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DOES HOMEOWNERSHIP MATTER?
THE LONG-TERM CONSEQUENCES OF LOSING
A HOUSE DURING THE GREAT RECESSION

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Does Homeownership Matter? The Long-Term Consequences of Losing a House during the Great Recession

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ABSTRACT

We examine the long-term impact of owning versus renting a home in the United States. Our research design compares two groups of homeowners who experienced similar negative income shocks during the Great Recession but had different probabilities of retaining their homes due to mortgage modifications. While the two groups exhibit nearly identical pre-trends, they diverge by 36 percentage points in home retention. More than half of this disparity persists nearly a decade later, translating into average housing capital gains of \$83,000. However, homeownership and housing wealth did not affect creditworthiness, consumption, or neighborhood quality, partly due to tightening credit constraints.

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1 Introduction

The U.S. government has long prioritized expanding homeownership, allocating approximately \$400 billion in annual subsidies to advance this objective.¹ Beyond its potential broader social benefits, homeownership is widely regarded as a key pathway to individual wealth accumulation and improved access to credit (Goodman & Mayer 2018).² However, homeowners are vulnerable to employment and income shocks that, even when temporary, can jeopardize their ability to meet mortgage obligations, leading to delinquency and, ultimately, the loss of their home through a distressed sale or foreclosure.³ Although this short-term downside risk of homeownership is well recognized, it is much less obvious how much becoming a renter versus remaining a homeowner matters in the long-run.

This paper provides the first estimates of the long-term impact of homeownership in the United States. Specifically, we examine how home loss in the aftermath of the Great Recession influences the long-run trajectory of homeownership, housing wealth, financial well-being, credit availability, consumption, and neighborhood choice. There are two critical challenges to studying this aspect of changes in homeownership. First, obtaining a comprehensive longitudinal dataset that captures income shocks, homeownership status, and related financial indicators for a large population is inherently difficult. We address this challenge by utilizing a unique dataset of 380,000 individuals from seven major U.S. markets who became homeowners or refinanced their mortgages between 2004 and 2008 (Bayer, Ferreira, & Ross 2016, 2018). We obtained their credit reports at three-year intervals from June 2004 to June 2022, enabling us to track changes in homeownership, delinquency, and foreclosure status over time, along with various financial and credit-related indicators.

Second, significant selection concerns arise: households facing negative income shocks may differ systematically from those with stable incomes, and furthermore, the ability to sustain homeownership in the aftermath of such shocks is often contingent on access to liquidity. As a result, retaining homeownership is likely correlated with a wide range of economic and financial factors that also

¹See FY2025 10-year projected estimates from the Federal Government Office of Management and Budget. The main expenses include deductibility of mortgage interest and local property taxes on owner-occupied homes, capital gains exclusion on home sales, and exclusion of net imputed rental income. Those estimates can be even higher after accounting for subsidies from government-backed mortgage institutions, such as Freddie Mac and Fannie Mae, and from other mortgage programs from FHA and Ginnie Mae. Bishop et al. (2025) present recent calculations of the tax subsidy for individual homeowners in the US.

²Wolff (2025) recently showed that, for households in the middle three quintiles of the wealth distribution, housing corresponds to 64% of their total wealth. Next comes pension accounts (15%), liquid assets and business equity (both with 8%), and then stocks and financial securities (4%).

³Those shocks include economic recessions (Hoynes, Miller, & Schaller 2012), firm closures and mass layoffs (Davis & Von Wachter 2011), technological advancements (Acemoglu & Restrepo 2020), trade shocks (Autor, Dorn, & Hanson 2013), and health crises (Lenhart 2019) among others.

shape long-term outcomes. We tackle the selection issue by implementing a novel research design that compares two groups of homeowners who experienced similar negative income shocks but had different probabilities of retaining their homes due to mortgage modifications. This approach leverages three key stylized facts about the Great Recession and its foreclosure crisis. First, nearly all mortgage defaults during the Great Recession were driven by temporary income shocks (Ganong & Noel 2023). Second, mortgage servicers, while having a financial incentive to grant modifications to borrowers with stronger credit profiles, lacked the capacity to effectively evaluate and process the overwhelming volume of delinquent households in the aftermath of the Great Recession, leading to a chaotic modification approval process (Agarwal et al. 2011a; Cordell et al. 2008; Ross & Wang 2021). Indeed, as we show below, based on all available observable variables, the ability for severely delinquent borrowers to get a modification during this period appears to have been close to random. Third, mortgage modifications significantly reduced the short-term likelihood of home loss among financially distressed households during the crisis (Agarwal et al. 2017; Ganong & Noel 2020).

Our analysis focuses on mortgage borrowers who became 90 days delinquent at any point in the two years before June 2010, representing 18% of our sample. Among these severely distressed borrowers, we compare outcomes between those who received a mortgage modification within the next three years (24% of the group) and those who did not. By conditioning on a 90-day delinquency, our analysis isolates the effect of losing one’s home from the underlying income or life shock that triggered the delinquency. This approach contrasts with prior studies of foreclosure’s short- and medium-run effects (Brevoort & Cooper 2013; Molloy & Shan 2013; Piskorski & Seru 2021) which compare foreclosed borrowers to a broader set of non-foreclosed homeowners, thereby potentially combining the impact of the foreclosure itself with any shock leading up to it.⁴

