, CS). There is a level shift due to LP-DiD being a variance weighted estimator while CS is equally weighted. To be conservative and consistent with prior literature that uses regression to estimate difference-in-differences, and hence will also be variance-weighted, I report variance-weighted estimates from LP-DiD.

$$
\begin{aligned}
Move_{b,t+k} - Move_{b,t+k-1} = &\beta_k \Delta PenaltyExpired_{b,t} \\
&+ \alpha^k_{l(b),t} \\
&+ \alpha^k_{c(b),t} \\
&+ \varepsilon^k_{b,t} \\
&\text{for } k \in \{0, ..., K\}
\end{aligned} \tag{1}
$$

The variable $PenaltyExpiration_{b,t}$ is a dummy variable equal to 1 if the borrower's prepayment penalty expired at some date $j \leq t$. Therefore,

$$
\Delta PenaltyExpiration_{b,t} = PenaltyExpiration_{b,t} - PenaltyExpiration_{b,t-1}
$$

is a dummy variable equal to one in the period a borrower's prepayment penalty expires in period t, zero otherwise. All specifications include lender-time ($\alpha^k_{l(b),t}$) and cohort-time ($\alpha^k_{c(b),t}$) fixed effects, where I define a cohort of borrowers by the month their loan was originated.

This specification is particularly useful in that it easily allows for "clean controls" by only estimating each β_k for the sample of borrowers that are either newly treated in period t ($\Delta PenaltyExpiration_{b,t} = 1$), not yet treated in post-periods $k \geq 0$ ($PenaltyExpiration_{b,t+k} = 0$), and not treated in any pre-period $k < -1$ ($PenaltyExpiration_{b,t} = 0$). Given in my sample every borrower is eventually treated, my specification is a not-yet-treated difference-in-differences specification: β_k estimates the difference in mobility at period $t + k$ for those borrowers whose penalty has expired or will expire in period t relative to borrowers whose penalty has not yet or will not yet expire until after period $t + k$.

In order to have a single causal estimate to report from the event study, I also report tables that show the pooled estimates of the difference-in-differences specification above in the year after and before the penalty expires

$$
\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + X_b \times \alpha_t + \varepsilon_{b,t}. \tag{2}
$$

As a source of robustness, in these tables I will also allow for pre-treatment controls X_b to interact with time fixed effects in order to flexibly control for any observable characteristics that could be confounding treatment effects. Unless otherwise specified, I control for these borrower characteristics flexibly using fixed effects.

4.1 Assumptions Needed for Causal Effect

For the difference-in-differences design to estimate a causal effect, two main assumptions must hold. First, there must be no anticipation: the treatment group cannot react to the penalty expiring before it expires. Second, parallel trends must hold: if it was not for the penalty expiring, the mobility rates of the treatment and control group would have trended in parallel. The no anticipation assumption is likely to hold in my setting because, as discussed in Section 2, these penalties were poorly understood by borrowers. While the parallel trends assumption is untestable, I visually show suggestive evidence that it holds by plotting the event study coefficients β_k of Equation (1) and finding no evidence of pre-trends.

4.2 Instrumented Difference-in-Differences Approach

Despite the lack of pre-trends being suggestive of the parallel trends assumption being valid, it does not rule out a particular strategic method of sorting: borrowers who know with precision that they have a moving opportunity in month $N + 1$ choosing a prepayment penalty of term length N.[6] In this case, with actual moving rates naturally correlating with expected mobility rates, the jump in moving could be driven not by the penalty expiring *per se* but due to borrowers who expect to move around that time choosing particular prepayment penalties. In that scenario, even a lack of pre-trends is consistent with a parallel trends violation.

To account for any unobservable characteristics that correlate with expected mobility and prepayment penalty term length, such as the example laid out above, I also take an instrumental variables approach that addresses this concern, exploiting the aforementioned variation in term lengths predicted by the MBS pool a mortgage ends up in. Specifically, I estimate the

[6]Naturally, this particular sorting can be made more general in that those who choose term length N do so because of higher expected moving opportunities in any month $M > N$. In any case it is clear that any differential expectation of moving opportunities can violate parallel trends.

following instrumented difference-in-differences specification:

$$\begin{aligned}
Move_{b,t+k} - Move_{b,t+k-1} =& \beta_k \Delta \widehat{PenaltyExpired}_{b,t} \\
&+ \alpha^k_{l(b)} \\
&+ \alpha^k_{c(b),t} \\
&+ \varepsilon^k_{b,t} \\
\Delta PenaltyExpired_{b,t} =& \pi_k \Delta MBSPredPenaltyExpired_{d(b),t} \\
&+ \delta^k_{l(b)} \\
&+ \delta^k_{c(b),t} \\
&+ \nu^k_{b,t} \\
&\text{for } k \in \{0, \ldots, K\}
\end{aligned} \tag{3}$$

where $MBSPredPenaltyExpiredn_{d(b),t}$ is the expiration of the term length most common in the *previous* month of the MBS pool one's mortgage is placed into. This isolates the variation in term lengths that is coming from investor preferences for a certain term length, where pools are priced based on the exposure to prepayment risk.

As in the main difference-in-differences specifications, I also report tables of pooled estimates of the form:

$$\begin{aligned}
\overline{Move}_{b,post} - \overline{Move}_{b,pre} =& \beta \Delta \widehat{PenaltyExpired}_{b,t} + \alpha_{l(b)} + \alpha_{c(b),t} + X_b \times \alpha_t + \varepsilon_{b,t} \\
\Delta PenaltyExpired_{b,t} =& \pi \Delta MBSPredPenaltyExpired_{d(b),t} + \delta_{l(b)} + \delta_{c(b),t} + X_b \times \delta_t + \nu_{b,t}
\end{aligned} \tag{4}$$

where once again I allow for flexibly controlling for pre-treatment borrower covariates. Unless otherwise specified, I control for these covariates using fixed effects of the values these covariates take. This is also an additonal benefit of using Dube et al. (2023) to solve the issues with two-way fixed effects estimation: given it uses regression, it easily can be ported to settings that require instrumental variables.

4.2.1 Validity of Instrument

Combining an IV approach with a DiD approach means both set of assumptions are necessary for a causal effect to be estimated. Therefore, alongside parallel trends and no anticipation, the instrument must satisfy relevance and the exclusion restriction: that is, the instrument correlates with $PenExpiration_{b,t}$ and any effect the instrument has on mobility for a borrower b works solely through the effect of their prepayment penalty expiring. The table results in section 5 will show that relevance is satisfied. For validity, while another untestable assumption, Table 4 re-does the R^2 exercise from before and finds that the instrument does not predict any borrower characteristics. As expected, the MBS pool ID strongly predicts the variation in the instrument, which is mechanical and precisely the variation I wish to isolate. It is also predicted more by my other credit supply proxies, namely the lender or servicer of your mortgage.

In all, the identification strategy of this instrumental variables approach relies on the lender and/or MBS investor driven variation in prepayment penalty length to not be correlated with borrower expected migration patterns. A threat to identification here would be that to the extent that lenders choose the penalty length based on expected prepayment risk horizons and that correlates with expected mobility risk horizons for borrowers, there would still be an unobserved correlation between expected mobility and penalty length choice, violating the exclusion restriction.

Again, while untestable, Table 5 provides additional evidence that the exclusion restriction holds. The table reports coefficients from the following regression

$$\frac{1}{N_{(d,c)}} \sum_{s \in (d,c)} \frac{MigrationOutflows}{NonMigrationPopulation} = \alpha + \rho NYearPenaltyShare_{d,c-1} + \varepsilon_{d,c} \tag{5}$$

where $\frac{1}{N_{(d,c)}} \sum_{s \in d} \frac{MigrationOutflows}{NonMigrationPopulation}$ is an MBS deal by month of origination (d,c) weighted average of the state migration outflows where the loans belonging to deal d were originated, $NYearPenaltyShare_{d,c-1}$ is the share of loans in deal d and originated in month $c-1$ with a N year prepayment penalty. The state migration flow data is from 2004 to 2005, so would have been in the information set of lenders and MBS investors during the securitization process of these loans.

The idea behind this test is that if there is an underlying correlation between the composition of penalty lengths in an MBS pool and expected mobility rates of the borrowers in the MBS pool, this would be able to predict cross-sectional patterns in mobility out of the regions where these loans were originated. The results in Table 5 show this is not the case: the penalty share of each MBS pool in my dataset has virtually no predictability of average state migration patterns in the MBS pools. The 2-year penalty share is the most statistically significant, but with a correlation coefficient of 0.03 and can reject correlations as low as 0.05.

Table 6 shows similar results by comparing the share of loans in an MBS deal with short penalty lengths (1 or 2 year prepayment penalties) versus long penalty lengths (3 or 5 year prepayment penalties) and changing the definition of the migration measure. Whether one looks only at out-flows, gross flows in and out of a state, or net flows out of a state, the results are consistent with each other: the correlation between the pool's penalty composition and loan-weighted migration flows is minimally statistically significant and not economically meaningful, sauggestive that the instrument is valid.

5 Results

This section presents the results of the effect prepayment penalties have on mobility and its implications for labor markets.

5.1 Mobility Responses around a Prepayment Penalty Expiration

This subsection presents the results of the effect prepayment penalties have on mobility.

5.1.1 Difference-in-Differences Results

Figure 2 presents the results of the event study estimation, a year before and a year after the prepayment penalty expires. The results show that move rates moved in parallel between borrowers of different prepayment penalty lengths and only diverge once the prepayment penalty has expired. The post period coefficients are also quite stable, staying around 0.15 percentage points even 9 months out.

Table 7 shows the pooled difference-in-differences result from equation 2. Column (1) shows the most basic difference-in-differences specification, with only month fixed effects included in the regression. Even in this specification with minimal controls, prepayment penalty expirations increase monthly moving rates by 0.12 percentage points, a 32% increase relative to the baseline control mean of 0.38 percentage points. Columns (2) and (3) separately add cohort (month of origination) by month and lender by month fixed effects, controlling for unobservable heterogeneity across borrower cohorts that enter the mortgage market as well as unobservable heterogeneity across lenders during the 2000s housing cycle, to ensure my results are not being driven by the changing landscape of the mortgage market at this time. We see that the results are largely unchanged, with coefficients now of 0.14 and 0.13 percentage points, respectively. Column (4), which corresponds to the specifications shown in all event studies in this paper, includes both of the aforementioned fixed effects and still finds a treatment effect of 0.14 percentage points, 38% of the baseline monthly move rate for borrowers who face a prepayment penalty. Columns (5), (6), and (7) show results when including borrower characteristics–FICO credit score and loan-value ratio at origination–by month fixed effects, zipcode by month fixed effects, and a specification with all fixed effects in previous columns. Again, we find that coefficients are very similar regardless of specification.

Given that these are penalties for prepayment, it is important that the treatment response is being driven by people moving after prepaying their mortgage. Figure 3 splits the mobility measure into three categories: moves where a prepayment has occurred in a 6-month window, a default has occurred in a 6-month window, or neither–meaning the mortgage is still current. As expected, the results mostly stem from prepayments, although a small fraction of the mobility response also comes from defaults.

5.1.2 Instrumented Difference-in-Differences Results

Figure 4 shows the results from the instrumented difference-in-differences design. The results are consistent with our original specification: there are no visual pre-trends and a jump in mobility right after the penalties expire, and sustaining this jump for nearly a year after expiration.

Table 8 compares the OLS and IV results. In Column (1), we see the original treatment effect that 0.14 percentage points more borrowers move after expiration relative to borrowers still facing the prepayment penalty, as reported in Column (4) of table 7. Columns (2) and (3) provide the estimates from the first stage and reduced form of the IV. From column (2) we see that relevance is satisfied: the predicted penalty expiration from the MBS pool ones lender was securitizing in the month prior to a borrowers origination predicts a 37% increase in the likelihood ones own prepayment penalty expires at the same date.[7] The reduced form estimate shows that the predicted penalty expiration increases mobility by 0.08 percentage points. Column (4) shows the 2SLS estimate: borrowers whose penalty has expired have annual move rates that are 0.22 percentage points higher than those who still face the penalty.

The IV estimate is larger than the OLS estimate, which at first may seem surprising. The main threat to my original specification was that borrowers sort into these prepayment penalties based on future moving rates, an endogeneity concern which should put downward bias on the OLS estimate. However, given the previous discussions on the lack of understanding of these features that was pervasive at this time, coupled with the lack of anticipatory behavior in all of the event study specifications, this seems broadly consistent with this specific sorting concern not being operative in my sample. In fact, the OLS estimates being smaller are consistent with a world where prepayment penalties are either shrouded or borrowers display rational inattention to their contracts and hence do not respond optimally or with full information to these penalties, hence reducing the treatment effect.

5.1.3 New York State Border Discontinuity

To bolster the credibility of the previous results, I complement my difference-in-differences specification with a state border discontinuity design.

New York state banking laws have banned prepayment penalties longer than a year for decades. Figure C1 provides the piece of legislation in the NY state banking code where this appears. Importantly, this law is binding and followed in the state of New York. Figure C2 shows the fraction of my sample that have a one year prepayment penalty, by zip-code. Across the entire U.S., the fraction of one year prepayment penalties is around 10%, in line with the

[7] See Appendix E to see more information on F-statistics, showing the instrument is indeed a strong one, even by the standard set in (Lee et al. (2022)).

observation breakdown reported in Table 1. New York, however, is a clear expection, with the great majority of zip-codes have $> 75\%$ shares of one year prepayment penalties.[8]

Exploiting the discontinuous change in ability to place long-term prepayment penalties on contracts around the NYC border, I use this discontinuity as an instrument, comparing mobility rates for mortgages originated in New York relative to the control group of mortgages originated near New York. Table C1 shows the summary statistics: while borrowers look similar across observable characteristics, the differences in share of mortgages with a 1-year prepayment penalty is large: nearby states have a percentage of 7%, while in New York it is 77%.

Figure C3 shows the dynamic difference-in-differences, where variation in prepayment penalty length composition is induced by the New York state border. The results are consistent with the previous research designs: there are no visual pre-trends in the year prior to the loan reaching one year after origination, with an increase in mobility in the year after a mortgage is originated in New York relative to neighboring states.

Table C2 shows the pooled results: the borrowers who originated a mortgage in New York move at a monthly rate that is 0.04 percentage points higher than those borrowers in nearby states. With a first stage of a 75 percentage point increase in one year prepayment penalties across state borders, this translates to a treatment effect of 0.06 points, again showing that, through two different IV approaches, the IV estimate is if anything larger than the OLS estimate. Relative to the control's average monthly move rate of 0.18 percentage points in the first year after a loan is originated, the treatment effect is 33% of the baseline moving rate, similar to the results in the previous sections.

5.2 Labor Market Implications of These Moves

This subsection presents the labor market implications these prepayment penalty induced moves have for these borrowers using two geographic measures: seeing if the lock-in of prepayment penalties varies by distance and measuring the local economic conditions of where the borrowers move to.

[8]There does not need to be full compliance with the law. Given this is a state law, some lenders who are not governed by state banking legislation may forgo it.

5.2.1 How Far Do Borrowers Move?

While I do not see the earnings or employment status of borrowers, I am able to see the zipcode of their new destination. Insofar as longer distance moves are more likely to be job-related, I can study the labor market implications of these moves by seeing how strong the mobility response is by the distance of the move.

Figure 5 shows the main event study as in Figure 2 but for 5 different definitions of a change in primary residence: whenever the first n digits of your zipcode changes after a move.

The panels make it clear the effect prepayment penalties have on mobility is robust to the definition of move by distance. Even in the strictest definition of a move, where only the first digit of your zipcode has changed after a move, we see no visual pretrends leading up to a prepayment penalty expiration and an immediate and persistent jump in monthly moving rates in the immediate months succeeding expiration.

Naturally, the treatment effect decreases in absolute size as the definition of a move increases in distance. Mechanically, these moves are rarer. To account for this, Figure A5 shows the same results but with the coefficients rescaled by the average monthly moving rate when everyone faces a prepayment penalty. Here, we see treatment effects relative to baseline that are nearly identical: for any definition of mobility, prepayment penalties reduce monthly moving rates by around 40%.

Table 9 presents the pooled estimates as I change the distance of a move. Again, we see a statistically significant effect across all definitions of a move. The table results also confirm the event study results of economic significance: when comparing to the baseline monthly move rate as reported at the bottom of the tables, we see that the distortion is just as large regardless of distance. The treatment effect relative to baseline is in the narrow window of 38-41%.

Why would prepayment penalties reduce mobility for both short and long distance moves? Given longer distance moves are more likely to improve the economic circumstances of borrowers, one would perhaps expect the effect of prepayment penalties to dissipate as the distance of a move increases.

However, as first discussed in (Bernstein and Struyven (2022)), this is consistent with a balance sheet channel of mobility where high upfront pecuniary costs can prevent moves that, while a borrower may find worthwhile to do when calculating the net present value of the benefits of the move, borrowers cannot borrow around due to credit constraints. In their paper, they build a toy model based on Stein (1995) that incorporates how mortgage frictions can affect mobility based on the distance of the move. With this in mind, in section 6, I provide suggestive evidence that it is borrowers who are most likely credit constrained that drive my main results. To my knowledge, this is the first paper that studies the direct effect mortgage frictions have on moves separated by the distance of the move in a U.S. setting.

5.2.2 Where Do Borrowers Move to and from?

Another advantage of seeing the destination zipcode of borrowers after they move when a prepayment penalty expires is that I can compare the economic circumstances of their new zipcode relative to their old one.

To do so, I split each zipcode into quartiles based on two cross-sectional measures of economic activity: the average wage as reported by the IRS and the average "economic connectedness" of the individuals in that zipcode, first created and discussed in (Chetty et al. (2022a,b)) using Facebook data. Economic connectedness is defined in their work as

> *Baseline definition of economic connectedness: two times the share of high-SES (socio-economic status) friends among low-SES individuals, averaged over all low-SES individuals in the ZIP code.*[9]

In Chetty et al. (2022b), they show this regional measure to be one of the strongest predictors of regional upward income mobility, which is one of the largest economic benefits found in the moving to opportunity literature (Chetty et al. (2016)).

With these two regional measures of economic activity, I am capturing two important measures of improved economic circumstances for movers: higher average levels of wages for current working adults, and higher average levels of labor market earnings for their future offspring. The extent that prepayment penalties disproportionately reduce mobility to regions

[9]For how the authors measure socioeconomic status, from (Chetty et al. (2022a), page 109): *Social scientists have measured SES using many different variables, ranging from income and wealth to educational attainment, occupation, family background, neighbourhood and consumption. To capture these varied definitions, we compute the SES for each individual in our analysis sample by combining several measures of SES, such as average incomes in the individual's neighbourhood and self-reported educational attainment (see the 'Privacy and ethics' section of the Methods for a discussion of how user privacy was protected during this project). We combine these measures of SES into a single SES index using a machine-learning algorithm.* See their paper for more details on the construction.

with high levels of either of these variables will dictate the economic significance of their constraint to moving.

I begin by showing whether the effect of prepayment penalties on mobility matters by where you begin. Figure 6 shows the main event study split into the four quartiles of wages. All four panels paint the same picture: prepayment penalties are equally a friction to mobility for borrowers in low and high wage areas. There is no anticipatory behavior in the year leading up to expiration, and there is a large and persistent jump in mobility in the year after. While some panels have results that are noisier than others, the average treatment effect is around 0.13-0.16 percentage points for each of the four quartiles.

This is shown precisely in Table 10. Each column corresponds to the quartile the zipcode falls in by average level zip-wage. We see that, when the results are pooled, the results are again shown to be in the range of 0.13-0.16 percentage points, and given the standard errors, are mostly statistically indistinguishable from each other.

In contrast, Figure 7 shows the results according to where the *destination* zipcode falls in the wage distribution, as shown in quartiles. The moves that were being prevented were predominantly to high wage areas. While in all four quartiles there is a noticeable jump in mobility after a prepayment penalty expires and hence moves to regions of all levels of economic activity were prevented, we see that move rates to zipcodes in the lowest quartile of wages never increase by more than 0.02 percentage points, move rates to zipcodes in the highest quartile see increases upwards of 0.12 percentage points, a treatment effect six times larger.

The pooled estimates can be found in Table 11. The first column shows the main pooled mobility response as reported in Table 7 column 4. Columns 2-5 show the portion of the overall treatment effect that is to each quartile. Just as in the event study, the moves are predominantly to the highest quartile: 0.09 percentage points are moves to Q4 while only 0.01 percentage points are moves to Q1, a 9 fold difference. With 0.03 percentage points being moves to Q3, moves to the upper half of the zip-wage distribution accounts for 86% of the mobility response after a prepayment penalty expires.