Given our sample of recent mortgage borrowers, we measure homeownership as the borrower having an open mortgage on their credit report. We find large effects on short-term homeownership. By June 2013, homeownership rates fell by 51 percentage points in the control group but only 15 percentage points in the treatment group, implying that modifications preserved ownership for 36% of distressed borrowers. This difference reflects a mix of foreclosures and home sales, including short sales in which lenders absorbed losses due to negative equity. Because mortgage modifications often occur before formal foreclosure proceedings begin, our estimates capture the effect of a potentially early intervention in the mortgage default process, which typically involves several formal steps and

⁴Several studies have also examined the impact of foreclosures on a wider set of non-financial outcomes including education (Been et al. 2024, 2011b) and health (Currie & Tekin 2015; Downing 2016; Downing et al. 2017)

takes considerable time (Chan et al. 2014; Gerardia, Lambie-Hansonb, & Willen 2013). Consistent with this, treated borrowers were 19 percentage points less likely to receive a notice of foreclosure and 9.5 percentage points less likely to enter foreclosure proceedings. These estimates suggest that about half of the short-run homeownership effect arises from sales prior to the credit agency recording a foreclosure notice, one-quarter from sales prior to the beginning of a formal foreclosure process, and the remainder from later stages of the foreclosure process. The final stages of foreclosures, such as court proceedings, property auction or Real Estate Owned (REO) property sale, therefore account for only a small share of the overall effect. In this way, our estimates may naturally differ from studies focused on the effects of a completed foreclosure, such as Diamond, Guren, & Tan (2025), who exploit variation in judicial leniency during court foreclosure proceedings.

We conduct a series of tests to validate our research design. First, we show that treatment and control groups display remarkably similar pre-trends in homeownership and similar levels and pre-trends on a broad set of credit, financial, and consumption outcomes. Notably, the significant credit and consumption declines resulting from the income shock are virtually identical for treatment and control. Second, we document that the homes initially owned by borrowers in both groups exhibit nearly identical patterns of past and future price appreciation, helping to rule out bank-driven considerations related to local housing market dynamics. Finally, we obtain similar instrumental variable estimates of the short-term effects of mortgage modification on homeownership using a national sample of Fannie Mae mortgages, where treatment is instrumented with variation in loan servicer modification rates in the spirit of Agarwal et al. (2017).

Given the substantial short-term impact of mortgage modifications on homeownership, we turn next to the centerpiece of the paper: an examination of the long-term impact of maintaining homeownership on a wide variety of financial and life outcomes. We begin by tracing the long-term effect on homeownership, finding that the substantial difference in homeownership rates in 2013 persists over time: in 2022, the treated group remains 19 percentage points more likely to be homeowners, more than half the size of the short-term effect.

Such persistence in the homeownership effect suggests the potential for considerable differential wealth accumulation between those who retained versus lost their homes. Indeed, our calculations indicate that, while those homeowners who did not receive a mortgage modification initially benefited from shedding negative home equity positions during the Great Recession, the modification group had caught up by the late 2010s and, by 2022, had accumulated an average of \$83,000 more in housing wealth, a sizable differential return on initial equity levels. The ability to observe in-

dividuals over the long term is crucial for this result: due to the path of U.S. house prices since 2010, meaningful wealth benefits to homeownership do not appear until years after the treatment. Nevertheless, in the long term, homeownership provides a substantial boost to housing wealth, the key driver of personal wealth for middle-income households (Wolff 2025). As the households in our sample age towards retirement, the treated group’s increased housing wealth is likely to grow in importance.

We next examine long-term impacts on credit, consumption and other outcomes. Credit scores in the treatment and control groups follow nearly identical paths over the subsequent decade: both experience a sharp initial decline in creditworthiness — reflecting the severe derogatory associated with a 90-day mortgage delinquency — followed by a gradual recovery. Notably, the presence of additional mortgage derogatories appears to have only a limited incremental effect on credit scores. This pattern suggests that formal foreclosure notices and proceedings impose relatively modest penalties beyond those already triggered by an initial serious delinquency. Consistent with these declines and tighter post-crisis credit markets, virtually no borrowers in either group opened new home equity lines of credit (HELOCs) during the 2010s. Consumption proxies — credit card balances, auto loan payments, and future delinquencies — also evolve similarly across groups. Given the large wealth differences that emerge by 2022, the absence of consumption effects likely reflects limited access to home equity borrowing, though other factors, such as the lasting behavioral effects of having experienced income shocks, home value declines, and foreclosure risk, may also contribute to the failure to spend out of home equity (Malmendier & Nagel 2011).⁵

We conclude by examining the long-term effects of maintaining homeownership on residential mobility and neighborhood quality. As expected, borrowers who did not receive a modification — and were therefore more likely to transition into renting — exhibit substantially higher mobility in every subsequent year. However, despite these differences in tenure, the treated and control groups ultimately reside in neighborhoods with strikingly similar characteristics, including average income, house prices, and renter share. Those neighborhoods are generally in the middle of the quality distribution, with a large share of renters, which seemingly explains the ability of homeowners and renters to remain in comparable neighborhoods. Thus, large disparities in homeownership status

⁵Several previous studies have found that rising home values stimulate consumption, although the evidence on the magnitude of these effects is somewhat mixed. For instance, Aladangady (2017), Graham & Makridis (2023), & Mian, Rao, & Sufi (2013) document large effects, whereas Browning, Gørtz, & Leth-Petersen (2013), Disney, Gathergood, & Henley (2010), & Guren et al. (2021) report more muted responses. In many of these studies, however, the relaxation of collateral constraints has been a key channel through which housing wealth influences consumption (DeFusco 2018; Sodini et al. 2023).

appear to have had little impact on long-run neighborhood resources for our sample.