Similar results can be seen for economic connectedness, showing labor market implications of these moves are robust to different definitions of regional labor market activity. Figure 8 repeats the exercise in Figure 6 but for quartiles based on economic connectedness. We see that visually there are no pre-trends leading up to prepayment penalties expiring while, once prepayment penalties expire, we see a quick and persistent jump in monthly moving rates. Consistent with the results by wages, the coefficients in the event study are similar across quartiles, with all event studies peaking at around 0.20 percentage points 6-9 months after expiration.

This is again shown precisely in Table 12. Each column corresponds to the quartile the zipcode falls in by average level economic connectedness. We see that, when the results are pooled, the results are again shown to be in the range of 0.13-0.17 percentage points, remarkably similar to the treatment effects when sorted by average wages, despite the fact that these measures of economic activity, while being relevant for standards of living, do not capture the same information.[10] This can be seen comparing the observations in each quartile by wages and economic connectedness seen in Tables 10 and 12 where while most borrowers are in the upper quartiles of the zip-wage distribution, most borrowers are in the lower quartiles of the zip-economic connectedness distribution.

Figure 9 shows the event studies by where the destination zipcode falls in the quartiles of economic connectedness. We see, just as in Figure 7, most of the moves are to zipcodes at the highest quartiles of economic connectedness.

Similar to the exercise for zip-level wages, the pooled estimates can be found in Table 13. The first column shows the main pooled mobility response as reported in Table 7 column 4. Columns 2-5 show the portion of the overall treatment effect that is to each quartile. Just as in the event study, the largest portion of moves are to zipcodes in the highest quartile of economic connectedness. The difference between the lowest and highest quartile is smaller when looking at quartiles of zip-level wages, but is still meaningfully larger: moves to Q4 are nearly double that of moves to Q1. With 0.03 percentage points being moves to Q3, moves to the upper half of the zip-economic connectedness distribution accounts for 57% of the mobility response after a prepayment penalty expires.

While the fact that prepayment penalties are constraints to mobility equally across the distribution of local economic activity in borrowers' original locations, and they on average mostly

[10] They are, naturally, very correlated, but not perfectly: as can be seen in Table A2, wages and economic connectedness have a correlation of 0.53. While large, there is still significant variation in each variable unexplained by the other ($R^2 = 0.27$).

move to areas of high economic activity when no longer constrained is evidence that prepayment penalties likely constrain moves that improve the economic circumstances of borrowers, it could also be that due to the composition of borrowers in each bin, it may be that the results in Figures 6 and 7 are driven by most borrowers residing in high wage zipcodes and staying in their part of the distribution when moving.[11]

To account for this, and to see if people by quartile use moves as an opportunity to improve their economic circumstances when the prepayment penalty expires, the following tables split the destination zipcode results of Tables 10 and 12 by the quartile of *origin* zip-code.

Tables 14 and 15 display the results. Each is split into four panels, corresponding to the 4 quartiles of average wage and average economic connectedness in the zipcode of origin, respectively. As in Tables 11 and 13, the Column (1) shows the overall treatment effect while Columns (2)-(5) show the monthly move rate to quartiles 1-4 of the destination zipcode for the respective zipcode variables of economic activity.

These tables confirm that, regardless whether a borrower's zipcode of origin is of low or high economic activity, the moves after a prepayment penalty expires are disportionately to high economic activity areas. For those in the lowest quartile of wages and economic connectedness, 80% and 47% of moves are to zipcodes of a higher quartile. In fact, for every type of move except those in the lowest quartile of economic connectedness, the largest fraction of moves is to the highest quartile of the relevant measure of economic activity.

6 Mechanisms

With the effect prepayment penalties have on mobility established, I now explore the role housing equity, credit score and spending behavior, and regional characteristics of financial sophistication play in driving these effects.

[11]Note, this story doesn't necessarily apply to the economic connectedness results, where most borrowers reside in zipcodes with low levels of economic connectedness.

6.1 Housing Equity

This subsection details the role housing equity plays in explaining the effect prepayment penalties have on mobility by showing how treatment effects vary with LTV ratios and studying the spending and debt repayment behavior of movers.

6.1.1 Mobility Response by Loan-Value Ratios

This subsection details the role housing equity play in explaining the effect prepayment penalties have on mobility by showing how treatment effects vary with LTV ratios.

Why would the mobility response to prepayment penalties depend on housing equity? Prepayment penalties are large (4-5% of home value), especially as a function of any future down payments (typically 20% of home value) one may need to make in the purchase of their next home. Given down-payments consist of a large, upfront payment that is necessarily cash based, ones ability to make this down-payment can impact the decision to move (Stein (1995); Ortalo-Magne and Rady (2006)).

Figure 10 shows the results in an event study. I allow treatment effects to vary by LTV buckets flexibly by estimating the event study for various LTV buckets. As can be seen from the figure, those with very low LTV ratios have a smaller jump in mobility once the penalty expires relative to those with high LTVs, especially those near being underwater. The effect is parabolic: treatment effects grow from very high levels of positive equity to low levels of positive equity, reaches a maximum around 80% LTV, and then decreases until there is essentially no response to prepayment penalties once a borrower is very underwater. This is consistent with another source of mortgage lock-in–negative equity–being more operative in this portion of the LTV distribution (Gopalan et al. (2021); Bernstein and Struyven (2022)).

Figure 11 shows the pooled difference-in-differences result by LTV ratio, showing the inverse-U shape more clearly. While households with very high positive equity and deeply underwater borrowers show mobility responses that are statistically insignificantly different from zero, those with LTV ratios of 80-100% see the largest mobility responses: when a prepayment penalty expires, borrowers in this LTV bucket increase their move rates by 15 basis points.

While bigger LTV ratios are associated with larger loan balances which are associated with larger prepayment penalties, these results are not mechanical: Figure A8 redoes the exercise but with the coefficients scaled by the average prepayment penalty amount, and the result is the same: responses are zero at the ends of the LTV distribution, while peaking in response around 80-100% LTV.[12]

6.1.2 Mobility Response by Loan-Value Ratios and Local Labor Market Characteristics

The previous sections have separately shown the importance prepayment penalties reducing mobility have on the labor markets borrowers end up in and the role housing equity plays in the likelihood one moves after a penalty expires. This subsection looks into whether the heterogeneity by LTV ratios holds up when looking at zipcodes by where they fall in the quartiles of economic activity discussed in section 5.

Figures 12 and 13 repeat the results by LTV ratio as in Figure 11 but now separated by the quartiles of the average wages in the zipcode of the origin and destination of the borrowers who moved, respectively. We can see that the hump shape response in Figure 11 holds across all quartiles. Especially when focusing on destination, the heterogeneity by LTV is most pronounced when moves are to the highest wage quartile, which when combined with the previous result that when penalties expire borrowers predominantly move to this highest wage quartile, suggests that credit constraints to moving are particularly relevant for these moves that improve economic circumstances.

The results look largely similar when looking at the other measure of regional economic activity, economic connectedness. These results are shown in Figures A10 and A11 in the Appendix. The hump shaped response by LTV is also prevalent in low or high economically connected areas, whether looking at where borrowers move from or where borrowers move to.

[12] My data does not provide the exact prepayment penalty amount–to my knowledge, no U.S. mortgage data does. However, the most common rule for prepayment penalties at the time was 6 months interest. I use this rule to impute prepayment penalty amounts using the loan balance and interest rate I do have in the data. I also have used other imputations of prepayment penalty amounts, such as 3-5% of the unpaid loan balance: results are robust to each imputation.

6.1.3 Does Debt Repayment and Automobile Consumption Change around a Move?

This subsection adds to the results in the previous subsection on the role of housing equity in explaining my mobility results by studying how spending on automobiles and debt repayment behavior changes around a move. If there is a need to liquidate ones housing wealth in order to cover other debts or make important purchases, and credit market imperfections make borrowing against illiquid wealth prohibitively costly, we could also see spending behavior be affected by these prepayment penalties expiring. I caveat that this piece of evidence is merely *consistent* with credit constraints playing a role; people could also simply use moves as opportunities to purchase automobiles, for example. Nonetheless, they demonstrate spending and debt repayment do change during a move, and thus the lock-in effect of prepayment penalties still has real borrower outcomes outside of mobility.

I will focus on two main measures the Equifax data is particularly well suited to study: revolving debt and car purchases. Revolving debt is defined to be home equity lines of credit and credit card debt. I proxy for car purchases by indicating a car was purchased in a certain month when automobile balances increase by more than $2000, as done in other previous work studying autmobile purchases using Equifax data (Di Maggio et al. (2017); Berger et al. (2021)).

Figure A9 displays the results. In the 3-4 months after a prepayment penalty expires, we indeed see debt repayment and spending behavior affected. The top panel shows that revolving balances peak at approximately 10%, which is over 100% of the baseline growth rate of 9% revolving credit when prepayment penalties are in effect. Similarly, in the 3 months after a prepayment penalty expires, the fraction of automobile purchases increased by .5 percentage points, about 20% of the baseline rate of automobile purchases when prepayment penalties are in effect. These effects are large but sensible: a house sale is associated with a large increase in liquid wealth, an increase that can be used to alleviate borrowing constraints. As well, car purchases, as home purchases, typically are financed along with down payments from ones own funds. For low income, low credit borrowers, a home sale may be necessary to finance the purchase of a new car. Again, these reduced form results may be consistent with other channels, but I take these results to mean they do not *rule out* a credit constraints story, given it would be harder to believe a credit constraints story if these event studies showed zero to small effects on spending behavior.

6.2 Other Explanations

This subsection looks at other potential candidates that could explain the mobility response to prepayment penalties.

6.2.1 Credit Scores and Credit Card Utilization Rates

This subsection details the role credit score and credit card utilization plays in explaining the effect prepayment penalties have on mobility.

Figure 14 displays two important measures of ones borrower profile: the borrower's FICO credit score at origination and their previous month's utilization rate on their credit cards (defined as the monthly balance they carry as a percent of their credit limit).

The top panel separates borrowers into three groups, based on credit score: whether their loan is considered Subprime, Alt-A, and Prime loans. While there are minor exceptions, virtually these categories translate to those with credit scores below 620, between 620 and 660, and above 660, respectively. We see that there is a slightly monotonic upward relationship between credit score and treatment effects: when penalties expire, prime borrowers' monthly move rates increase by 0.20 percentage points while that of subprime borrowers' increase by 0.12 percentage points. However, given the size of the standard errors, the I cannot reject large differences across groups.

Similarly, the bottom panel separates borrowers by quartiles of credit card utilization rates of the previous month. Again, we see lower utilization rate borrowers increase moving rates than higher utilization rates, but are not statistically nor economically different from each other.

What explains the lack of difference in these categories? For credit score, many reasons can muddle the true relationship between them and treatment effects at this time. First, it is well documented the endogeneity of credit scores to the housing cycle around this time, meaning borrowers of a "true" credit score type could be mis-categorized when lending standards are lax and house prices are rising (Mian and Sufi (2017). As well, while credit scores may capture ex-ante liquidity constraints, which would imply a negative relationship between credit score and treatment effects, credit scores can also correlate with mobility opportunities or financial sophistication (Agarwal et al. (2023)), which would imply a positive relationship. In this time period, with liquidity constraints and financial sophistication likely playing large roles in the subprime mortgage market, both relationships could offset each other to make the equilibrium outcome in heterogeneous treatment effects not obvious.

The lack of heterogeneity in credit score utilization rates could be consistent with the credit score explanation, but also is consistent with not just a simple credit constraints story, but specifically *housing induced* credit constraints, namely down-payment constraints as in (Stein (1995)). Given this is a segment of the market that is likely overall very credit constrained, the only method to alleviate this constraint is building housing equity in your home, consistent with the results in the previous section on the role of housing equity.

6.2.2 Regional Proxies of Financial Sophistication

This subsection details the role financial sophistication may play in explaining the mobility responses to prepayment penalties. While there is no perfect measure of financial sophistication, I use two regional measures to proxy for financial sophistication in an area: the share of individuals in a CBSA who have at least a college degree and who are at least 55 years old, respectively. I look at these regiona measures as proxies for financial decision making because of prior evidence that shows such traits can correlate with poor financial decision-making (e.g., Laibson et al. (2009); Lusardi (2012)).

The top panel shows the share of individuals in a CBSA with at least a bachelor's degree does not exhibit any heterogeneity in treatment effects. The lowest quartile of borrowers' see an increase in monthly moving rates of 0.20 percentage points after a prepayment penalty expires, while the highest sees an increase of 0.19 percentage points.

The bottom panel shows the share of individuals in a CBSA that are at least 55 years old exhibit minimal heterogeneity in treatment effects. The lowest quartile of borrowers' see an increase in monthly moving rates of 0.20 percentage points after a prepayment penalty expires, while the highest sees an increase of 0.13 percentage points. As in the past subsection, however, the standard errors are large making the differences not statistically significantly different from one another.

Overall, while there may be issues of financial sophistication in this market overall, cross-sectionally there isn't suggestive evidence to say some borrowers being relatively *more* financially sophisticated drives any heterogeneity in treatment effects.

These results also suggest that, while I am studying the effect of prepayment penalties in the private label market of the mid 2000's, these results should generalize to other segments of the mortgage market. While subprime lending is less common today, we see that treatment effects are still strong amongst high credit score borrowers. This is consistent with prime borrowers perhaps having more moving opportunities than subprime borrowers, meaning any mortgage lock-in effects will be stronger for this group. Given the tighter lending standards post financial crisis, it is likely the average estimates I find are an underestimate of the lock-in effect of prepayment penalties in the current mortgage market.

7 Conclusion

This papers studies the effect prepayment penalties have on mobility. I do so by using a panel of millions of borrowers who took out mortgages that were later securitized in the private label market during the mid-2000's U.S. housing boom. I use the fact that prepayment penalty expiration dates depended on the age of the loan and varied by contract to run a difference-in-differences empirical specification to estimate mobility rates of borrowers before and after the penalties expire, relative to borrowers whose penalties have still not yet expired.

I find that prepayment penalties reduce mobility in an economically meaningful way: when they expire, monthly move rates increase by nearly 38% relative to the baseline rate of those facing the prepayment penalty. These moves are to locations with better labor markets and more economic opportunity. I also find strongest evidence for the equity position in ones house driving these effects, consistent with models of credit constraints, specifically down-payment constraints in the housing market (Stein (1995); Ortalo-Magne and Rady (2006)).

My results contribute to our growing knowledge of how mortgage contracts can affect mobility. I report a previously undocumented source of lock-in. While this lock-in was most prevalent in the 2000s, 5% of the mortgage market still has prepayment penalties. However, while in the past most penalties were on first liens mortgage, today they are predominantly on home equity lines of credit (HELOC). While my results are robust to the purpose a mortgage was originated, future work should study if there are any differences in lock-in effects for home equity loans, whether due to observable borrower characteristics, lending standards in the mortgage market, or both.

My results have direct policy-making and economic implications. First, the underlying institutional feature that results in prepayment penalties having lock-in effects in the U.S. is because selling a house triggers a mortgage termination thanks to mortgages largely not being assumable (another borrower can move in and take over the mortgage, subject to meeting a new down payment) nor portable (a borrower can take their old mortgage with them to a new home, subject to meeting a new down payment). Second, my results suggest the change in lock-in was largely unanticipated by borrowers until it occurred, implying the disclosure of such contract details can have an affect on the degree to which they lock borrowers into their contract. Since the financial crisis, efforts have been made to disclose prepayment penalty terms in a clearer way. Future work should study whether these disclosures have helped borrowers anticipate these expirations more, allowing them to make more informed move decisions that account for future changes in their mortgage.

Finally, while I show that prepayment penalties serve as a financial constraint to moving, the costs of a mortgage feature is just one side of the coin: it is still not obvious in a normative sense whether prepayment penalties are a net benefit to consumers. This is especially relevant given the restrictions placed on prepayment penalties since Dodd-Frank has been in effect, making it important to know whether borrowers have indeed been made better off. In future work, I hope to empirically estimate the extent to which prepayment penalties do provide benefits to consumers in the form of easier access to credit or lower interest rates, whether through solving asymmetric information issues (Dunn and Spatt (1985); Mayer et al. (2013)) at mortgage origination, or slowing refinancing speeds that can help lower interest rates (Amromin et al. (2020); Berger et al. (2023); Zhang (2023)).

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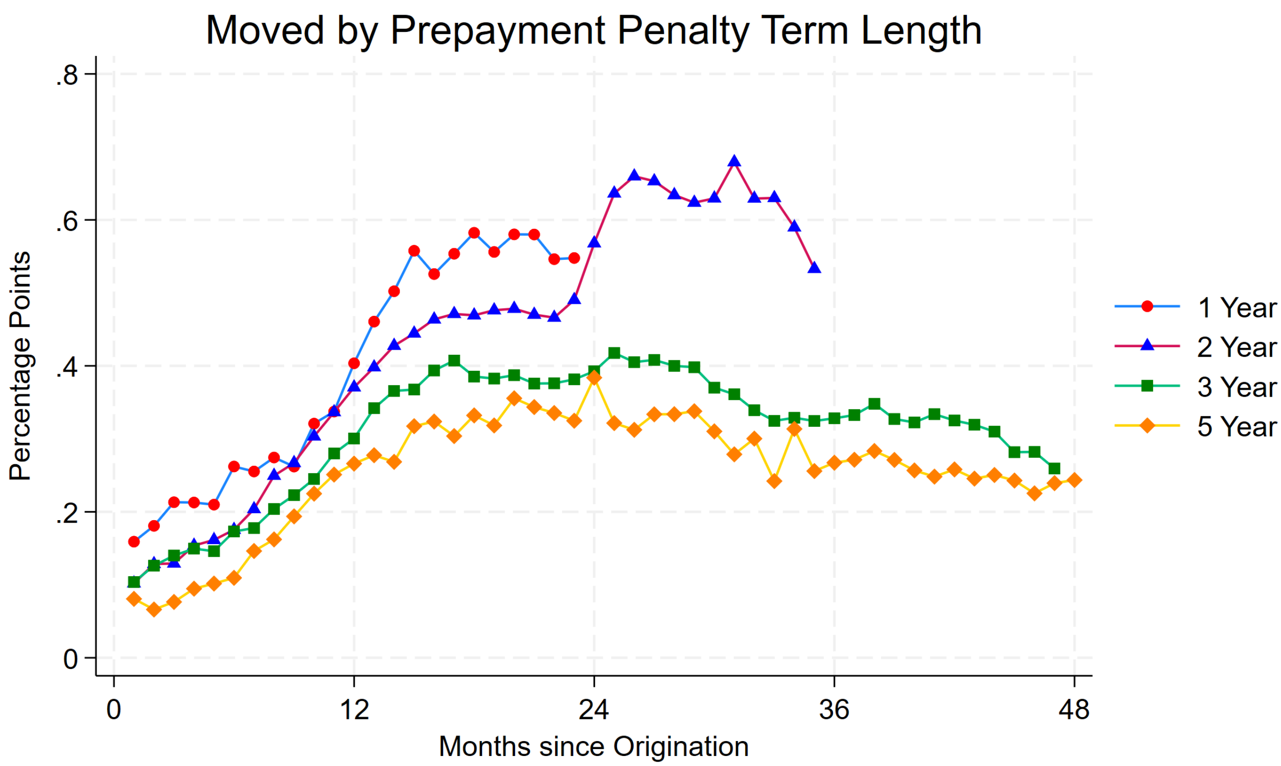
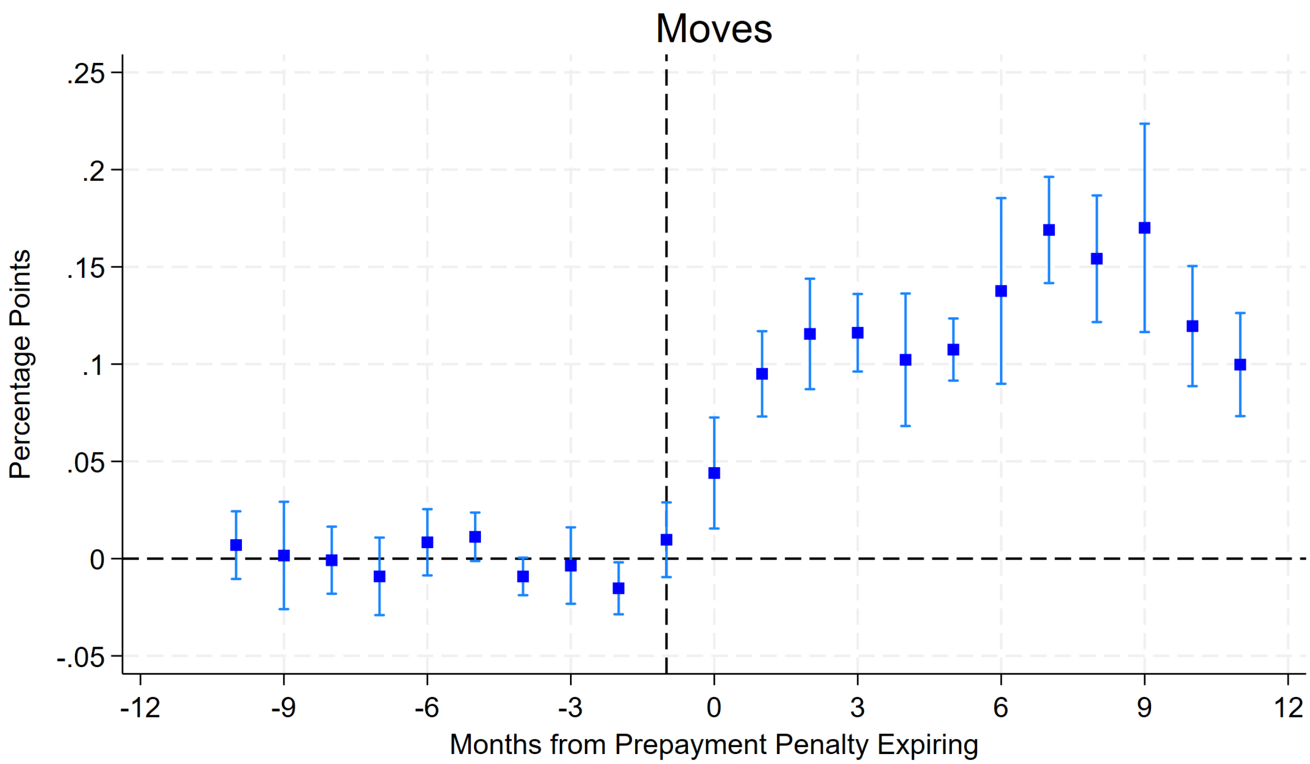
Figure 1: Monthly Moves by Prepayment Penalty Term



This figure shows the monthly moving and prepayment rates for the four types of prepayment penalty term lengths I study: the 1, 2, 3, and 5 year penalty bans. Move is defined here as the first month where the zipcode of the mortgaged property no longer appears in a borrowers Equifax credit bureau records. The top panel plots the move rates in a given month where the mortgage was voluntarily prepaid within a six month window; the bottom panel shows the prepayment rates in a given month where the borrower has moved from the mortgaged property within a six month window.

Figure 2: Event Study: Monthly Moves by Prepayment Penalty Term

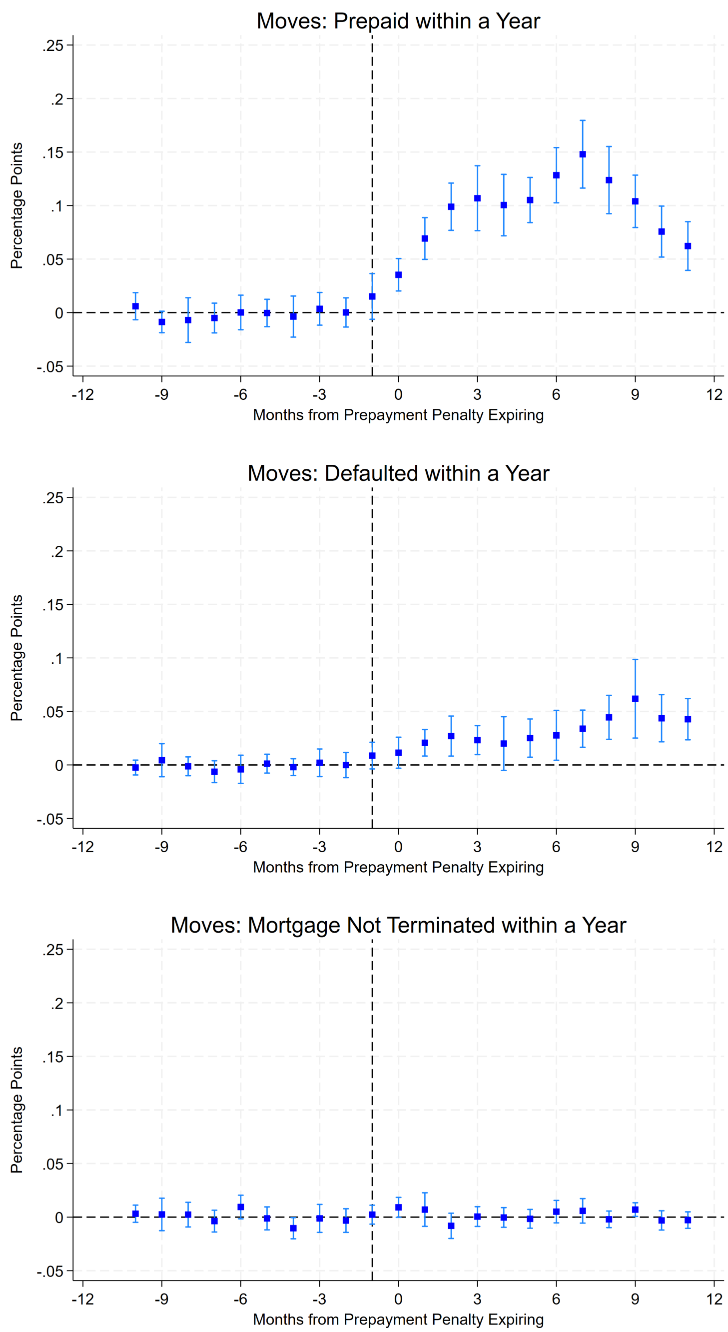


This figure estimates the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 3: Event Study: Monthly Moves by Penalty Term and Mortgage Performance

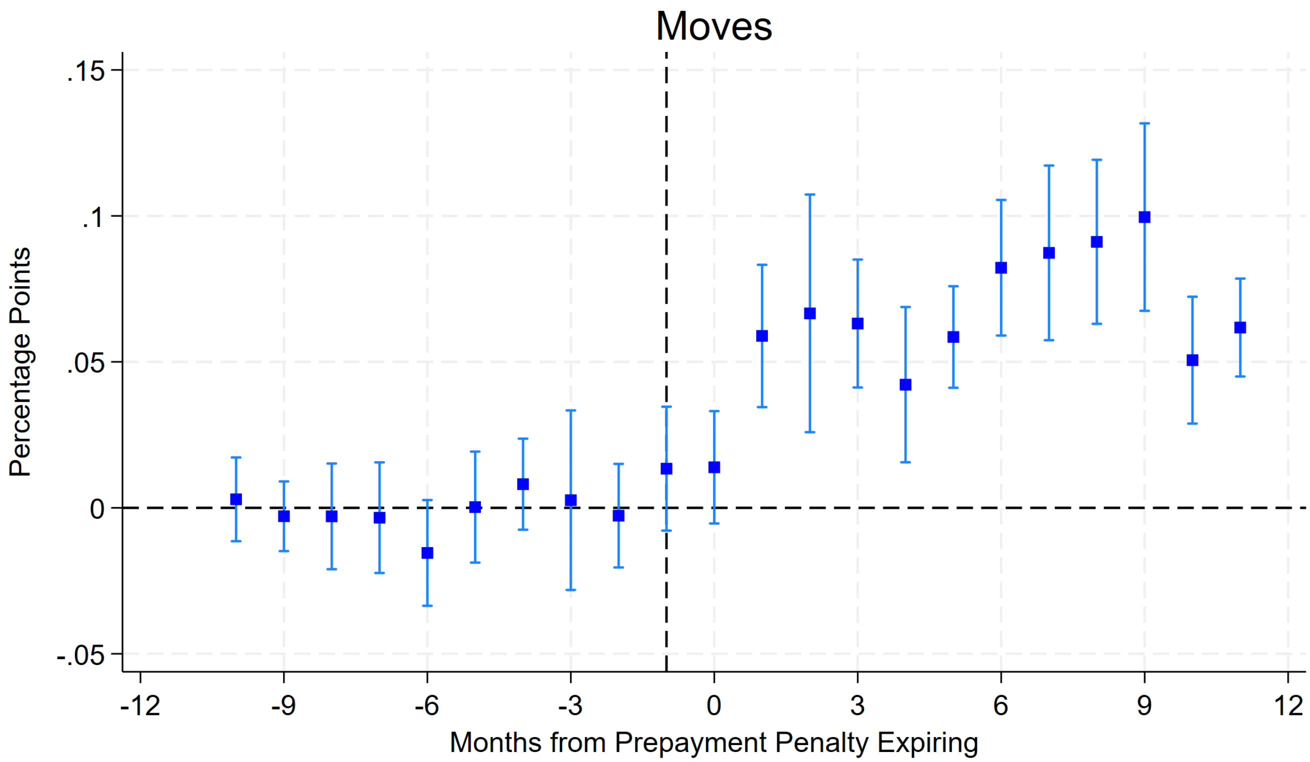


This figure estimates the effect prepayment penalties have on mobility, with mobility separated by whether its accompanied with a mortgage prepayment, default, or no termination, according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 4: Instrumented Difference-in-Differences

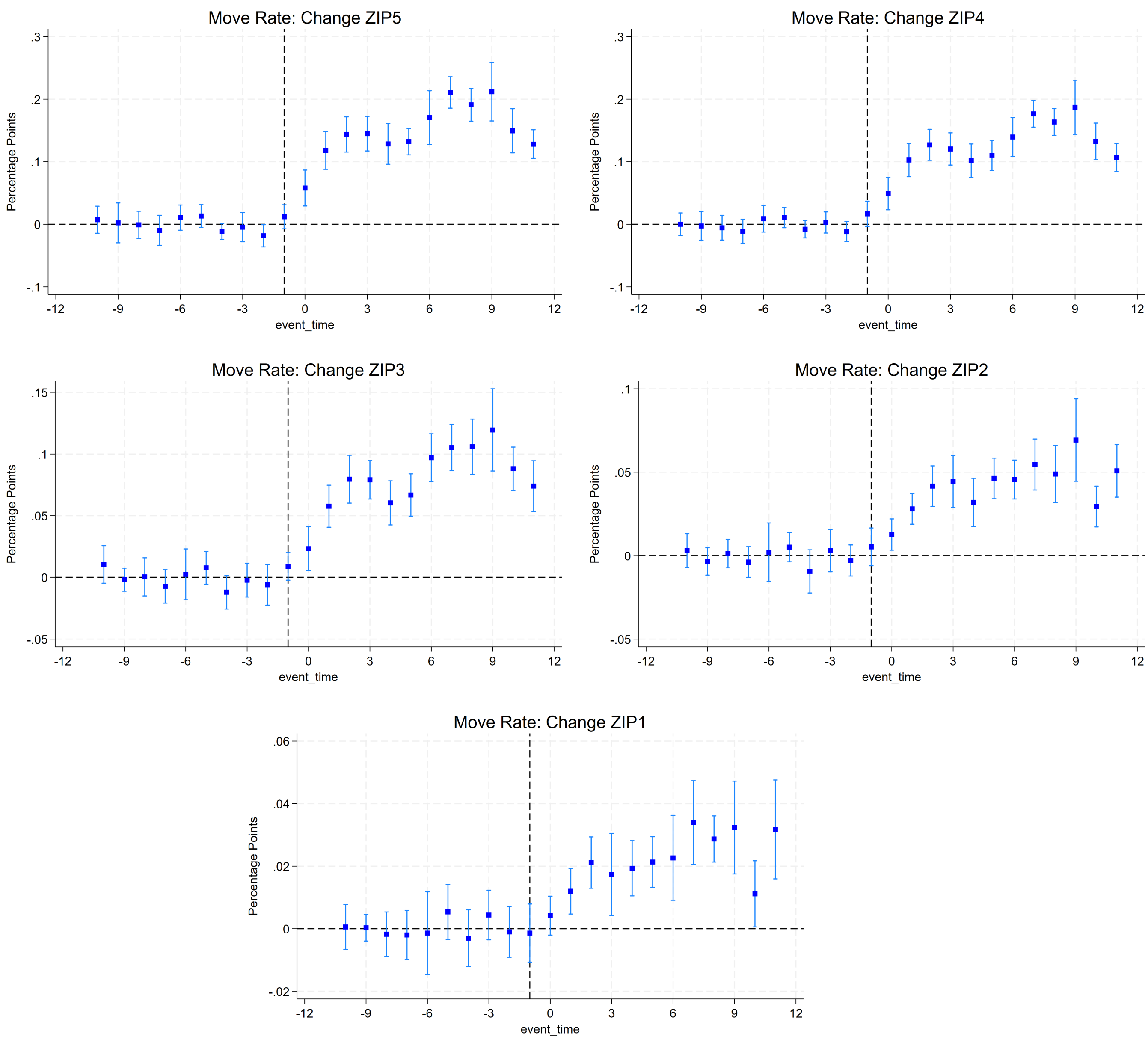


This figure estimates the reduced form effect the prepayment penalties instrument has on mobility according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta MBSPredPenaltyExpired_{d(b),t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The instrument $\Delta MBSPredPenaltyExpired_{d(b),t}$ equals one in the month the most common penalty in borrower b's MBS pool $d(b)$ expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 5: Move by Distance

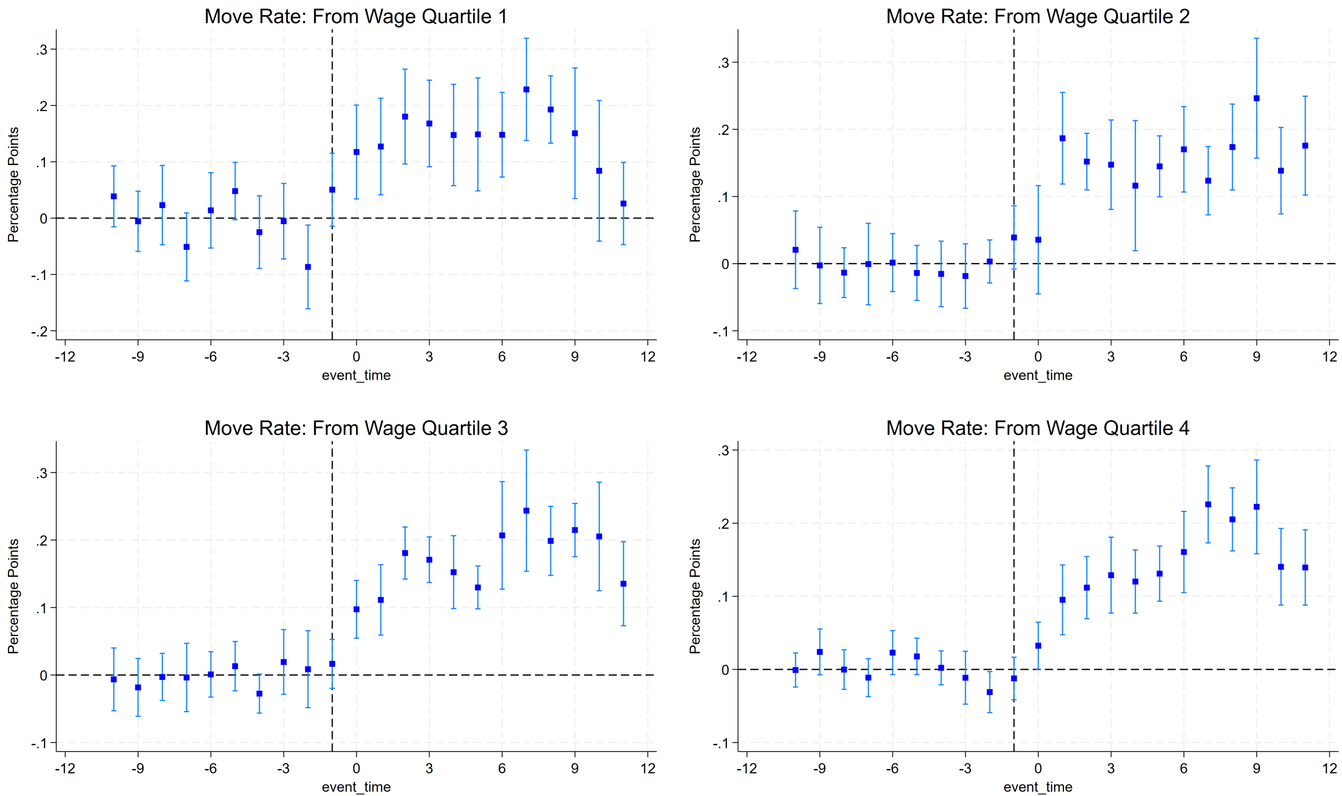


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 6: Move by Average Wage in Zipcode Origin

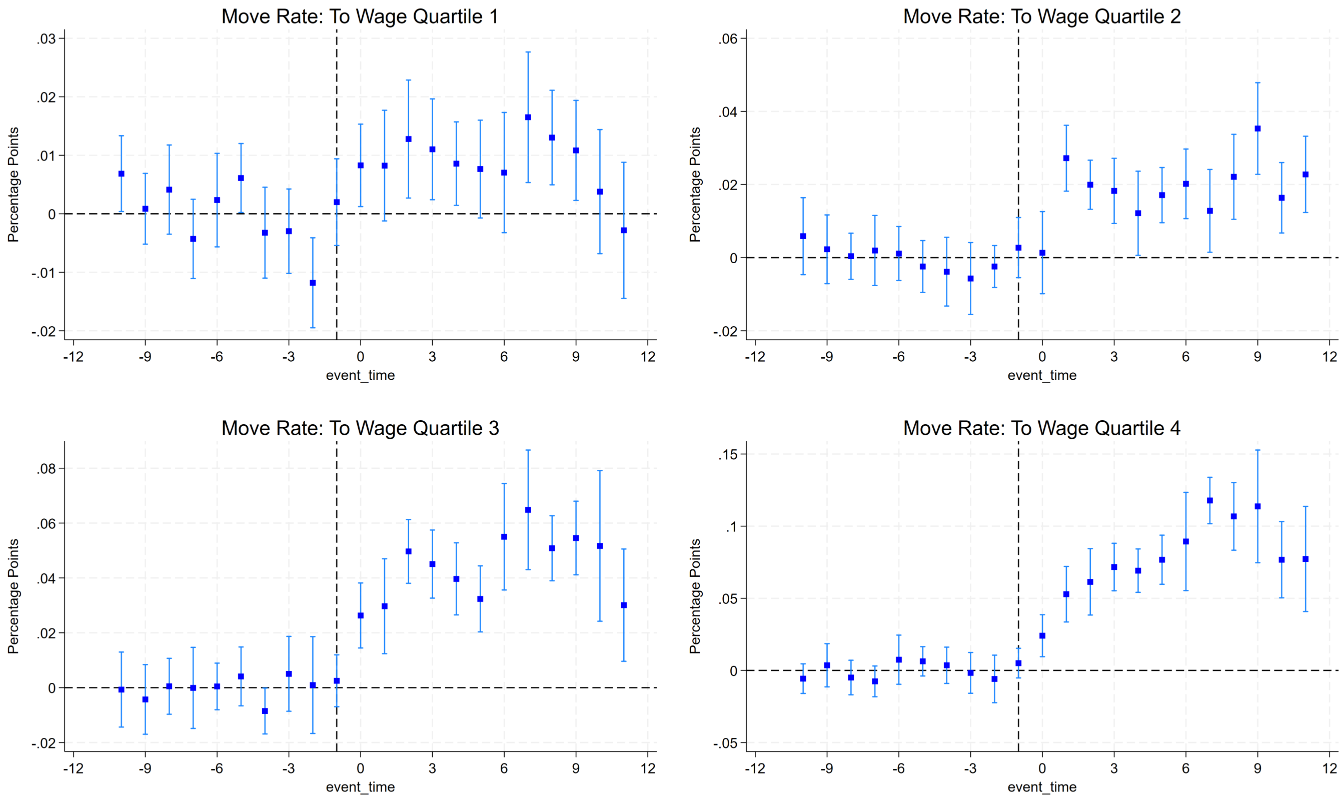


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 7: Move by Average Wage in Zipcode Destination