Our research contributes to the literature in a number of ways, starting with providing causal effects of homeownership in the United States. Despite long-standing interest in the effects of homeownership, well-identified causal studies remain limited because quasi-experimental variation in ownership status is rare. Sodini et al. (2023), for example, provide evidence on the benefits of homeownership using a unique setting in which renters of public housing co-ops in Stockholm, Sweden, faced quasi-random changes in the ability to transition into homeownership.

At the same time, we complement work on the effects of homeownership, such as Sodini et al. (2023) and Hausman, Ramot-Nyska, & Zussman (2022), by evaluating home *loss* rather than home acquisition, and highlighting that the consequences of losing a home are not simply the inverse of acquiring one. While we do not directly examine homeownership’s impact on children (Haurin, Parcel, & Haurin 2002) or social capital (DiPasquale & Glaeser 1999), we find minimal effects on household consumption patterns that likely impact children such as neighborhood quality.

Finally, a large body of existing work evaluates the effects of foreclosure and income shocks in the context of the Great Recession. Our findings on homeownership and the implied effects on housing wealth are consistent with Piskorski & Seru (2021) who document the difficulty households face in re-establishing homeownership after the crisis, and also consistent with Molloy & Shan (2013) who find increased mobility, but minimal impact on neighborhood choice following a foreclosure notice. Our findings may appear inconsistent with Brevoort & Cooper (2013), who find negative effects of foreclosure on credit score. However, our entire sample has already experienced a major mortgage derogatory, so that we estimate the effects of losing a home on a different margin. We complement this literature by providing the first long-term estimates of effects beyond the recession and subsequent recovery. In so doing, we are the first to show the persistence of home loss over time, estimate large effects on housing wealth that only accumulate over the long term, but also document minimal long-term impacts in other domains like credit worthiness and consumption.

The rest of the paper proceeds as follows: Section 2 presents the context of the Great Recession, Section 3 describes in detail the theoretical and empirical effects of homeownership, Section 4 explains our data and research design, Section 5 shows the short-term effects of receiving a mortgage modification on homeownership, and Section 6 presents the long-run estimates of the impact of homeownership on housing wealth, credit, consumption, and neighborhood quality. Section 7 concludes the paper and discusses policy implications.

2 Background

Housing prices in US metropolitan areas entered a period of rapid appreciation in the late 1990s and early 2000s, until they reached a peak around 2006 (Ferreira & Gyourko 2023). Those changes were accompanied by a growing housing supply, increasing mortgage transactions, and an increasing share of mortgages originated by subprime lenders (Chan, Haughwout, & Tracy 2015; Mayer, Pence, & Sherlund 2009). By late 2006, housing prices had begun to fall and the subprime sector started to contract, followed by a dramatic increase in foreclosure notices. Then, as the financial crisis worsened and the Great Recession began, housing prices fell rapidly. By early 2009, BLS-reported unemployment rates soared to 10% and foreclosures more than doubled to 1.4% (Been et al. 2011a). Foreclosures remained at a high level throughout 2010 creating a large stock of seriously delinquent mortgages — in the 24 months preceding June 2010, as the Great Recession built, 16% of households with a GSE-conforming mortgage experienced a 90-day delinquency.⁶ Many studies have also documented that the beginning of the foreclosure crisis was worse for minority and low-income borrowers and the neighborhoods in which they resided, e.g. Bayer, Ferreira, & Ross (2014, 2016), Ghent, Hernández-Murillo, & Owyang (2014), and Chan et al. (2013).

The factors driving the increase in foreclosures during the Great Recession have been extensively discussed, including the decline in underwriting standards (Bhutta & Keys 2022; Keys et al. 2010; Mian & Sufi 2009), fraud (Jiang, Nelson, & Vytlačil 2014; LaCour-Little & Yang 2013), and housing price declines and negative equity (Ferreira & Gyourko 2015; Palmer Forthcoming). While many of the early explanations focused on the features of subprime loans, recent research points to temporary income shocks (Ganong & Noel 2020) as the main driver of the foreclosure crisis. Foote et al. (2008) and Campbell & Cocco (2015) note that even when a homeowner is in negative equity, the option value associated with potential future capital gains can be quite large, and households are unlikely to default on their mortgage unless they experience a significant income shock and face liquidity or borrowing constraints. Bhutta, Dokko, & Shan (2017) further suggest emotional attachment and various behavioral factors as another explanation for low rates of default. In this context, declining housing prices put households at risk of losing their home, but default was unlikely to occur unless the household experienced a significant income shock. Overall, Ganong & Noel (2020) conclude that 70 percent of foreclosures were driven solely by negative life events (i.e., cash-flow defaults), while 24 percent were driven by the interaction between negative life events and negative equity.

⁶These numbers and later Fannie Mae statistics cited in this section are based on Fannie Mae public loan performance data for mortgages issued between 2004 and 2008.