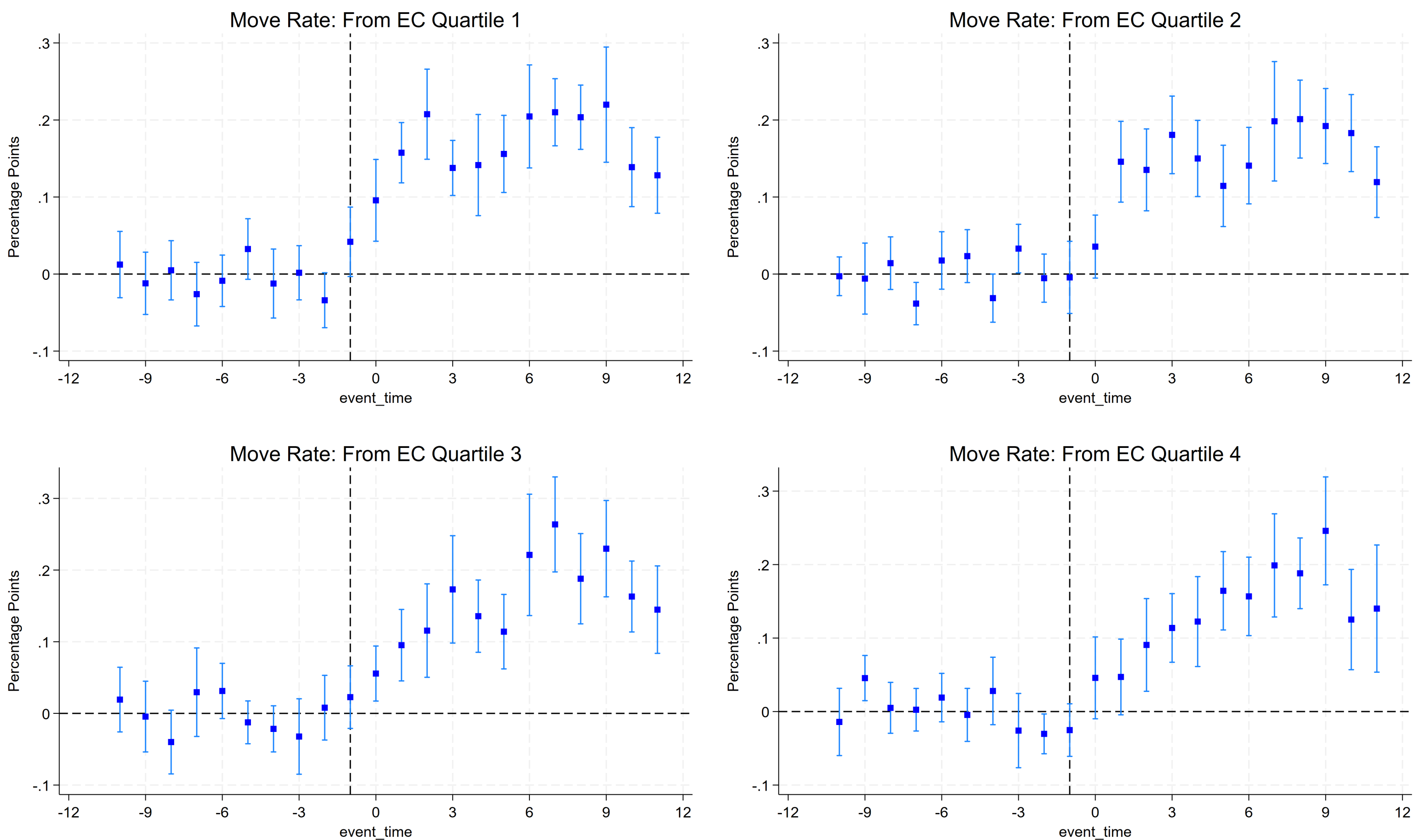


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 8: Move by Average Economic Connectedness in Zipcode Origin

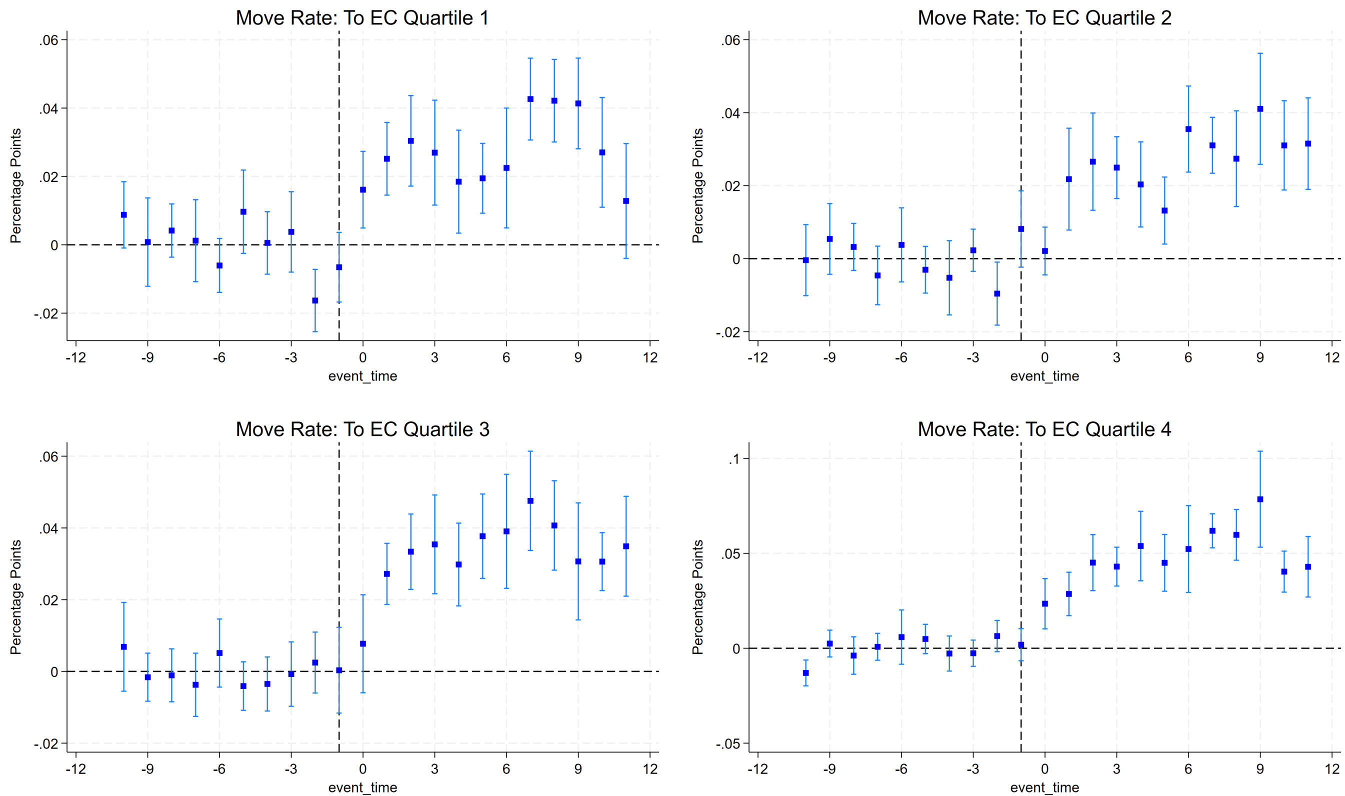


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 9: Move by Average Economic Connectedness in Zipcode Destination

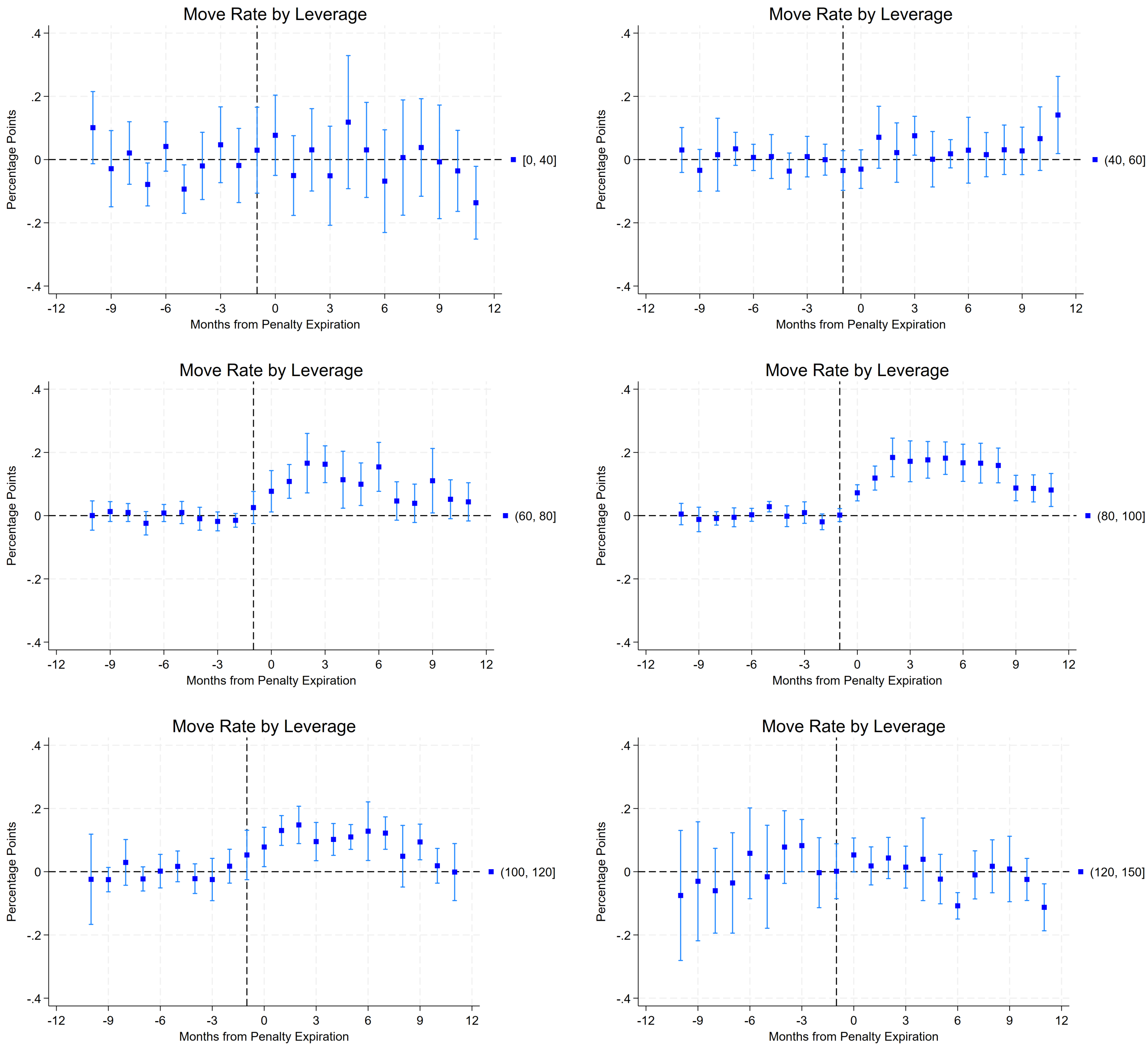


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 10: Heterogeneity: LTV Ratios

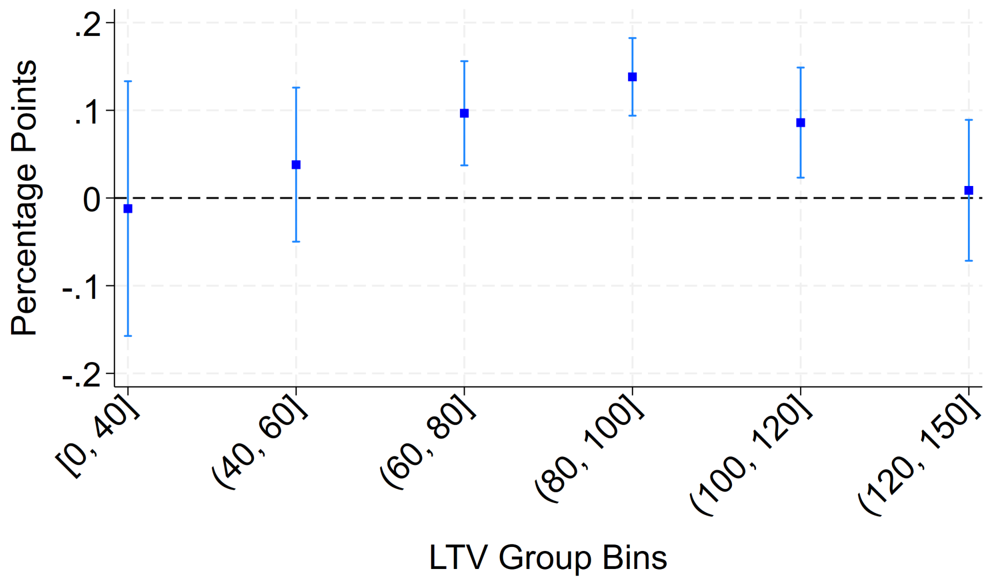


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 11: Heterogeneity: LTV Ratios

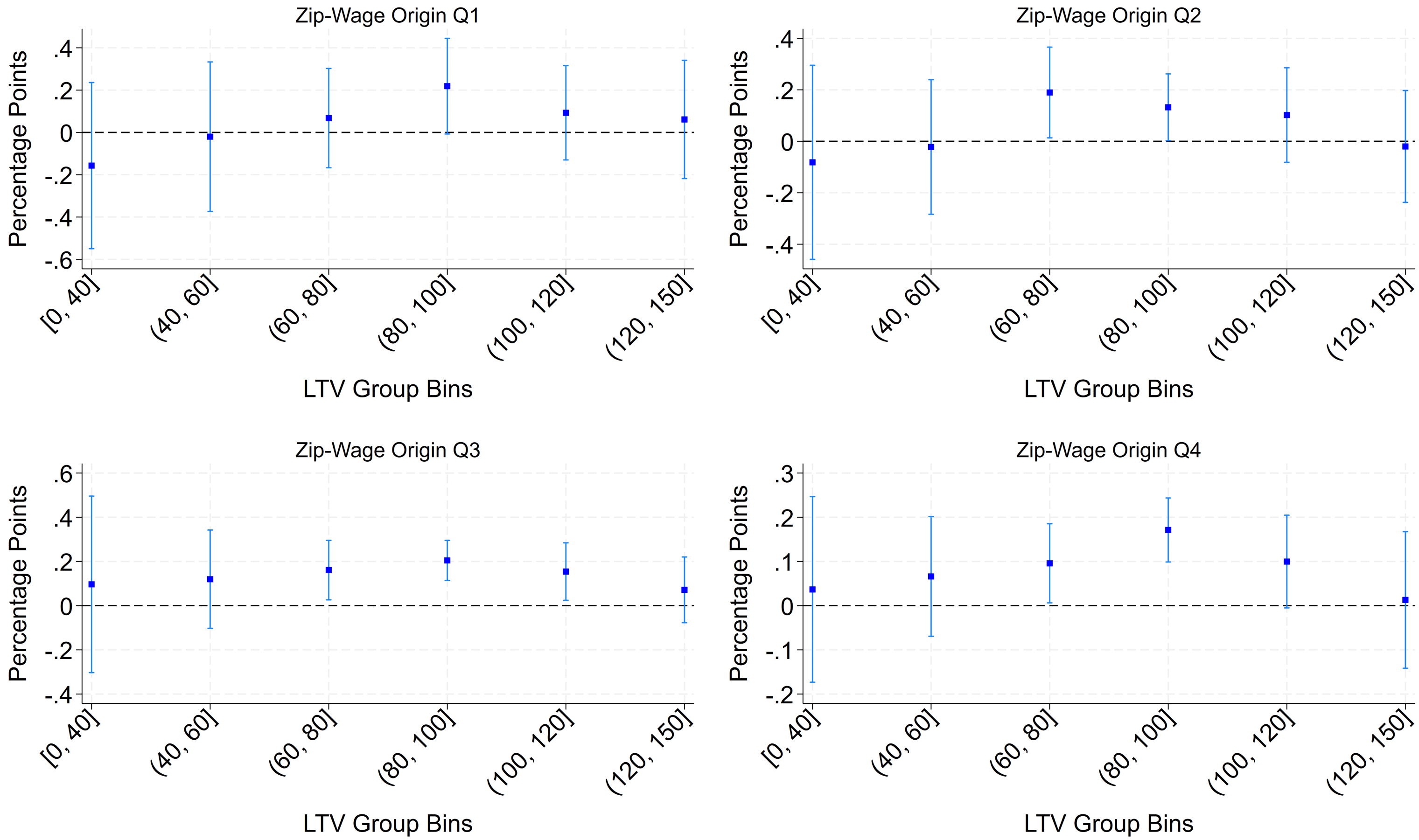


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 12: Heterogeneity: LTV Ratios and Zip-Wage Origin

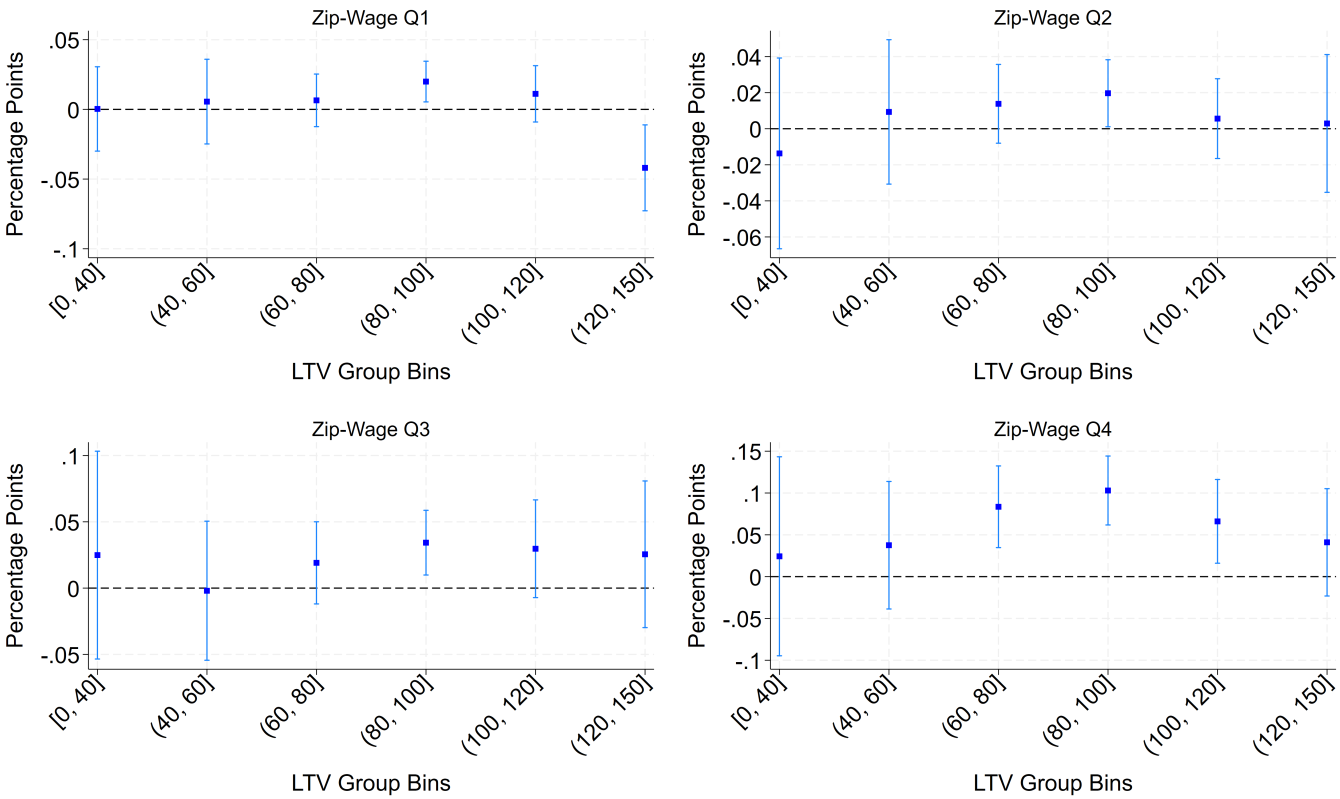


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 13: Heterogeneity: LTV Ratios and Zip-Wage Destination

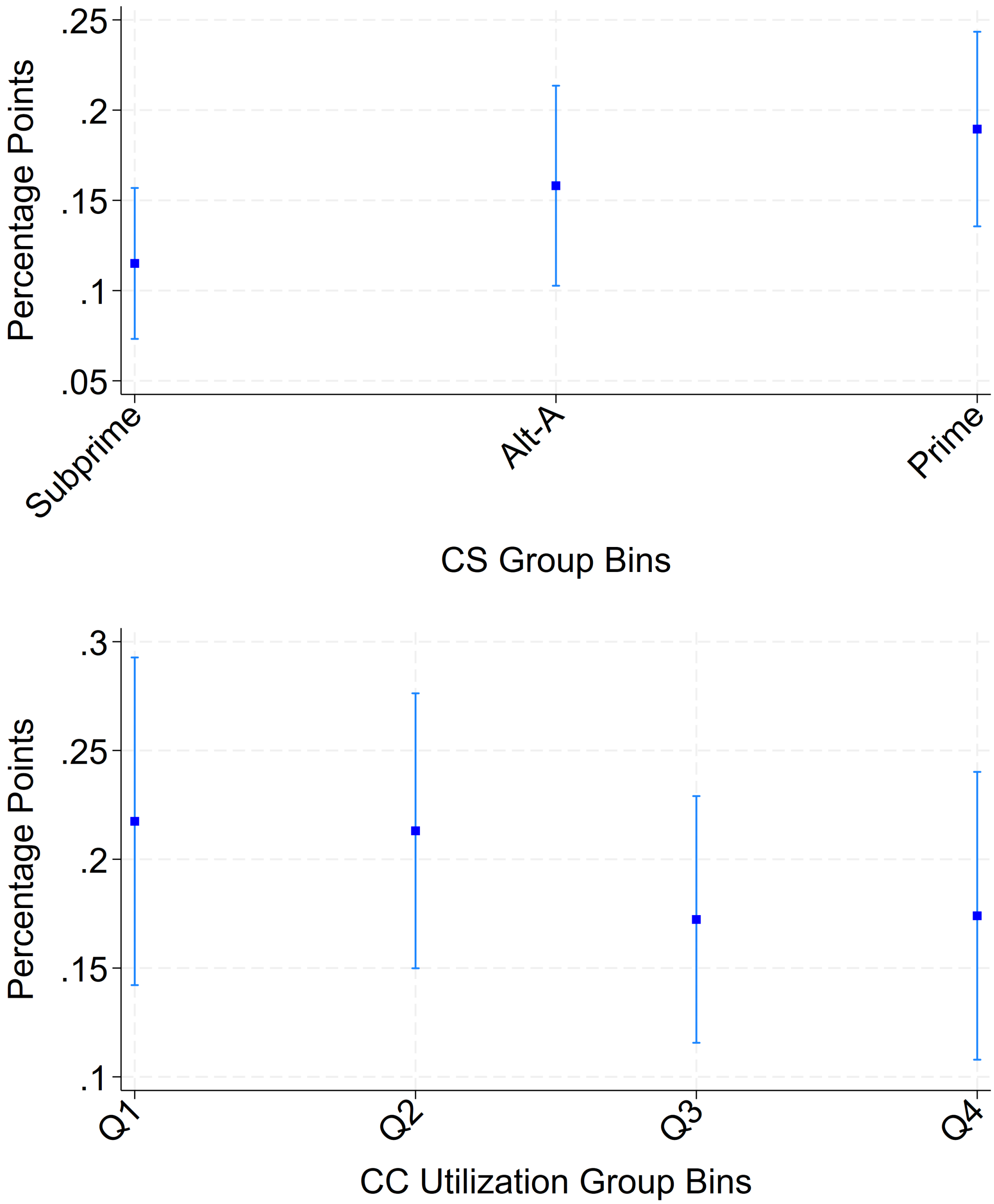


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 14: Heterogeneity by Credit Score and Utilization Rates

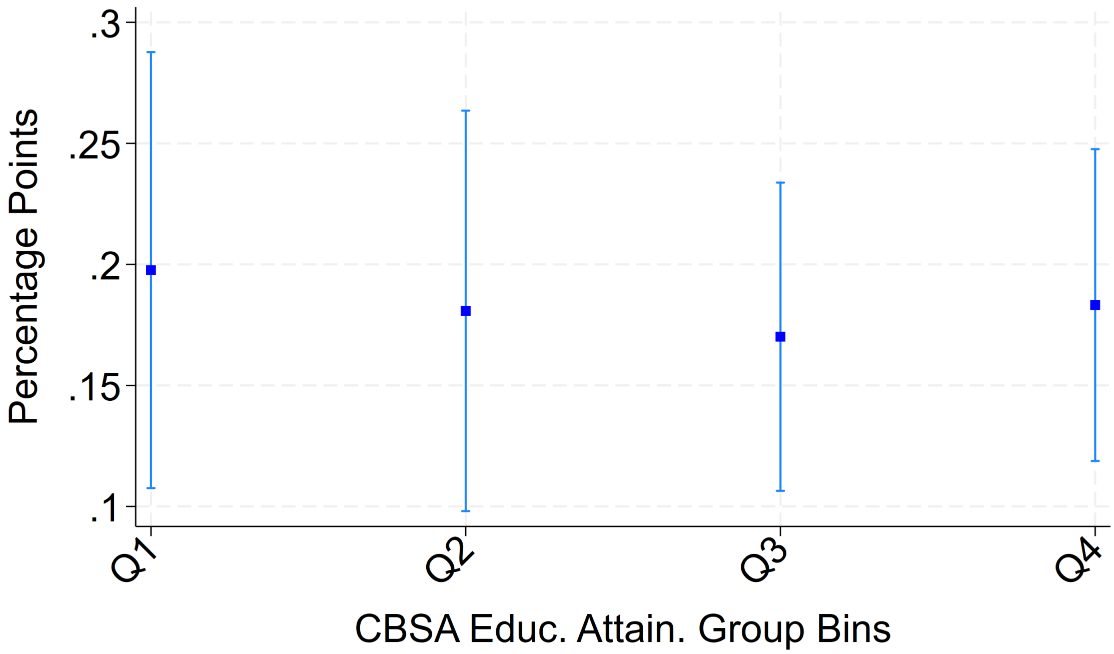


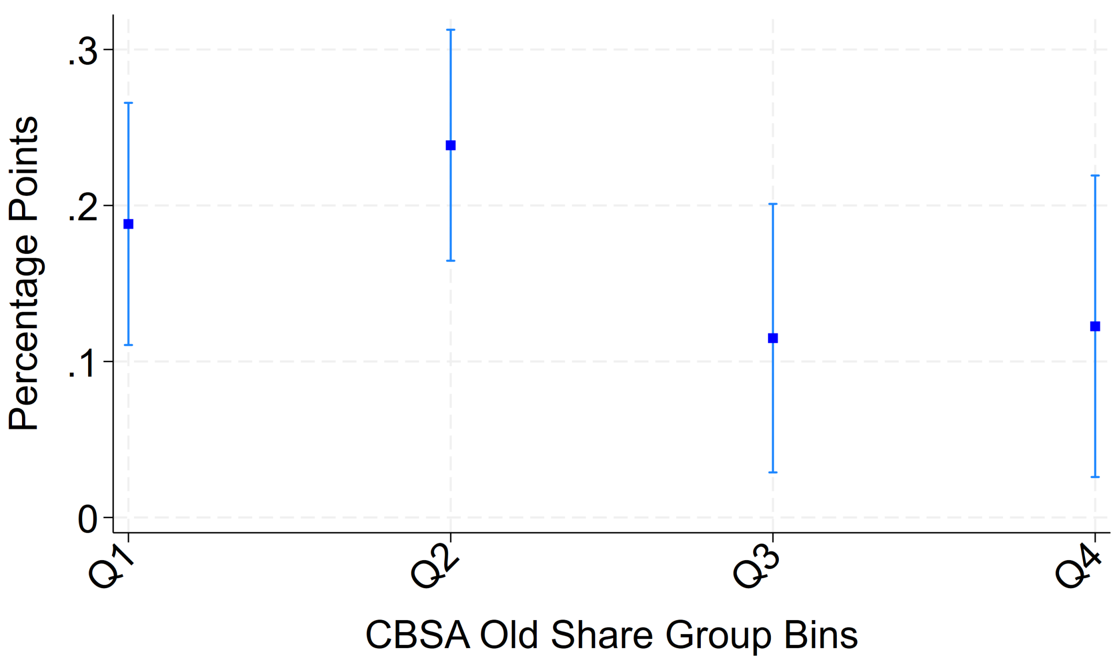
This figure estimates how the effect prepayment penalties have on mobility varies by FICO credit scores at mortgage origination according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure 15: Heterogeneity by CBSA Educational Attainment and Age





This figure estimates how the effect prepayment penalties have on mobility varies by FICO credit scores at mortgage origination according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 1: Summary Statistics: Origination Characteristics

	Prepayment Penalty Term Length			
	12	24	36	60
	mean	mean	mean	mean
Predicted Income (Thous. USD)	45.91	36.47	41.59	36.57
Equifax Vantage 2 Credit Score	689.23	642.53	679.83	658.28
FICO Credit Score	667.27	614.67	655.47	628.31
Mortgage Balance (Thous. USD)	3.5e+05	2.1e+05	2.5e+05	2.1e+05
CLTV (%)	81.80	86.19	82.49	81.07
Interest Rate (%)	5.65	7.68	6.08	7.37
Observations	150776	587261	525221	66456

This table shows summary statistics of the mortgage records at origination. Predicted income is provided by Equifax and is derived by a proprietary algorithm using their credit records to best predict a borrower's income. Vantage 2 and FICO credit scores come from Equifax and Moody's Analytics, respectively. CLTV is the combined loan-to-value ratio at origination. Each column corresponds to one of the four prepayment penalty lengths used in the analysis.

Table 2: Summary Statistics: Performance under Penalty

	Prepayment Penalty Term Length			
	12	24	36	60
	mean	mean	mean	mean
Prepaid	0.47	1.50	1.32	0.81
Defaulted	0.63	1.31	1.03	0.94
Moved	0.30	0.28	0.25	0.19
Purchased a Car	2.67	2.99	2.47	2.39
3 Month Pct. Change in Revolving Credit	22.09	10.51	7.19	6.66
Observations	1367015	10034619	14394950	2216675

This table shows summary statistics of the mortgage records over the life of the loan. Prepaid is an indicator for the month a loan is voluntarily paid off. Defaulted is the month a loan is first reported as 60 days past due. Moved is the month after the last month the original zipcode of the mortgaged property appears in Equifax records. Purchased a car is any month the automobile debt balance discreetly increases by at least $2000 dollars in Equifax. Revolving credit is the sum of balances on home equity lines of credit (HELOCs) and credit cards. Each column corresponds to one of the four prepayment penalty lengths used in the analysis.

Table 3: What Explains Variation in Prepayment Penalty Term Length?

	Credit Supply Proxies		
	Lender	Servicer	MBS
Adj. R^2	0.11	0.08	0.23

	Credit Demand Proxies						
	PIM	Van.	FICO	Bal	Rate	LTV	Subprime
Adj. R^2	0.00	0.00	0.00	0.02	0.04	0.02	0.00

This table estimates the explanatory power variables used in my analysis have on predicting prepayment penalty length by predicting adjusted r-squareds of the following specification:

$$PrepaymentPenaltyLength_b = \alpha_{x(b)} + \epsilon_b$$

From left to right, the variables x are Equifax's predicted income model, Equifax's Vantage 2 credit score, FICO credit score, the loan balance at origination, the interest rate at origination, the LTV ratio at origination, whether the loan was labelled subprime, whether the loan has a fixed or adjustable rate, the lender identifier, the loan servicer identifier, and the loan's MBS pool identifier.

Table 4: What Explains Variation in Predicted Prepayment Penalty Term Length?

	Credit Supply Proxies		
	Lender	Servicer	MBS
Adjusted R-Squared	0.23	0.21	0.81

	Credit Demand Proxies						
	PIM	Van.	FICO	Bal	Rate	LTV	Subprime
Adjusted R-Squared	0.00	0.00	0.01	0.01	0.10	0.03	0.01

This table estimates the explanatory power variables used in my analysis have on predicting the MBS pool prepayment penalty instrument length by predicting adjusted r-squareds of the following specification:

$$MBSPredPenalty_b = \alpha_{x(b)} + \epsilon_b$$

From left to right, the variables x are Equifax's predicted income model, Equifax's Vantage 2 credit score, FICO credit score, the loan balance at origination, the interest rate at origination, the LTV ratio at origination, whether the loan was labelled subprime, whether the loan has a fixed or adjustable rate, the lender identifier, the loan servicer identifier, and the loan's MBS pool identifier.

Table 5: State Migration Outflows not Predicted by MBS Penalty Composition

	Migration Outflows			
1 Year Penalty Share	-0.02			
	(0.01)			
2 Year Penalty Share		0.03^{***}		
		(0.01)		
3 Year Penalty Share			-0.02^{*}	
			(0.01)	
5 Year Penalty Share				0.00
				(0.01)
R-Squared	0.000	0.001	0.001	0.000
N	14507	14507	14507	14507

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates how well the composition of prepayment penalties in an MBS pool predict the state average migration patterns in these MBS pools. Specifically, the table reports the coefficient ρs of the regression

$$\frac{1}{N_d}\sum_{s(d)\in d}\frac{MigrantOutflows}{NonMigrantPopulation}_{s(d)} = \alpha + \rho NYearPenaltyShare_{d,c} + \varepsilon_{d,c}$$

where each variable is standardized so the coefficients can be interpreted as correlation coefficients. Standard errors are clustered by month of origination.

Table 6: State Migration Flows not Predicted by MBS Penalty Composition

	Out Flows	Gross Flows	Net Flows
Short Penalty Share	0.02*	0.02*	0.02
	(0.01)	(0.01)	(0.01)
R-Squared	0.000	0.001	0.000
N	14507	14507	14507

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates how well the composition of prepayment penalties in an MBS pool predict the state average migration patterns in these MBS pools. Specifically, the table reports the coefficient ρs of the regression

$$\frac{1}{N_d}\sum_{s(d)\in d}\frac{MigrantOutflows}{NonMigrantPopulation}_{s(d)} = \alpha + \rho ShortTermPenaltyShare_{d,c} + \varepsilon_{d,c}$$

$$\frac{1}{N_d}\sum_{s(d)\in d}\frac{MigrantOutflows + MigrantInflows}{NonMigrantPopulation}_{s(d)} = \alpha + \rho ShortTermPenaltyShare_{d,c} + \varepsilon_{d,c}$$

$$\frac{1}{N_d}\sum_{s(d)\in d}\frac{MigrantOutflows - MigrantInflows}{NonMigrantPopulation}_{s(d)} = \alpha + \rho ShortTermPenaltyShare_{d,c} + \varepsilon_{d,c}$$

where each variable is standardized so the coefficients can be interpreted as correlation coefficients. $ShortTermPenaltyShare$ is the share of mortgages with 1 or 2 year prepayment penalties in their MBS pool. Standard errors are clustered by month of origination.

Table 7: Mobility Increases when Prepayment Penalties Expire

	Moved						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Penalty Expiration	0.12**	0.14***	0.13***	0.14***	0.19***	0.15***	0.19***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Month FE	Y	Y	Y	Y	Y	Y	Y
Cohort-Month FE	N	Y	N	Y	Y	Y	Y
Lender-Month FE	N	N	Y	Y	Y	Y	Y
BorrChar-Month FE	N	N	N	N	Y	N	Y
Zip-Month FE	N	N	N	N	N	Y	Y
N (millions)	35.929	35.929	35.912	35.912	20.682	35.603	20.391

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 8: MBS Instrumental Variables Design

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.14***			0.22***
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.37***	0.08***	
		(0.04)	(0.01)	
N (millions)	35.912	38.665	35.912	35.912

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta \widehat{PenaltyExpired}_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

$$\Delta PenaltyExpired_{b,t} = \gamma \Delta MBSPredPenaltyExpired_{d(b),t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The instrument $\Delta MBSPredPenaltyExpired_{d(b),t}$ equals one in the month the most common penalty in borrower b's MBS pool $d(b)$ expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 9: Mobility Response by Distance

	(1) ZIP5	(2) ZIP4	(3) ZIP3	(4) ZIP2	(5) ZIP1
Penalty Expired	0.14***	0.12***	0.08***	0.04***	0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Control Mean	0.38	0.31	0.19	0.11	0.05
N (millions)	35.912	36.038	36.237	36.356	36.435

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 10: Mobility Response by Average Wage of Zip-Code Origin

	Q1	Q2	Q3	Q4
Penalty Expired	0.15***	0.15***	0.16***	0.13***
	(0.03)	(0.02)	(0.01)	(0.01)
N (millions)	4.297	6.959	10.907	13.491

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 11: Mobility Response by Average Wage of Zip-Code Destination

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.14***	0.01***	0.02***	0.03***	0.09***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
N	35.912	35.912	35.912	35.912	35.912

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 12: Mobility Response by Economic Connectedness of Zip-Code Origin

	Q1	Q2	Q3	Q4
Penalty Expired	0.17^{***}	0.14^{***}	0.15^{***}	0.13^{***}
	(0.02)	(0.01)	(0.01)	(0.02)
N (millions)	12.552	8.852	7.348	5.986

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 13: Mobility Response by Average Economic-Connectedness of Zip-Code Destination

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.14***	0.03***	0.02***	0.03***	0.05***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
N (millions)	35.912	35.912	35.912	35.912	35.912

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 14: Mobility Response by Wage of Destination and Origination

Panel A: Q1 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.15***	0.03***	0.04**	0.03***	0.05***
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
N (millions)	4.297	4.297	4.297	4.297	4.297

Panel B: Q2 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.15***	0.02***	0.03***	0.04***	0.06***
	(0.02)	(0.00)	(0.01)	(0.01)	(0.01)
N (millions)	6.959	6.959	6.959	6.959	6.959

Panel C: Q3 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.16***	0.02***	0.03***	0.04***	0.08***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
N (millions)	10.907	10.907	10.907	10.907	10.907

Panel D: Q4 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.13***	0.00*	0.01**	0.02***	0.11***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
N (millions)	13.491	13.491	13.491	13.491	13.491

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table 15: Mobility Response by EC of Destination and Origin

Panel A: Q1 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.17***	0.07***	0.03***	0.03***	0.02***
	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)
N (millions)	12.552	12.552	12.552	12.552	12.552

Panel B: Q2 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.14***	0.03***	0.03***	0.03***	0.04***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
N (millions)	8.852	8.852	8.852	8.852	8.852

Panel C: Q3 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.15***	0.01***	0.02***	0.04***	0.05***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
N (millions)	7.348	7.348	7.348	7.348	7.348

Panel D: Q4 Origin

	All	Q1	Q2	Q3	Q4
Penalty Expired	0.13***	0.01**	0.02**	0.02***	0.07***
	(0.02)	(0.00)	(0.01)	(0.01)	(0.01)
N (millions)	5.986	5.986	5.986	5.986	5.986

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

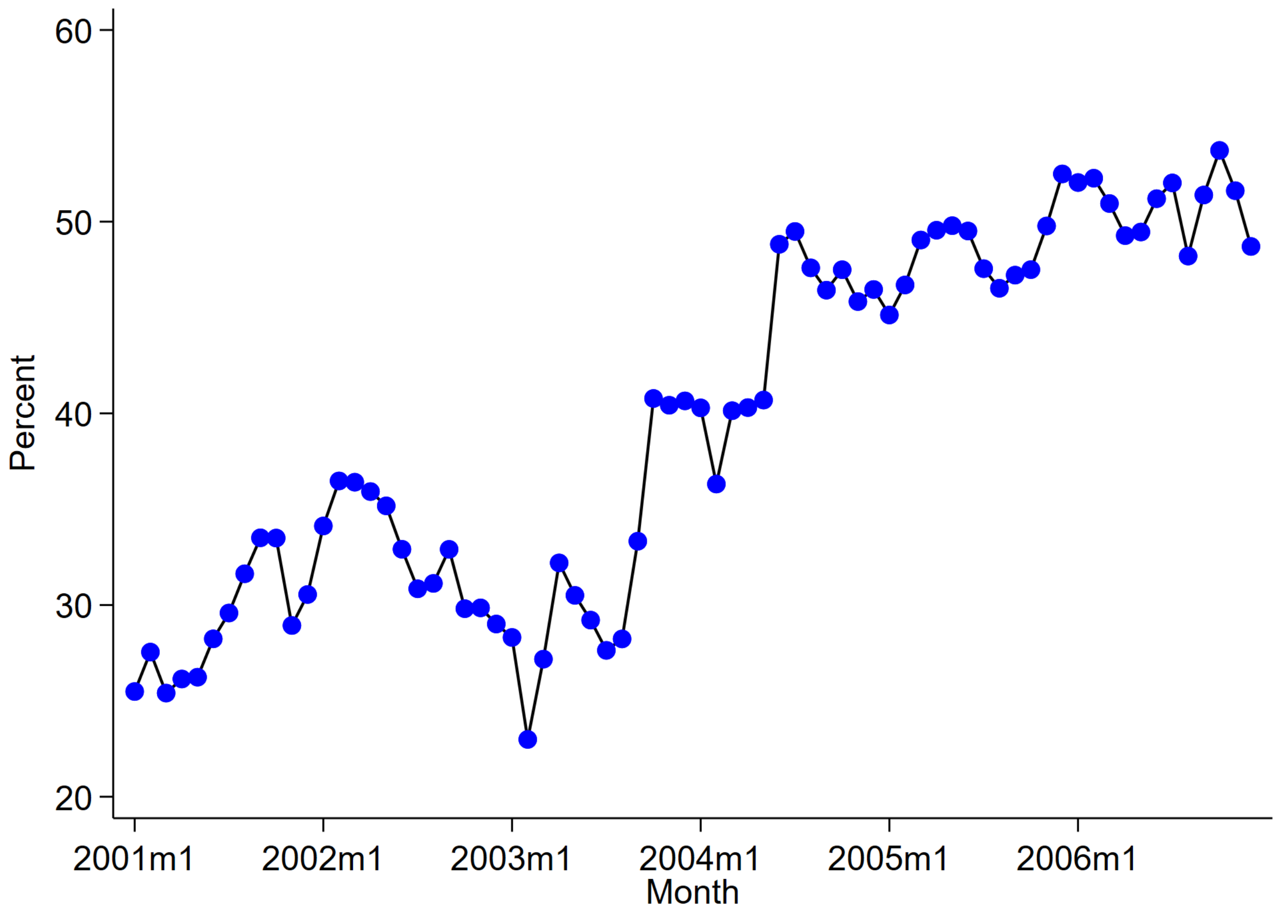
This table estimates the effect prepayment penalties have on small versus large distance moves according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. For each column, a move is defined to be when the first n digits of the borrower's zipcode of their primary address changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