How did mortgage servicers respond to the crisis? Agarwal et al. (2011a) find that within six months of becoming seriously delinquent in 2008, more than half of mortgages had no recorded action related to either foreclosure or loss mitigation, which they attribute to the industry being overwhelmed by the number of problem mortgages and by the severe asymmetries of information faced during the loss mitigation process. Starting in 2006, the fraction of extremely delinquent borrowers with no foreclosure notice on their credit reports increased substantially (Ross & Wang 2021). Cordell et al. (2008) notes that considerable consolidation occurred during the run up to the housing crisis yielding labor savings from economies of scale. However, loss mitigation was labor intensive with minimal economies of scale compared to traditional servicing activities, and, as a result, servicers lacked both the staff and the technology to adequately support loan modification efforts.⁷ Not surprisingly, Agarwal et al. (2011b) and Reid, Urban, & Collins (2017) document substantial heterogeneity across servicers in whether modifications were even offered to borrowers.

The modification-related strain on servicers increased by at least an order of magnitude following the announcement of the Home Affordable Modification Program (HAMP). From early 2009 to mid-2010, the volume of new loan modifications among Fannie Mae mortgages increased twenty-fold. Agarwal et al. (2017) exploit variation in the pre-HAMP modification experience of loan servicers to estimate the effect of HAMP, concluding that HAMP led to 1 million additional modifications. This rapid expansion in modifications was managed by an industry that was already overwhelmed and under-resourced.

In sum, the Great Recession represents an economic episode in which a substantial share of U.S. homeowners faced temporary income shocks, had negative equity in their homes, and, in many cases, had limited savings or access to credit to support continued mortgage payments. In response to federal policies, a rapid expansion of mortgage modifications occurred in a moment when loan servicers had minimal experience and insufficient resources to manage these circumstances effectively, leading to review and decision processes that were often characterized as cursory and chaotic. Further, most homeowners themselves had no experience with, or even prior knowledge of, the potential for mortgage modifications. Consistent with this context, in the research design section we show that there are minimal observable differences in levels and trends between seriously delinquent mortgage borrowers who received and did not receive a mortgage modification.

⁷Loan modifications were uncommon before the start of the Great Recession, at less than 0.03% of all Fannie Mae mortgages, making their labor intensity less important pre-crisis.

3 Theoretical and Empirical Predictions

We now present a discussion of the theoretical and empirical predictions of the effects of homeownership on the key outcomes that we examine: housing wealth, credit scores, credit availability, consumption, mobility, and neighborhood quality. Several features of our setting are important to keep in mind as we present this discussion. In particular, our analysis starts with a set of homeowners who took out a new mortgage during the 2000s housing boom. The majority of these homeowners initially gained a significant amount of housing equity as house prices rose through the peak of the boom and then quickly fell to a position of substantial negative equity by 2010 as house prices collapsed.

Our research design, explained in detail in the next section, will focus on a subset of those homeowners who experienced a negative income shock during the Great Recession and became seriously delinquent on their mortgage by 2010. On average, these households resided in neighborhoods with roughly the national median level of neighborhood income and with relatively high shares of rental housing, and they were relatively marginal homeowners in the sense that their pre-crisis income and credit scores were lower than most homeowners. Many of these marginal homeowners presumably would not have been able to keep making their mortgage payments following the income shock, triggering severe delinquency, a foreclosure procedure, or a sale of the property. Among this subset of delinquent homeowners, we will estimate the causal effect of retaining homeownership by comparing homeowners who received a mortgage modification — our treatment group - versus homeowners who had similar negative income shocks (as demonstrated empirically below) but did not receive a mortgage modification — our control group.

We now discuss the potential effects of maintaining homeownership on each set of outcomes.

3.1 Effect of Homeownership on Housing Wealth

The vast majority of homeowners in our sample had negative equity by 2010. Therefore, the short-run effect of maintaining homeownership through a mortgage modification on housing wealth would typically have been quite negative. Households who sold their home through a short sale or lost their home through a foreclosure would have reverted to roughly zero home equity, while those receiving mortgage modifications would have carried their existing negative equity forward.

In the long run, the predicted effect of maintaining homeownership on housing wealth depends on several factors, including: (i) house price dynamics for households who kept their homes, (ii) the

difficulty of regaining homeownership for those who were unable to maintain it through a mortgage modification, and iii) differential mobility to neighborhoods with more or less potential for price appreciation.

During the 2010s and early 2020s, all three of these factors likely worked to the significant advantage of those who were able to stay in their homes through a modification. The Case-Shiller index indicates that house prices nationally increased by 55% from January 2007 to January 2022, providing a substantial return to homeowners on their highly leveraged assets. As prices increased, it automatically became harder for renters to save for the necessary down payment and to comply with minimum debt to income ratios in order to get a new mortgage (FannieMae 2025). Moreover, with serious mortgage derogatories on their credit report and the general tightening of access to mortgage credit in the 2010s, the scars from losing their homes in the early 2010s would likely have made it very difficult for many of these individuals to re-enter homeownership quickly. Finally, mobility to neighborhoods with potential growth might be facilitated by holding an asset that serves as an automatic hedge for price growth in other neighborhoods (Sinai & Souleles 2013). Thus, we expect those homeowners who were able to keep their homes due to a mortgage modification to have accumulated much more housing wealth over the study period.

We do not measure other forms of wealth, such as from investments in the stock market, but it is highly unlikely that renters could offset the potential housing wealth differential via alternative investments. First, housing is the main component of wealth for households in the middle of the income or wealth distribution. Second, rents also increased dramatically during the 2010s, reducing the ability to save.⁸ Moreover, renters would be unlikely to leverage any savings in the same way that homeowners leverage a house purchase with 80% LTV loans. Meanwhile, homeowners generally had mortgages with fixed nominal monthly payments plus the option of refinancing at even lower interest rates depending on market conditions (Bhutta & Keys 2016).