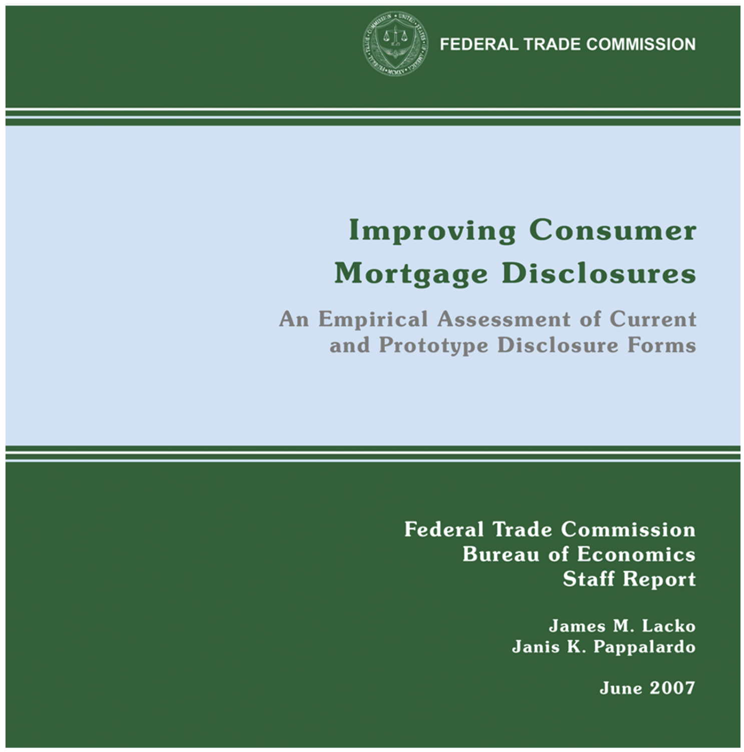
Appendix A Additional Figures and Tables

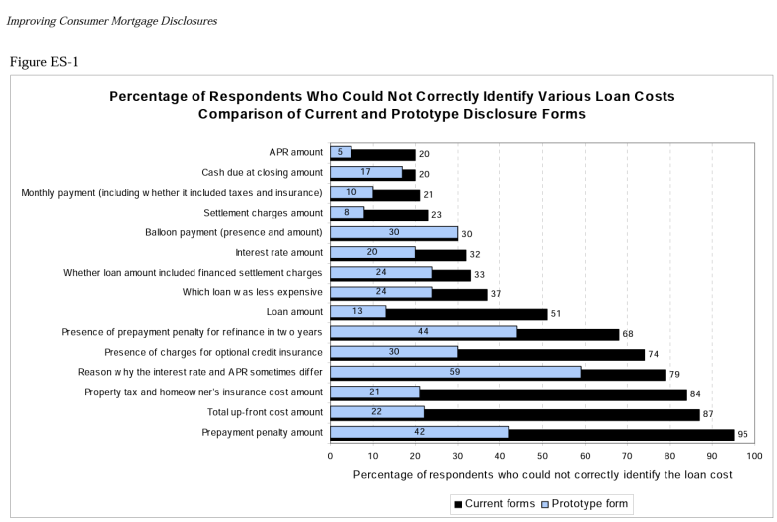
Figure A1: Percent of PLS Mortgages with a Prepayment Penalty



This figure shows the percent of privately securitized mortgages with a prepayment penalty form 2001 to 2006.

Figure A2: Disclosure Report Results





This figure shows the front page and results of the Federal Trade Commission's study on understanding the complexity of mortgage disclosure forms during the 2000s housing boom.

Figure A3: Prepayment Penalty Information on Pre-Housing Crisis Disclosure Forms

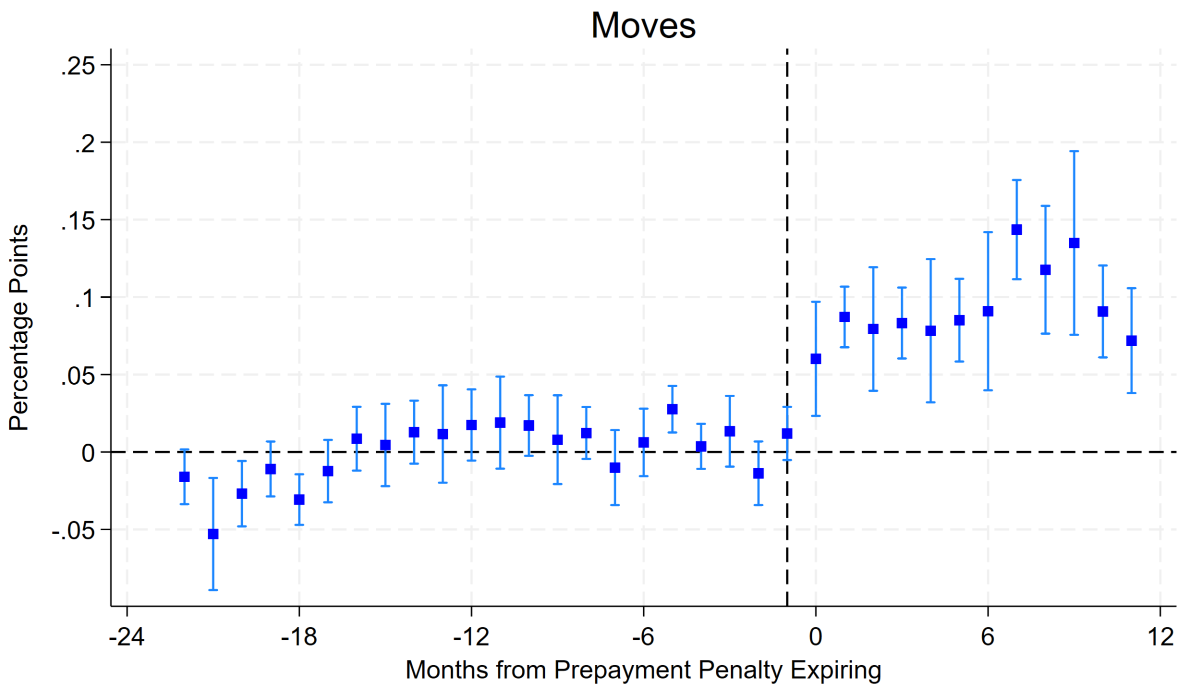
Prepayment: If you pay off early, you

☐ may ☒ will not have to pay a penalty.

☐ may ☒ will not be entitled to a refund of part of the finance charge.

This figure shows how the existence of a prepayment penalty was disclosed in closing documents for mortgages during the 2000s housing boom.

Figure A4: Dynamic DiD: Monthly Moves by Penalty Term, Longer Pre-Trend

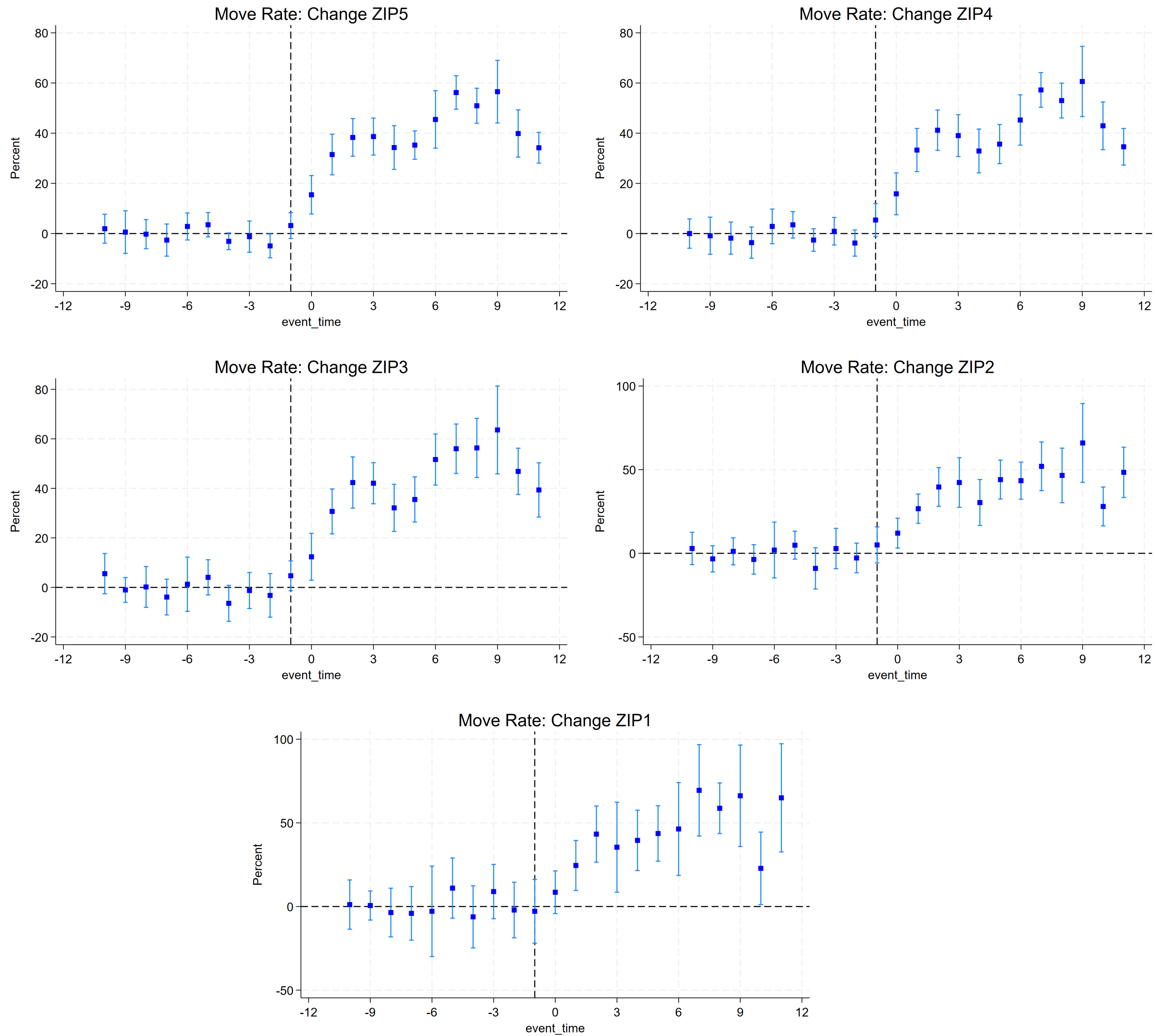


This figure estimates the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A5: Move by Distance: Scaled by Baseline Move Rate

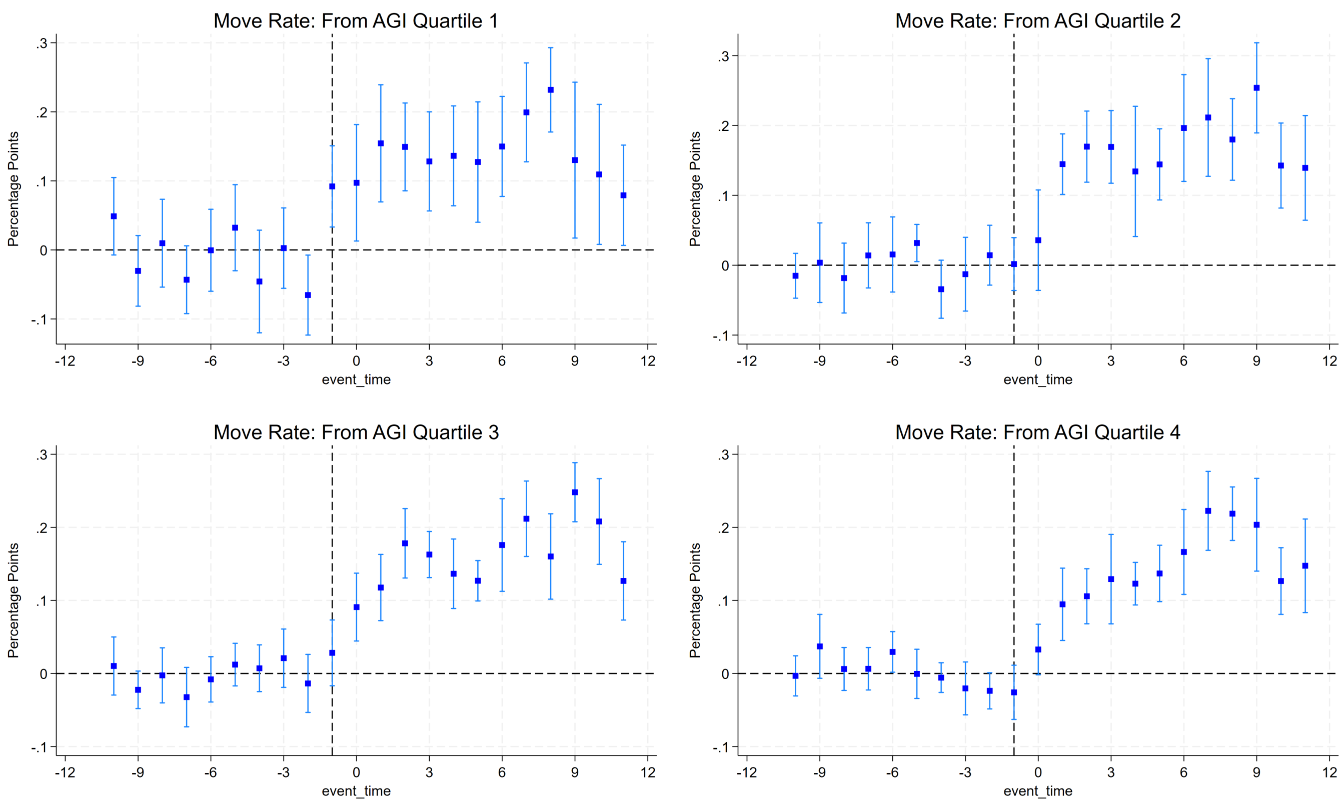


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A6: Move by Average Adjusted Gross Income in Zipcode Origin

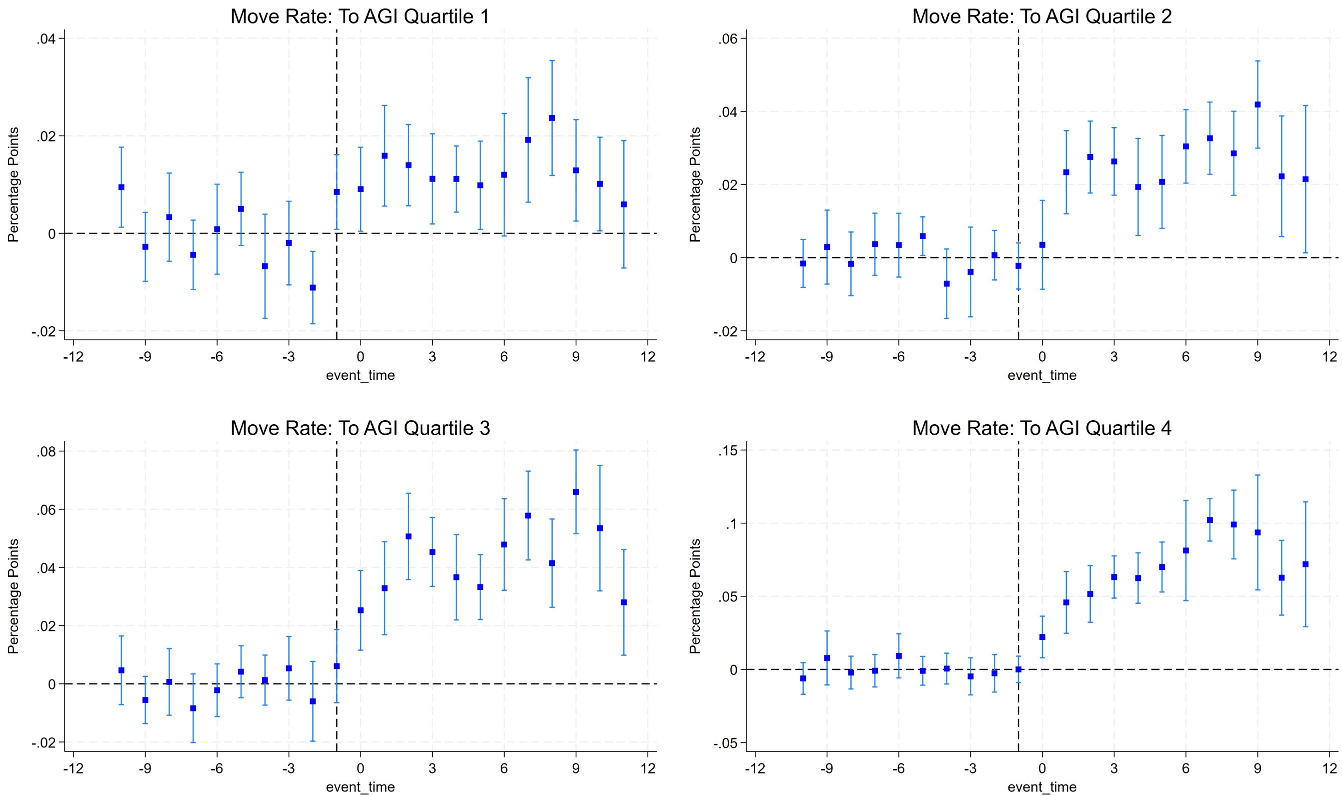


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A7: Move by Average Adjusted Gross Income in Zipcode Destination

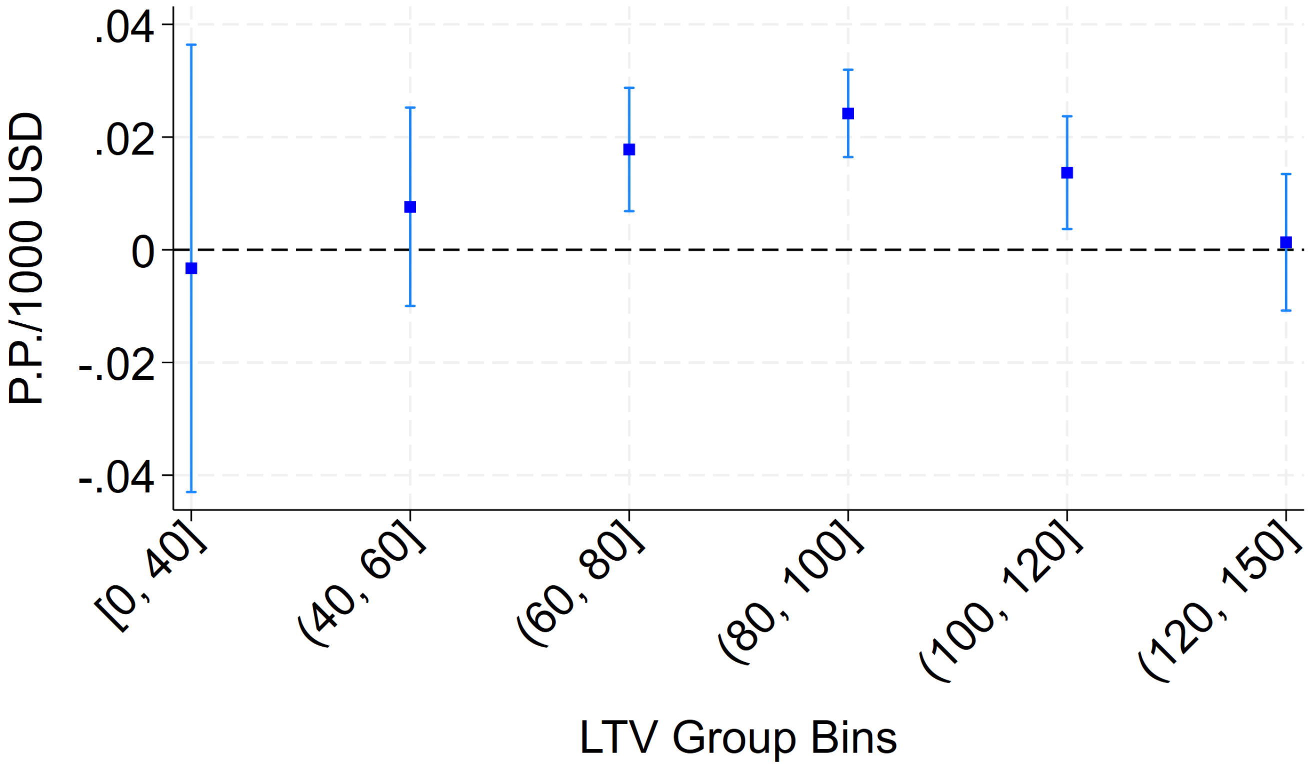


This figure estimates how the effect prepayment penalties have on mobility varies by the distance of the move according to the following specification:

$$Move_{b,t+k} - Move_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. In each panel, the move indicator is only for moves when the first n digits of a zipcode changes. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A8: Heterogeneity: LTV Ratios, Rescaled