3.2 Effect of Homeownership on Credit Scores and Credit Availability

The effect on credit scores will likely depend on two key factors: the negative impact of having an extra derogatory in one's credit file and the flexibility that renters may have to manage other debt payments.

In gauging the likely impact of maintaining homeownership through a mortgage modification on

⁸We calculate ZIP-level percent changes in house values and rents using the 5-year ACS from 2007-2011 and 2018-2022. We find that in our sample areas, the mean percent change in home value is 49.3%, while the mean change in rent is 48.1%. The mean percent change in FHFA housing price index is 44.5%.

subsequent credit scores, it is critical to keep in mind that both the treatment and control households in our main analysis already have one or more serious mortgage derogatories on their credit report as of 2010, having already had many months of delinquent mortgage payments. Subsequent foreclosures or short sales might have a further negative effect on credit scores depending on the formulae credit agencies use to construct credit scores (Avery et al. 1996), an issue we examine in detail below. Working in the opposite direction, without a required mortgage payment to make, households who lost their homes (an event that is likely much more common without a modification) might regain some flexibility in their ability to manage their other debt payments by adjusting their housing consumption. That in turn may help them improve their credit scores.

With respect to credit availability, we expect it to be severely limited for households in both the treatment and control groups for a significant portion of our study period, each having started the 2010s with an income shock, severe mortgage delinquency, and the substantial drops in credit scores discussed above. Moreover, HELOCs, which facilitated the use of “houses as ATMs” prior to the Great Recession (Chen, Michaux, & Roussanov 2020), practically disappeared after 2010.⁹ And even though mortgage refinancing at lower interest rates was available to homeowners after 2010, for the most part those transactions did not change the total amount of debt available to homeowners, but instead only reduced short-term monthly payments.¹⁰

3.3 Effect of Homeownership on Housing and Non-Housing Consumption

At their core, most economic models predict that, to the extent possible, households will try to smooth consumption out of total wealth over the course of their lifetimes. Adding housing to these models in a realistic way introduces several important features that leads to a rich set of predictions regarding the effect of homeownership on housing and non-housing consumption.¹¹

In the absence of borrowing constraints and transaction costs, households would respond to unanticipated changes in home equity by adjusting both their housing and non-housing consumption in

⁹HELOCs and fixed amount Home Equity Loans (HEL) fell substantially during the financial crisis. HELOC volume fell from a peak of \$190 billion in the boom to \$40 billion in 2009. It began to grow again in 2014, but plateaued at about \$100 billion in 2017, still well below pre-crisis levels. HEL volume fluctuated from \$2.7 to \$5.2 billion between 2010 and 2020, far below its pre-crisis peak of \$31 billion (Pradhan 2024).

¹⁰Pennington-Cross & Chomsisengphet (2007) demonstrate that the subprime sector played a dominant role in issuing cash-out refinance mortgages prior to the financial crisis, and those refinancing options essentially vanished following the crisis. Moreover, GSE cash out refinancing declined substantially during the financial crisis with no meaningful recovery in loan volume until the pandemic in 2020 (Federal Housing Finance Agency 2021). Annual loan volume of this kind fell from a high of \$350 billion in the boom to under \$200 billion in 2011 and was still only just above \$200 billion in 2019. Also see Chen, Michaux, & Roussanov (2020).

¹¹Guren, McKay, Nakamura, Steinsson (2021), for example, develop a life cycle model with borrowing constraints, uninsurable income risk, illiquid housing, and long term mortgages that generates predictions along the lines described here.

the same direction in every period, including the current period. However, adding housing transaction costs to the model has the direct effect of muting housing consumption responses, as adjustments to the level of housing consumption are now costly. Furthermore, changes in a homeowner's own home value are typically very highly correlated with the price of housing alternatives in the same market, limiting housing consumption responses to changes in house prices.

The predicted effect of home equity on non-housing consumption depends critically on the nature of borrowing costs. When the ability to borrow against home equity is available, gains in housing wealth can have two distinct effects and can be especially powerful in the presence of other borrowing constraints that limit the ability to borrow against expected future income. First, as mentioned above, an increase in wealth generally leads to a desire to consume more in every period, leading to a modest predicted increase in consumption in the present period. Second, for households who are constrained in their ability to borrow against expected future income — e.g., those Kaplan & Violante (2014) label "wealthy hand-to-mouth" — gains in housing wealth present an opportunity to move a much more considerable amount of consumption forward.¹²

In general, then, we expect the impact of maintaining homeownership on non-housing consumption to operate primarily through housing wealth and to depend critically on the ability of the households in our sample to borrow against any positive equity accrued in their home. In fact, several studies on the effect of housing wealth on consumption focus on the relaxation of collateral constraints as a key channel through which housing wealth operates (DeFusco 2018; Sodini et al. 2023). If, on the one hand, such borrowing constraints are minimal, economic theory would predict fairly unambiguously that consumption will respond positively to increases in housing wealth. If, on the other hand, borrowing against home equity is difficult, we would generally expect minimal effects on both housing and non-housing consumption, especially for households with minimal access to other savings or forms of credit. Given their initial status as marginal homeowners on the basis of income and credit scores, the subsequent income shock and severe mortgage delinquency, and the general tightening of requirements for mortgage credit and home equity lines over our study period, many households in our sample might face difficulties accessing any accrued home equity (Conklin, Gerardi, & Lambie-Hanson 2025). We explore the role of access to credit carefully below.