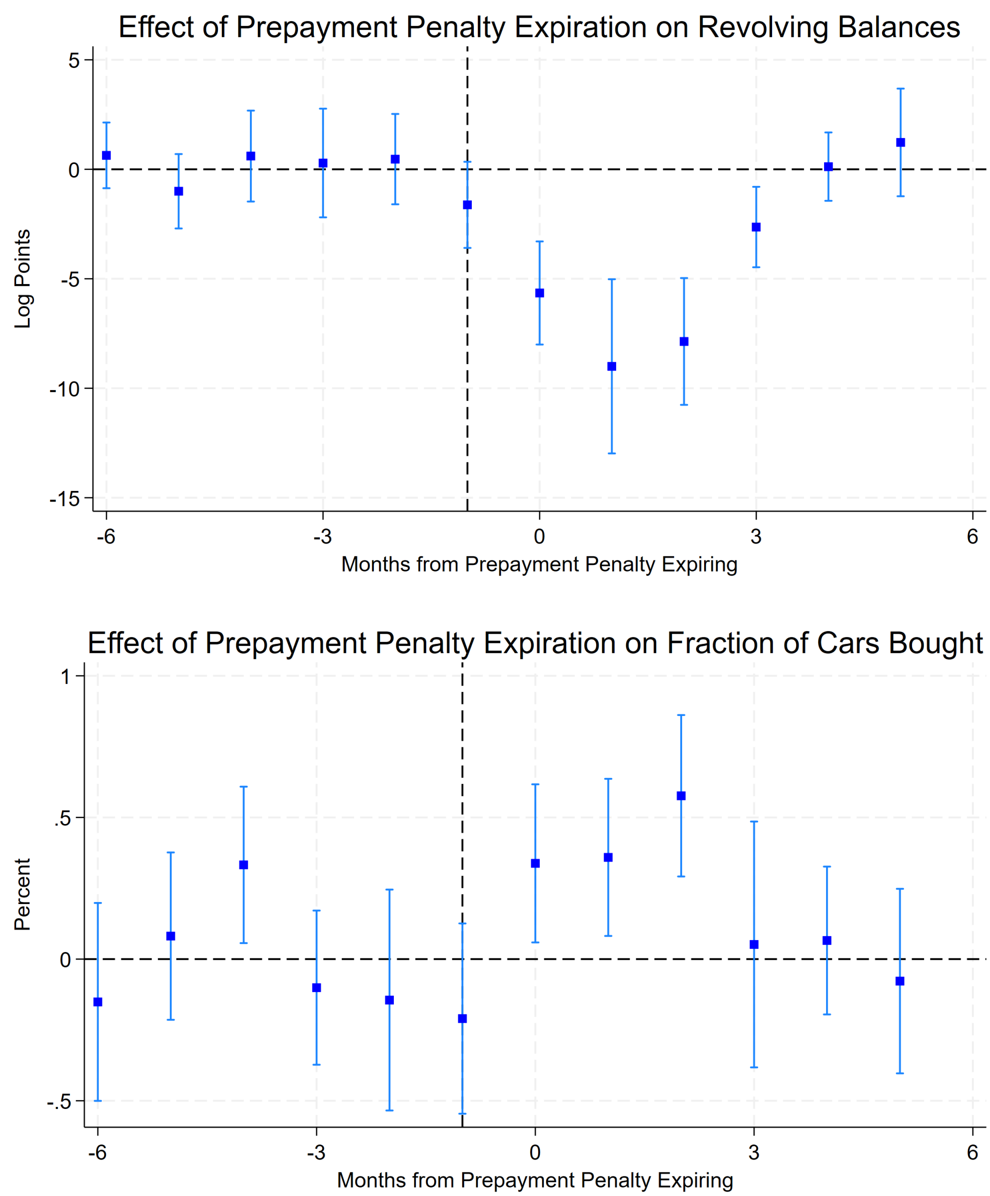


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $\overline{Move}_{b,post}$ is a dummy indicator for a borrower b moving in the year after expiration, rescaled by the prepayment penalty amount. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A9: Event Study: Debt Repayment and Auto Purchase

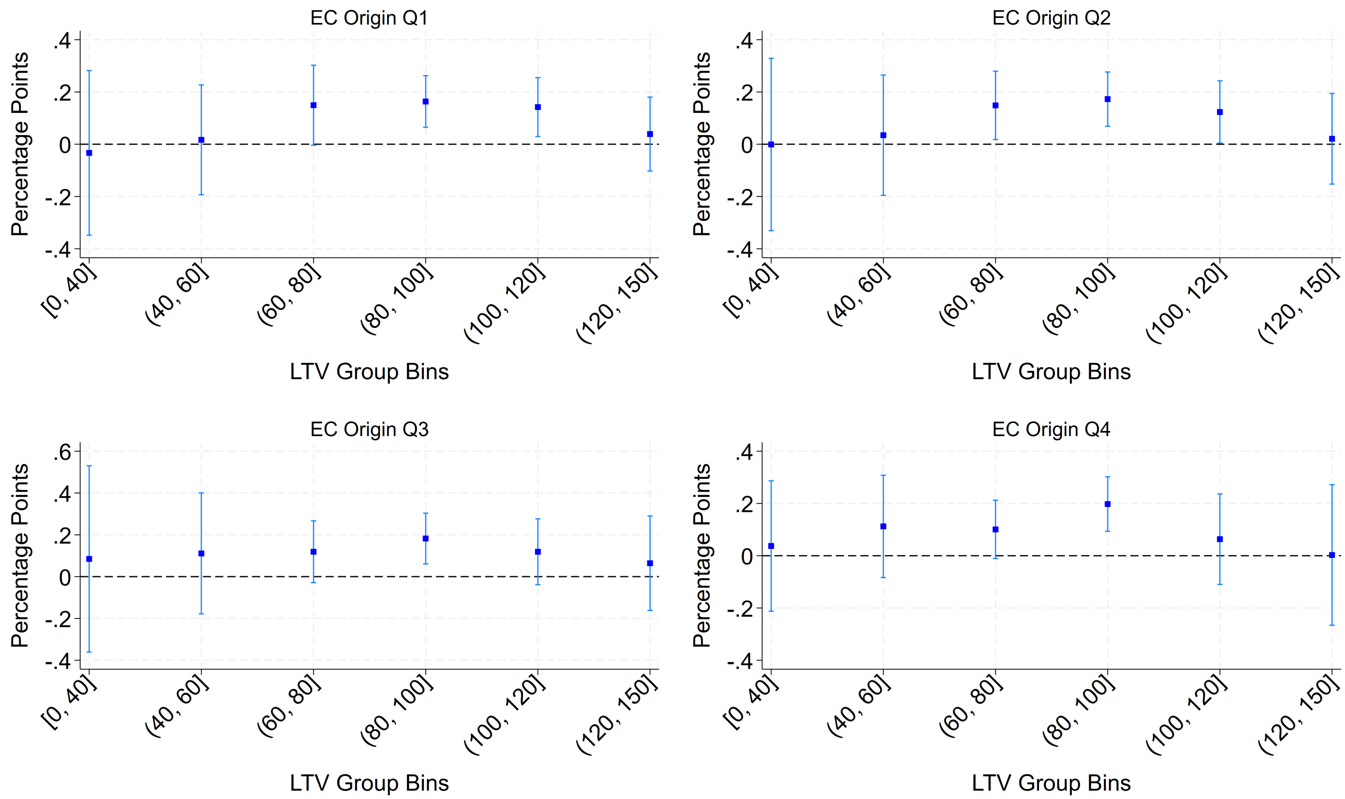


This figure shows how debt repayment and spending behavior changes around prepayment penalties expiring according to the following specification:

$$Y_{b,t+k+2} - Y_{b,t+k-1} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

The top panel shows the 3 month change in revolving balances, defined as the total balance on home equity lines of credit (HELOCs) and credit cards. The bottom panle shows the fraction of car puchases, as measured by a \$2000 increase in automobile balances that month.

Figure A10: Heterogeneity: LTV Ratios and Zip-Economic Connectedness Origin

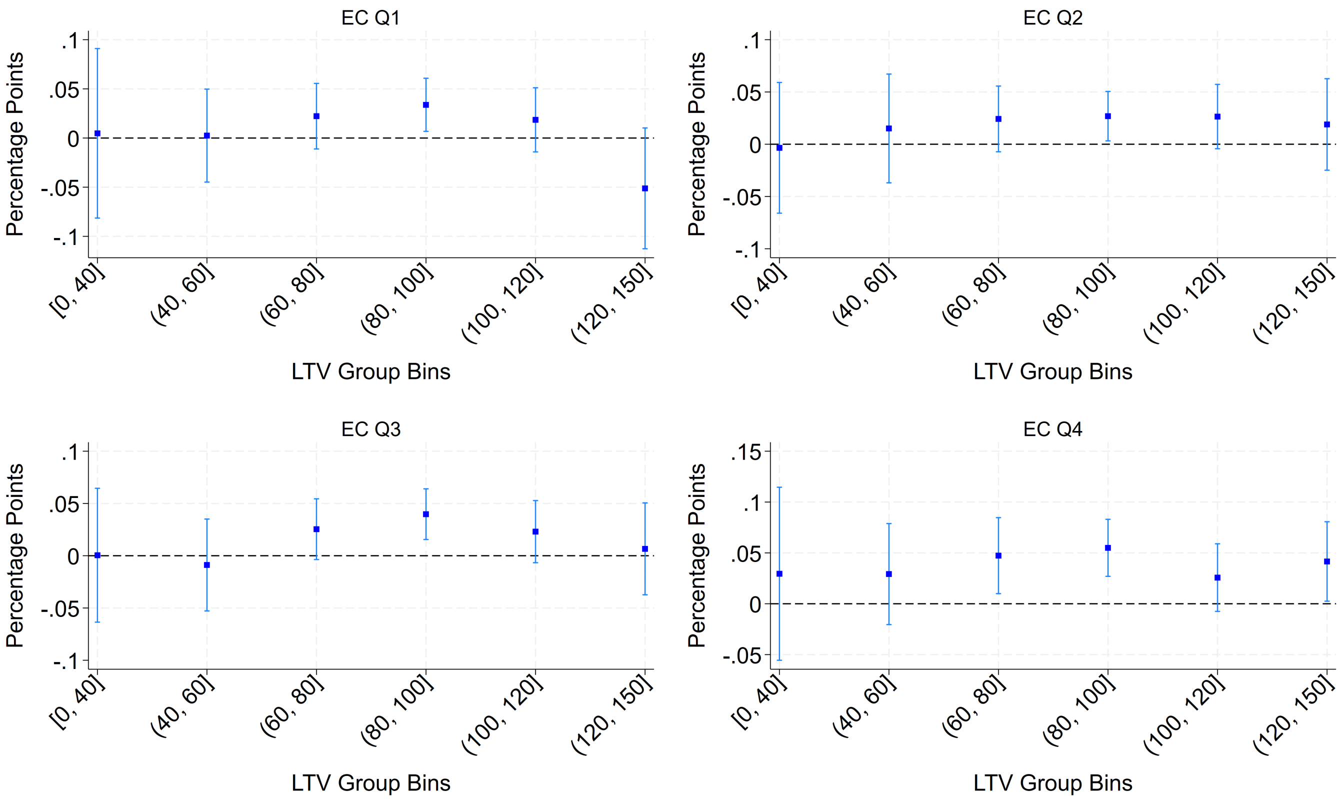


This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Figure A11: Heterogeneity: LTV Ratios and Zip-Economic Connectedness Destination



This figure estimates how the effect prepayment penalties have on mobility varies by LTV ratios according to the following specification:

$$\overline{Move}_{b,post} - \overline{Move}_{b,pre} = \beta \Delta PenaltyExpired_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,t+k}$ is a dummy indicator for a borrower b moving at month $t+k$. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Table A1: Summary Statistics: Performance

	Prepayment Penalty Term Length			
	12	24	36	60
	mean	mean	mean	mean
Prepaid	1.83	2.52	1.38	0.81
Defaulted	0.93	1.60	1.12	0.94
Moved	0.34	0.31	0.24	0.19
Purchased a Car	2.41	2.84	2.37	2.39
3 Month Pct. Change in Revolving Credit	12.20	7.71	5.74	6.66
Observations	2581577	12091933	16550108	2216675

This table shows summary statistics of the mortgage records over the life of the loan. Prepaid is an indicator for the month a loan is voluntarily paid off. Defaulted is the month a loan is first reported as 60 days past due. Moved is the month after the last month the original zipcode of the mortgaged property appears in Equifax records. Purchased a car is any month the automobile debt balance discreetly increases by at least $2000 dollars in Equifax. Revolving credit is the sum of balances on home equity lines of credit (HELOCs) and credit cards. Each column corresponds to one of the four prepayment penalty lengths used in the analysis.

Table A2: Zip-Level Variable Correlations

	Economic Connectedness	
Wages	0.53***	
	(0.03)	
Ln(Wages)		0.66***
		(0.01)
N	18909	18909

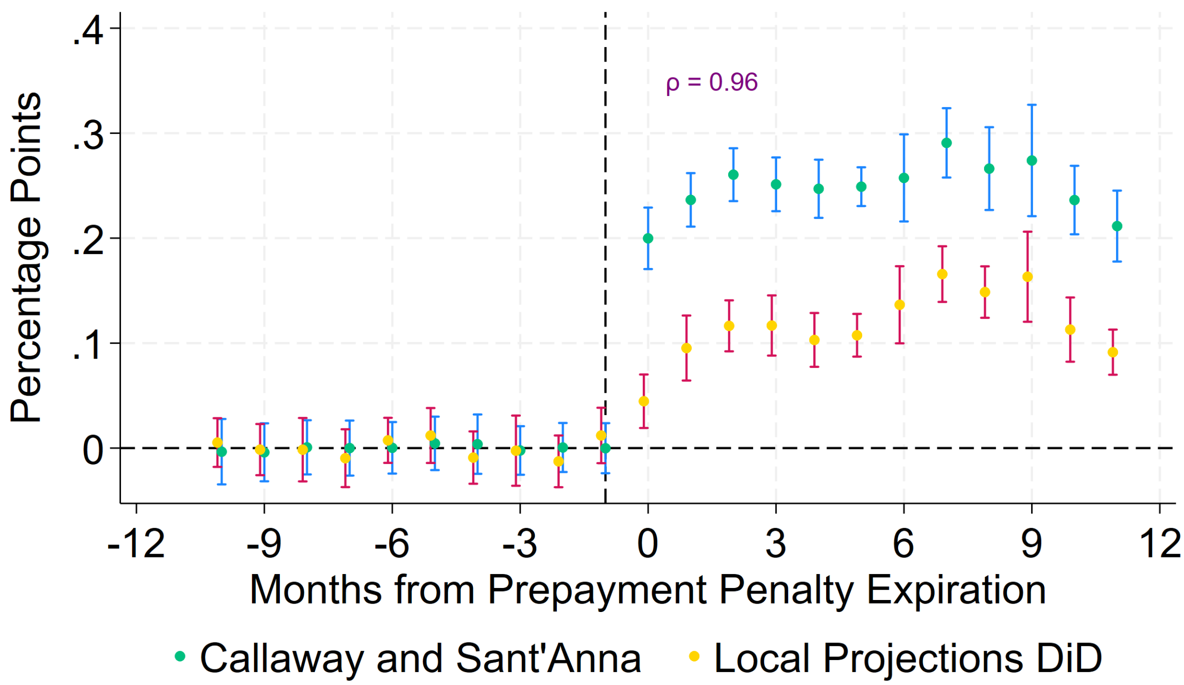
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table shows zip-level correlations of the main two variables used in the labor market results subsection of section 5. Economic connectedness is from (Chetty et al. (2022a)) and is a measure of the fraction of low-SES individuals interact with high-SES individuals, as proxied by friendships on Facebook. The zip-level wage data comes from tax returns and is published by the Internal Revenue Service (IRS) Statistics of Income (SOI) division. Variables have been demeaned and scaled by their standard deviation so the coefficients are the Pearson correlation coefficients of the two variables. Standard errors are robust and in parentheses.

Appendix B Comparing Difference-in-Differences Estimators

Figure B1: Dynamic Difference-in-Differences: LP-DiD versus Other Estimators



This figure shows how LP-DiD compares to other common estimators that solve the issues with TWFE.

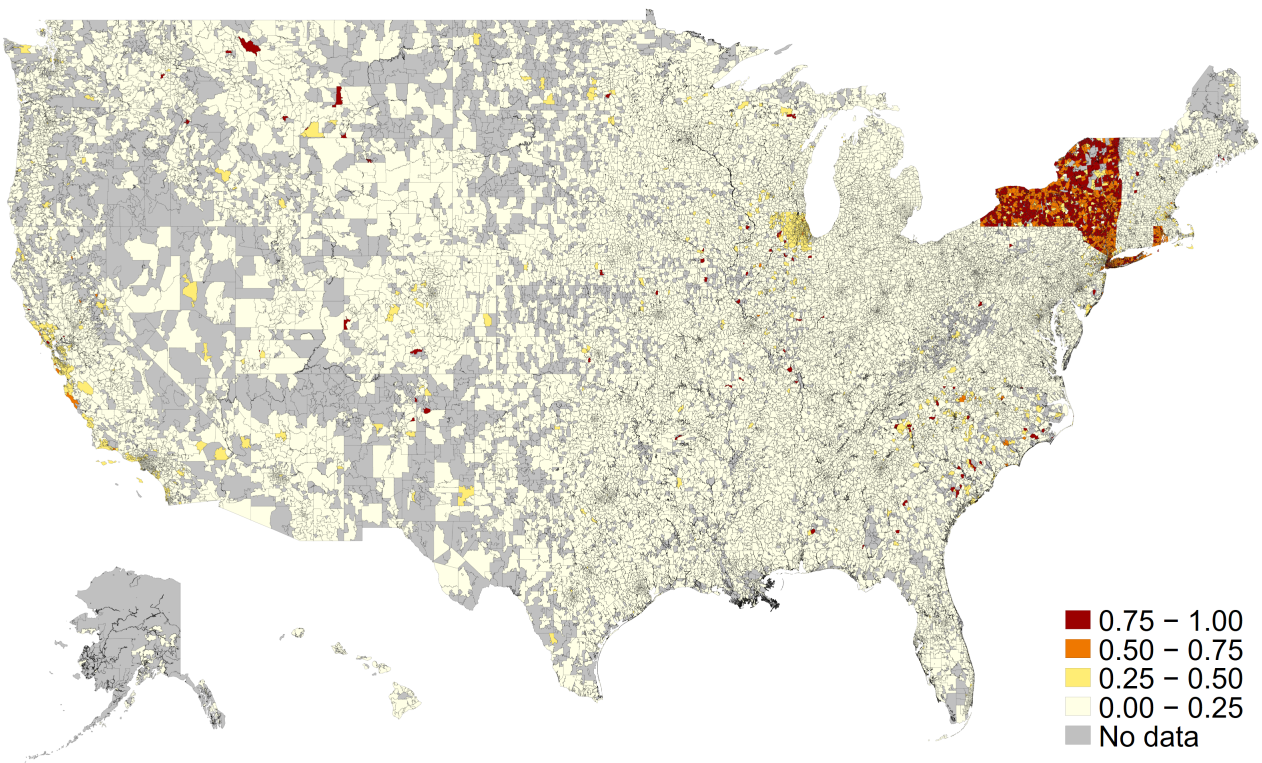
Appendix C New York State Border Discontinuity Design

Figure C1: New York Legislation Banning any Prepayment Penalties of Length Greater than One Year

b. notwithstanding any other provision of law, the unpaid balance of the loan or forbearance may be prepaid, in whole or in part, at any time. If prepayment is made on or after one year from the date the loan or forbearance is made, no penalty may be imposed. If prepayment is made prior to such time, no penalty may be imposed unless provision therefor is expressly made in the loan contract, provided that no penalty may be imposed if prohibited by sections six-l and six-m of the banking law. In all cases, the right of prepayment shall be stated in the instrument evidencing the loan or forbearance, provided, however, that the provisions of this subdivision shall not apply to the extent such provisions are inconsistent with any federal law or regulation.