¹²Estimates suggest annual consumption effects of increased housing equity that range from 0–3% of wealth gains at the low end (Browning, Gørtz, & Leth-Petersen 2013; Disney, Gathergood, & Henley 2010; Guren et al. 2021) to 5–10% of wealth gains at the high end (Aladangady 2017; Graham & Makridis 2023; Mian, Rao, & Sufi 2013).

3.4 Effect of Homeownership on Mobility and Neighborhood Quality

Due to the relatively high cost of housing transactions, renters move much more often than homeowners. This fact is both a sharp theoretical prediction in tenure choice models and a clear empirical regularity, with renters moving 3-4 times as often as owners (Jia et al. 2023). That those who were able to maintain their homes through a mortgage modification will move less frequently in subsequent periods follows as a clear prediction in our context (Ferreira, Gyourko, & Tracy 2010; Fonseca & Liu 2024).

Whether there is a causal treatment effect on neighborhood quality is a more nuanced question. One possibility raised in the literature is that homeownership and neighborhood quality might be bundled — i.e., accessing certain highly resourced neighborhoods might be difficult as a renter (Deng, Ross, & Wachter 2003; Ross & Yinger 2002). While such bundling might be important at the high end of the neighborhood quality distribution, the homeowners in our sample reside in neighborhoods with about the median level of income on average, well below the median income of homeowners, and in neighborhoods that are roughly one-third renters. Thus, we do not expect bundling to affect neighborhood choice much in our study since any individuals who lose their homes already live in places with plenty of rental options. In fact, as we show below, the home purchases in our sample prior to the crisis were associated with at most very small increases in neighborhood income. Also, there was a growth of institutional ownership of housing during the 2010s (Gorback, Qian, & Zhu 2025), which also helped with expanding rental options in many suburban neighborhoods that were traditionally comprised of homeowners.

A potentially more relevant issue is the impact of housing wealth on neighborhood choice. Following the full recovery and expansion of housing prices, we might expect that wealth differences will begin to matter for the consumption of neighborhood quality near the end of the study period. But given the relatively low mobility rates of homeowners, any such differences in neighborhood quality may only emerge quite slowly or not appear at all over a ten year period.

4 Data and Research Design

In this section, we present our novel individual-level panel data and demonstrate how the unique context of the Great Recession supports our research design: a large fraction of the U.S. population were new homeowners; income shocks were pervasive; and loan modifications, though chaotically implemented, were ultimately effective at keeping people in their homes.

4.1 Sample and Data Sets

We begin with a sample of new mortgage originators, households that either purchased a new home or refinanced an existing mortgage between 2004 and 2008. This sample was originally developed in early 2009 by a subset of the authors, who used Home Mortgage Disclosure Act (HMDA) data and transaction and lien data from Dataquick Inc. to select a stratified, random sample of mortgages originated in seven key US housing markets, covering 50 large counties, from 2004 through 2008—see Bayer, Ferreira, & Ross (2016, 2018) for details.¹³ These earlier papers linked this sample of borrowers and mortgages to credit data from 2009, examining how these households fared in the foreclosure crisis that occurred in the immediate aftermath of the Global Financial Crisis during the Great Recession. In the current paper, we follow this sample of borrowers through 2022, allowing us to examine how their lives were affected in the medium to long run across many dimensions.

Specifically, the original sample includes 270,000 home purchase and refinance mortgages originated during the summers of 2004 through 2008 by a mix of single borrowers and co-borrowers, often married couples, for a total of 380,000 individual borrowers. The sample is restricted to remove non-arm’s length transactions and those involving institutional buyers and sellers, and was further restricted to transactions occurring between May and September of each year and to owner-occupied one-to-four-family structures. HMDA loans and Dataquick assessor records were then matched using whether the mortgage was home purchase or refinance, year of transaction, loan amount, Census tract of the housing unit, and the lender name. Finally, a subset of mortgages were randomly selected in equal numbers by site, by year, and by home purchase versus refinance mortgage.¹⁴ These resulting data contain the address of the property and the name(s) of the individual(s) involved, as well as characteristics of the house, loan, and borrower. The final data set provides a complete set of observable variables for 375,524 unique individuals. Summary statistics are provided in the first column of Table 1. Our sample has an average HMDA-reported income of \$109,602. The average loan size is \$224,100, with an average LTV of 0.77. The sample is 9.6% non-Hispanic Black, 16.6% Hispanic, and 7.9% non-Hispanic Asian.

¹³The seven broad housing markets include the Chicago, IL Consolidated Metropolitan Statistical Area (CMSA); Cleveland, OH Metropolitan Statistical Area (MSA); Denver, CO MSA; Los Angeles, CA CMSA; Miami, Ft. Lauderdale and Palm Beach MSA’s; San Francisco, CA CMSA; and the Maryland portions of the Washington DC-Baltimore CMSA. These sites were selected based on the regional availability of the housing transaction and lien data—at the time, these were the only sites with information on refinance mortgages available through Dataquick back to 2004.

¹⁴There was also oversample of African-American borrowers; Hispanic borrowers; white borrowers who reside in low income or minority neighborhoods; and high-cost loans, defined as rate spread loans in the HMDA data. Throughout our analysis in the current paper, we weight observations to account for this over-sampling, thereby matching the composition of newly originated mortgages of each type in each of our seven selected regions, giving each region equal weight.

We provided this sample to one of the major credit rating agencies, which used names and addresses to match borrowers to archival credit reporting data from June 30th of each year.¹⁵ We received detailed credit reports in seven waves, every three years from 2004 to 2022. This panel provides rich information about each individual’s financial circumstances before/during mortgage origination, and then for up to 18 years after origination. We define our matched sample as the set of individuals who had a Vantage credit score in the valid range (300 – 850).¹⁶ There are 294,185 such individuals in this matched sample, and they are observably similar to the full sample, as shown in the second column of Table 1.

Because of our triennial waves of credit report data, some households with a delinquency appearing on their 2010 reports had also already lost their homes by June 2010. For this reason, and to rule out reporting lags, we constrain the sample to individuals who also still have an open mortgage on their 2010 credit report, in order to understand the impact of a subsequent modification. All results presented below are robust to relaxing this constraint. The impact of adding this constraint, which is shown in column 3 of Table 1, has no meaningful effect on any of the variables. Our final sample includes 231,128 individuals.

Finally, we supplement our panel credit data with neighborhood characteristics drawn from several sources. First, we match each house in the sample to a Census tract, and use the 2000 Decennial Census to calculate neighborhood characteristics. Second, the credit reporting agency provided the ZIP code of primary residence for each individual in each of the seven waves of credit data. For each ZIP code, we use FHFA house price indices to estimate house price growth over each three year period and IRS income data to provide a measure of neighborhood income in each sample year. We use the former to estimate the house price appreciation that each household experienced if they owned a home during a given period. Finally, we use ZIP Code Tabulation Area aggregations of data from the 2010 Decennial Census and 2008-2012 5-year ACS to measure local house price levels and the fraction of households that are renters, which for simplicity we treat as fixed characteristics of a neighborhood (Schroeder et al. 2025).

4.2 Research Design

Our goal is to estimate the long-term impact of losing versus keeping one’s home. Because nearly all mortgage defaults during the Great Recession were driven by negative income shocks, our re-

¹⁵June 30th was selected because this was the earliest archive month that contained demographic data for 2010.

¹⁶The Vantage credit score is an alternative to the FICO credit score that was developed and maintained collectively by the three major credit reporting agencies.

search design begins by conditioning the sample to the occurrence of a severe (i.e., 90-day) mortgage delinquency. This sample constraint allows us to focus on the relevant set of individuals at risk of foreclosure. Next, we use the receipt of a mortgage modification as a treatment that shifts the probability of retaining one’s home following said shock. Below we show empirically that seriously delinquent borrowers who did versus did not receive a modification are very similar on observable attributes and experienced very similar financial circumstances during that period, suggesting that modification provides quasi-random variation in who was able to keep their homes.

Figure 1 shows the timeline on which we base our design. We observe households seven times from 2004 to 2022. All households either purchased a home or refinanced a home mortgage between 2004 and 2008. Next, we assign delinquency status in 2010 based on the presence of at least one 90-day delinquency in the previous 24 months. This is our definition of delinquency throughout the paper, as well as our sample-defining definition of distressed borrowers. A full 18% of the borrowers in our sample were delinquent in 2010. Then, we use the 2013 report to identify borrowers who received a mortgage modification in the previous 36 months (that is, in the period 2010 to 2013). 24% of delinquent borrowers had received a mortgage modification by June 2013.¹⁷ Hereafter, we refer to delinquent households that received a modification as our treatment group, and delinquent households that did not receive a modification as our control group.

The year 2013 is also the credit report year for which we define our main short-term outcome, i.e., homeownership. Throughout the paper, we measure homeownership as the presence of an open mortgage on the credit report. Since our final sample is comprised entirely of new borrowers for which we observe an open mortgage in 2010, actual homeownership and measured homeownership in 2013 should correspond very closely to each other. Finally, as reported in Figure 1, we use the 2016, 2019, and 2022 credit reports to measure long-term outcomes, such as homeownership, housing wealth, credit scores, credit availability, consumption, and neighborhood quality.

4.2.1 Empirical Model

Our main empirical specification is based on a version of the event study conducted by Sodini et al. (2023), which investigated the impact of gaining homeownership among a group of public housing co-ops in Stockholm. We adopt an inverse strategy, estimating the impact of *preserving* relative to *losing* homeownership for a large group of delinquent homeowners in the United States during the

¹⁷In the main analysis, we exclude the small number of households that already had mortgage modifications by June 2010. We show in a robustness exercise in Section 5 that estimated treatment effects are very similar for these modifications.

Great Recession. Specifically, we estimate the effects of receiving a mortgage modification m between 2010 and 2013 on homeownership status and other borrower outcomes y for individuals i residing in house h according to the following equation:

$$y_{iht} = \beta_t m_{ih} + g_t(X_{ih}) + \epsilon_{iht} \quad (1)$$

We initially estimate equation (1) using data from 2013. Since all individuals were homeowners in 2010, the coefficient β_t represents the short-term impact of receiving a mortgage modification. The term $g(X_{ih})$ includes pre-modification characteristics of individuals and houses, such as location (metropolitan areas and neighborhood features), house attributes (size and quality), loan characteristics (loan size, interest rate, lender, etc.), individual attributes (income, race, age, having a co-borrower, etc.), and credit report information from both 2004 and 2007. As we demonstrate below, the inclusion of this comprehensive set of controls has minimal effect on the estimation of β_t .

For long-term outcomes, our main tables and figures provide estimates from similar cross-sectional models for the years 2016, 2019, and 2022. We follow this procedure to allow the pre-modification controls to have differential effects across years.