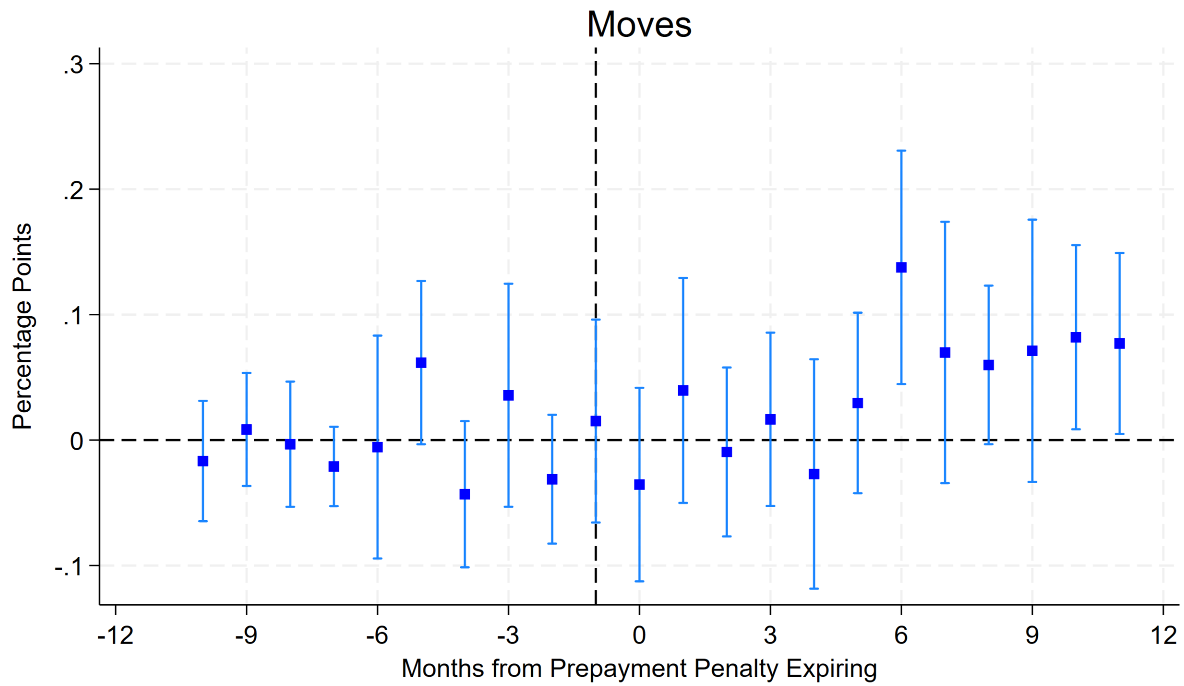
This figure shows the piece of New York State banking legislation that rules out the implementation of prepayment penalties longer than one year for mortgages originated in state.

Figure C2: Share of Mortgages with 1 Year Prepayment Penalty by Zipcode



This figure shows a zip-level map of the fraction of mortgages originated with prepayment penalties that were specifically a 1 year prepayment penalty.

Figure C3: New York Legislation Banning any Prepayment Penalties of Length Greater than One Year



This figure shows the mobility rates around 1 year after a mortgage is originated for mortgages originated in and around New York state.

Table C1: Summary Statistics: New York State Border Discontinuity

	Nearby NY mean	NY mean
Predicted Income (Thous. USD)	39.18	42.87
Equifax Vantage 2 Credit Score	649.38	659.17
FICO Credit Score	622.27	638.85
Mortgage Balance (Thous. USD)	192.88	331.86
Combined LTV (%)	82.42	77.45
Interest Rate (%)	7.17	6.47
MPP1 (%)	0.07	0.77
Observations	63734	37474

This table shows summary statistics of the mortgage records at origination. Predicted income is provided by Equifax and is derived by a proprietary algorithm using their credit records to best predict a borrower's income. Vantage 2 and FICO credit scores come from Equifax and Moody's Analytics, respectively. CLTV is the combined loan-to-value ratio at origination. Each column corresponds to one of the four prepayment penalty lengths used in the analysis.

Table C2: NY State Border Discontinuity IV Design

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.05*			0.06*
	(0.02)			(0.03)
NY Instrument		0.75***	0.04*	
		(0.07)	(0.02)	
N (millions)	2.146	2.146	2.146	2.146

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table estimates the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,post} - Move_{b,pre} = \beta \Delta \widehat{PenaltyExpired}_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

$$\Delta PenaltyExpired_{b,t} = \gamma \Delta NewYork_{b,t} + \alpha_{l(b),t} + \alpha_{c(b),t} + \varepsilon_{b,t}$$

where $Move_{b,post}$ is a dummy indicator for a borrower b moving a year after the prepayment penalty expires and $Move_{b,post}$ is a dummy indicator for a borrower b moving a year before the prepayment penalty expires. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The instrument $\Delta NewYorkb, t$ equals one when a mortgage originated in New York state reaches the age of 1 year. The fixed effects are lender-month ($\alpha_{l(b),t}$) and month of origination-month ($\alpha_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

Appendix D How Credit Constraints Affect Mobility: Stein (1995) Model

To see how my results are consistent with credit constraints impacting mobility, this section outlines the Stein (1995) housing model with down-payment constraints to understand the mobility response to prepayment penalties I find by LTV ratios. I keep everything largely the same but incorporate a prepayment penalty by adding a fixed cost q charged when paying off your mortgage early in the case of a move.

D.1 Definitions

Let

- U_i be utility for agent i.
- H_i, C_i be housing and non-housing consumption for agent i.
- M_i be a dummy variable for whether agent i moves. θ is then the discrete utility gain from moving.
- K_i be the debt agent i has. Assume $K_i's$ are evenly distributed along $[K_L, K_H]$.
- q is the prepayment penalty: extra payment you have to make to move/terminate mortgage.
- P is the price of a housing unit (fixed for simplicity and unnecessary for my point, not looking at price effects).
- $0 < \gamma < 1$ is a down-payment constraint. Must put at least a fraction γ of own funds to buy new home.

D.2 Model

From Stein (1995), the household problem is

$$\max_{H_i, C_i, M_i} U_i = \alpha \ln H_i + (1-\alpha) \ln C_i + \theta M_i$$

subject to a budget constraint

$$PH_i + C_i + qM_i \leq 1 + P$$

and, in the case of changing housing consumption (moving), a down-payment constraint

$$\gamma P H_i \leq (P - K_i - q) \iff H_i \leq \frac{P - K_i - q}{\gamma P}.$$

Finally, it's assumed everyone is endowed with one unit of housing and, at the end of the period, households receive labor income (so before they make the move decision) of $1 + K_i$. Thus, those with higher debt have lifetime wealth more backloaded. However, in lifetime net wealth, everyone has $1 + K_i + P - K_i - qM_i = 1 + P - qM_i$.

D.2.1 No Down-Payment Constraint: $\gamma \to 0$.

Since there is no constraint and households have log utility, optimal housing is

$$H_i = \alpha \frac{1 + P - q}{P}.$$

Here we see q will reduce housing consumption, but households still find it optimal to move (in this toy model H_i can only change by moving).

Essentially, I am assuming here when borrowers are unconstrained, even at a cost q the move is worth the discrete utility gain θ. Can think of q being relatively "small" relative to the value of the house for these high wealth individuals.

D.2.2 Down-Payment Constraint: $\gamma > 0$.

Now there will be three types of borrowers: unconstrained movers, constrained movers, and nonmovers.

1. Unconstrained movers have the lowest level of debt range $[K_L, K^*]$. In this range, borrowers always move and consume H_i equal to the no down-payment constraint scenario

above.

2. There is then constrained movers, who have an intermediate debt range $[K^*, K^{**}]$. These are borrowers who move but face the down-payment constraint. The indifference between being an unconstrained mover and constrained mover is defined by K^* as

$$\alpha\frac{1+P-q}{P} = \frac{P-K^*-q}{\gamma P} \iff K^* = P-q-\alpha\gamma\frac{P(1-P-q)}{P}$$

Any constrained mover will then consumer housing units H_i^c which is

$$H_i^c = \frac{P-K_i-q}{\gamma P}.$$

3. Lastly, there are nonmovers, who have the largest debt range $[K^{**}, K_H]$. K^{**} denotes the debt amount that makes a borrower indifferent between being a constrained mover and nonmover. This is implicitly defined by the K^{**} that solves

$$H^{**} = \frac{P-K^{**}-q}{\gamma P}.$$

and

$$\alpha \ln H^{**} + (1-\alpha)\ln(1+P-q-PH^{**}) + \theta = 0.$$

As the name implies, any borrowers with debt in this range will not move.

D.3 How This Ties to My LTV Result

It is these three groups of borrowers that drive my results:

- Unconstrained movers are the very low LTV borrowers. If $P >> q$, optimal housing consumption and moving will not be affected much by prepayment penalty q, which is what we see.

- For constrained movers, if $P - K_i$ is close to q, optimal constrained housing consumption H_i^c can change a lot when q goes to 0, and hence we see more moving and more housing consumption. This corresponds to those borrowers I see with intermediate LTV ratios that do respond to prepayment penalties.

- Lastly, the nonmovers are those borrowers who are so constrained that they don't move at all. It is precisely these super high LTV borrowers where we'd expect the prepayment penalty to no longer be marginal and hence we wouldn't see a response to the prepayment penalty expiring.

This theory gives empirical predictions consistent with what I find in the data. The response to the prepayment penalty q should be largest for constrained movers, which in the data is proxied by borrowers with high LTV ratios, but still some positive equity in their house.

Another way of showing this is simply in math. Let H^U be housing for unconstrained movers, H^{CM} for constrained movers, and H^{CN} for constrained non movers. Then we have

$$
\begin{aligned}
|P\frac{\partial H^{UM}}{\partial q}| &= \alpha < 1 \\
|P\frac{\partial H^{CM}}{\partial q}| &= \frac{1}{\gamma} > 1 \\
|P\frac{\partial H^{CN}}{\partial q}| &= 0.
\end{aligned}
$$

Appendix E MBS IV vs OLS Month by Month

Given my event studies are run as cross-sectional regressions and I must estimate the pooled estimation using *suest* in Stata, comparing the ratio of the pooled estimates of the reduced form estimates and the ratio of the first stage estimates (pooling the reduced form and first stage estimates first, then taking the ratio of the pooled averages) will not necessarily equal the pooled estimates of the 2SLS estimates (taking the ratio of the reduced form and first stage coefficients month by month, then pooling the ratios). The latter will give the correct results, and is what is done in Table 8 columns (1) and (4). However, to transparently show that indeed in all my regressions $\frac{\text{Reduced Form}}{\text{First Stage}} = \text{IV}$, this section of the appendix shows the non pooled estimates, comparing OLS to IV and showing the relevant first stage and reduced form estimates that give you the pooled results in table 8.

E.1 Post-Expiration Coefficients

Table E1: MBS Instrumental Variables Design: 0 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.15***			0.12***
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.05**	
		(0.03)	(0.01)	
R-Squared	0.003	0.580	0.003	0.000
F-Statistic	99.79	174.16	10.19	13.82
N	31144442	31144442	31144442	31144442

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E2: MBS Instrumental Variables Design: 1 Month After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.21***			0.26***
	(0.02)			(0.05)
MBS Predicted Penalty Expired		0.39***	0.10***	
		(0.03)	(0.02)	
R-Squared	0.003	0.577	0.003	0.000
F-Statistic	178.38	173.43	36.91	26.65
N	30481995	30481995	30481995	30481995

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E3: MBS Instrumental Variables Design: 2 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.23***			0.27***
	(0.01)			(0.05)
MBS Predicted Penalty Expired		0.38***	0.10***	
		(0.03)	(0.02)	
R-Squared	0.003	0.575	0.003	0.000
F-Statistic	243.05	173.71	18.71	28.96
N	29798406	29798406	29798406	29798406

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E4: MBS Instrumental Variables Design: 3 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.23***			0.27***
	(0.01)			(0.04)
MBS Predicted Penalty Expired		0.38***	0.10***	
		(0.03)	(0.02)	
R-Squared	0.003	0.574	0.003	0.000
F-Statistic	281.89	173.73	44.36	46.83
N	29094588	29094588	29094588	29094588

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E5: MBS Instrumental Variables Design: 4 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.21***			0.19***
	(0.02)			(0.05)
MBS Predicted Penalty Expired		0.38***	0.07***	
		(0.03)	(0.02)	
R-Squared	0.003	0.572	0.003	0.000
F-Statistic	144.77	173.60	14.43	14.42
N	28371415	28371415	28371415	28371415

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E6: MBS Instrumental Variables Design: 5 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.21***			0.23***
	(0.01)			(0.04)
MBS Predicted Penalty Expired		0.38***	0.09***	
		(0.03)	(0.01)	
R-Squared	0.003	0.571	0.003	0.000
F-Statistic	402.18	173.67	62.38	35.99
N	27630342	27630342	27630342	27630342

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E7: MBS Instrumental Variables Design: 6 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.25***			0.29***
	(0.02)			(0.04)
MBS Predicted Penalty Expired		0.38***	0.11***	
		(0.03)	(0.01)	
R-Squared	0.003	0.569	0.003	0.000
F-Statistic	107.81	172.84	58.13	49.05
N	26876463	26876463	26876463	26876463

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E8: MBS Instrumental Variables Design: 7 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.29***			0.30***
	(0.01)			(0.05)
MBS Predicted Penalty Expired		0.38***	0.11***	
		(0.03)	(0.02)	
R-Squared	0.003	0.566	0.003	0.000
F-Statistic	532.55	170.71	40.34	43.63
N	26099618	26099618	26099618	26099618

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E9: MBS Instrumental Variables Design: 8 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.27^{***}			0.32^{***}
	(0.02)			(0.05)
MBS Predicted Penalty Expired		0.37^{***}	0.12^{***}	
		(0.03)	(0.02)	
R-Squared	0.003	0.557	0.003	0.000
F-Statistic	308.82	174.33	30.76	47.33
N	25277258	25277258	25277258	25277258

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E10: MBS Instrumental Variables Design: 9 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.29^{***}			0.34^{***}
	(0.03)			(0.05)
MBS Predicted Penalty Expired		0.35^{***}	0.12^{***}	
		(0.03)	(0.03)	
R-Squared	0.003	0.545	0.003	0.000
F-Statistic	123.53	166.45	23.07	39.51
N	24406610	24406610	24406610	24406610

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E11: MBS Instrumental Variables Design: 10 Months After

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.23^{***}			0.21^{***}
	(0.02)			(0.04)
MBS Predicted Penalty Expired		0.34^{***}	0.07^{***}	
		(0.03)	(0.01)	
R-Squared	0.003	0.537	0.003	0.000
F-Statistic	154.96	151.37	22.17	25.23
N	23505658	23505658	23505658	23505658

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E12: MBS Instrumental Variables Design: 11 Months After

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.20***			0.24***
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.32***	0.08***	
		(0.03)	(0.01)	
R-Squared	0.003	0.532	0.003	0.000
F-Statistic	252.09	140.50	43.94	49.29
N	22591253	22591253	22591253	22591253

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These tables estimate the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,t+k} - Move_{b,k-1} = \beta^k \Delta \widehat{PenaltyExpired}_{b,t} + \alpha^k_{l(b),t} + \alpha^k_{c(b),t} + \varepsilon^k_{b,t}$$

$$\Delta PenaltyExpired_{b,t} = \gamma \Delta MBSPredPenaltyExpired_{d(b),t} + \alpha^k_{l(b),t} + \alpha^k_{c(b),t} + \nu^k_{b,t}$$

where $Move_{b,t}$ is a dummy indicator for a borrower b having moved as of month t. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The instrument $\Delta MBSPredPenaltyExpired_{d(b),t}$ equals one in the month the most common penalty in borrower b's MBS pool $d(b)$ expires. The fixed effects are lender-month ($\alpha^k_{l(b),t}$) and month of origination-month ($\alpha^k_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.

E.2 Pre-Expiration Coefficients

Table E13: MBS Instrumental Variables Design: 1 Month Before

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.10***			0.12***
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.04***	
		(0.03)	(0.01)	
R-Squared	0.004	0.582	0.003	0.000
F-Statistic	103.82	175.31	11.04	12.61
N	30384272	30384272	30384272	30384272

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E14: MBS Instrumental Variables Design: 2 Months Before

	(1) OLS	(2) FS	(3) RF	(4) IV
Penalty Expired	0.07***			0.06*
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.02*	
		(0.03)	(0.01)	
R-Squared	0.004	0.584	0.004	0.000
F-Statistic	58.82	176.89	4.34	4.06
N	29598180	29598180	29598180	29598180

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E15: MBS Instrumental Variables Design: 3 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.08***			0.08
	(0.01)			(0.05)
MBS Predicted Penalty Expired		0.39***	0.03	
		(0.03)	(0.02)	
R-Squared	0.004	0.587	0.004	0.000
F-Statistic	48.49	177.84	2.50	2.32
N	28784793	28784793	28784793	28784793

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E16: MBS Instrumental Variables Design: 4 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.08***			0.09***
	(0.01)			(0.02)
MBS Predicted Penalty Expired		0.39***	0.03***	
		(0.03)	(0.01)	
R-Squared	0.004	0.591	0.004	0.000
F-Statistic	142.25	178.59	17.15	18.35
N	27942380	27942380	27942380	27942380

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E17: MBS Instrumental Variables Design: 5 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.10***			0.07*
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.03*	
		(0.03)	(0.01)	
R-Squared	0.004	0.594	0.004	0.000
F-Statistic	117.75	180.94	5.82	5.93
N	27068903	27068903	27068903	27068903

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E18: MBS Instrumental Variables Design: 6 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.10***			0.04
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.02	
		(0.03)	(0.01)	
R-Squared	0.004	0.596	0.004	0.000
F-Statistic	91.01	182.70	2.11	2.27
N	26164466	26164466	26164466	26164466

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E19: MBS Instrumental Variables Design: 7 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.08***			0.06*
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.02	
		(0.03)	(0.01)	
R-Squared	0.004	0.599	0.004	0.000
F-Statistic	40.45	183.42	3.32	4.04
N	25229317	25229317	25229317	25229317

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E20: MBS Instrumental Variables Design: 8 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.09***			0.08**
	(0.01)			(0.03)
MBS Predicted Penalty Expired		0.39***	0.03**	
		(0.03)	(0.01)	
R-Squared	0.004	0.601	0.004	0.000
F-Statistic	61.20	182.56	6.92	7.18
N	24264582	24264582	24264582	24264582

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E21: MBS Instrumental Variables Design: 9 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.09***			0.06**
	(0.02)			(0.02)
MBS Predicted Penalty Expired		0.39***	0.02*	
		(0.03)	(0.01)	
R-Squared	0.004	0.604	0.004	0.000
F-Statistic	30.62	182.23	6.37	7.88
N	23270369	23270369	23270369	23270369

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E22: MBS Instrumental Variables Design: 10 Months Before

	(1)	(2)	(3)	(4)
	OLS	FS	RF	IV
Penalty Expired	0.10***			0.07**
	(0.01)			(0.02)
MBS Predicted Penalty Expired		0.39***	0.03**	
		(0.03)	(0.01)	
R-Squared	0.004	0.607	0.004	0.000
F-Statistic	73.87	181.98	8.03	8.63
N	22246726	22246726	22246726	22246726

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These tables estimate the effect prepayment penalties have on mobility according to the following specification:

$$Move_{b,t+k} - Move_{b,k-1} = \beta^k \Delta Pen\widehat{altyExp}ired_{b,t} + \alpha^k_{l(b),t} + \alpha^k_{c(b),t} + \varepsilon^k_{b,t}$$

$$\Delta PenaltyExpired_{b,t} = \gamma \Delta MBSPredPenaltyExpired_{d(b),t} + \alpha^k_{l(b),t} + \alpha^k_{c(b),t} + \nu^k_{b,t}$$

where $Move_{b,t}$ is a dummy indicator for a borrower b having moved as of month t. The treatment indicator $\Delta PenaltyExpired_{b,t}$ equals one in the month that the penalty expires. The instrument $\Delta MBSPredPenaltyExpired_{d(b),t}$ equals one in the month the most common penalty in borrower b's MBS pool $d(b)$ expires. The fixed effects are lender-month ($\alpha^k_{l(b),t}$) and month of origination-month ($\alpha^k_{c(b),t}$). Standard errors are clustered by lender and reported in the parentheses.