4.2.2 Potential Selection into Modification

An obvious concern is that mortgage modifications may not be randomly distributed throughout the population. Rather, both the borrower and lender must agree to the modification, so that selection may occur on either side. As a first comparison to evaluate the possibility of selection, Figure 2 reports four variables (homeownership, Vantage credit score, credit card balance, and number of auto loans) that summarize relevant risk factors that would be observable to a lender making a modification decision. We report these variables for four groups of homeowners in our sample: non-delinquent with no modification, non-delinquent with a modification, delinquent with no modification (control), and delinquent with a modification (treatment). Panel (a) shows homeownership rates for all groups rising by 30% in 2007 from a base of around 55% in 2004, and then reaching 100% in 2010.¹⁸ Panel (b) shows that the group who do not become delinquent on their mortgages have high and stable credit scores. We also find that non-delinquent homeowners who receive modifications have slightly lower Vantage scores and also a drop in credit scores from 2007 to 2010, which is

¹⁸Homeownership rates rise between 2004 and 2010 in the sample for several reasons: new home purchases of first-time homeowners, coborrowers being added to new mortgages, and the increased capture of existing homeowners in early credit repository data due to new mortgage transactions.

suggestive of a small negative income shock. These homeowners took advantage of modifications without incurring heavy delinquencies and/or large income shocks, and were potentially favored by lenders as having higher creditworthiness than other delinquent homeowners. Moreover, these cases are also much simpler to be analyzed by mortgage services. Given the selected nature of this group, we will exclude them from the analysis below.

The other two groups in Figure 2 are delinquent borrowers and constitute the main treatment and control groups. Panel (b) shows that they have much worse pre-crisis credit scores that also fall precipitously from 2007 to 2010 as the households fall behind on mortgage payments. But delinquent borrowers with and without modifications have remarkably similar financial observables in the years leading up to and containing their delinquencies. Credit card balances in Panel (c) and auto loans in Panel (d), which are standard credit profile proxies for consumption, also show similar patterns with large declines between 2007 and 2010. Similar credit score and consumption changes in 2010 are consistent with borrowers experiencing very similar income shocks. We show similar trends in Appendix Figure 1, which tracks delinquencies across credit cards, auto loans, student loans, and unsecured personal loans. For all four types of credit, the distressed borrowers differ from non-distressed borrowers in both levels and trends. However, the treatment and control groups are on almost-indistinguishable paths.

These descriptives imply that selection by lenders into modification for severely delinquent borrowers is not a substantial threat to our identification. This lack of selection is consistent with the idea, discussed in Section 2, that during the 2010-2013 period, lenders were overwhelmed by the stock of seriously delinquent mortgages and unable to identify, among this set of delinquent borrowers, borrowers who would have better income recovery or house value growth prospects.

4.2.3 Balance Tests

Figure 3 provides a difference-in-means test for a larger set of covariates related to home and mortgage features, household characteristics, neighborhood variables, and credit information. Blue circles compare our treatment group to non-delinquent households with no modification. Differences are generally large in many credit and household variables.¹⁹ However, brown circles — which compare our treatment and control groups — show observable differences that are dramatically smaller.²⁰

¹⁹The table version of Figure 3 (Appendix Table 1) shows that households with modifications are much more likely to be Black (26pp versus 12pp) and Hispanic (34pp versus 15pp), have lower average credit scores in 2007 (665 versus 737) and higher LTVs at origination (0.82 versus 0.76), and have higher mortgage payment to income ratios (0.35 versus 0.23).

²⁰Appendix Table 1 reveals that in the subsample made up only of households with serious mortgage delinquencies, those with modifications are only slightly more likely to be Black (26pp versus 22p) and Hispanic (34pp versus 31pp), have

The comparison on observables matches the results from Figure 2, and suggests that lenders are not engaging in dramatic selection when offering mortgage modification to high delinquency borrowers. As a formal test, we use the non-delinquent sample of borrowers to estimate a “homeownership index,” using OLS to relate homeownership in 2013 to the suite of observables presented in the balance table. Next, we estimate out-of-sample fitted values of that regression for our delinquent sample, producing an estimated probability of homeownership based on observables. In Table 2, we report the average prediction by group. We find that our modification sample are, if anything, slightly negatively selected, with an expected homeownership rate of 70% compared to 71% in the delinquent sample who receive no modification. This level of selection is quite small compared to the selection arising when comparing our modification sample to all other mortgages, in which the average likelihood of homeownership in 2013 is 79%. This suggests that, conditional on 90-day delinquency by 2010, selection related to future (2013) homeownership is minor, and if present, biases against finding a positive treatment effect of mortgage modification.

In the next subsection, we directly test for selection into modification based on unobserved variables using an instrumental variables approach. Furthermore, in our main empirical results, presented in Section 5, we show that the estimated treatment effect of modification is virtually unaffected by the addition of observable controls that could proxy for unobserved selection. Indeed, we find no impact on our estimates from controlling for the presence of a spousal coborrower (indicating attachment to the home), region fixed effects and borrower age (indicating the permanence of income shocks), credit score and subprime loan proxies (indicating unobserved risk), or pre-treatment credit card balances (indicating magnitude of income shocks). Finally, in Section 6, we show that the homes owned by the treatment and control groups followed similar price evolutions. Thus, lenders were not selecting on the path of future house prices when allowing a modification. Overall, the range of empirical tests based on our rich set of observables lead us to conclude that unobservable selection into treatment appears unlikely to drive the results presented below. We confirm this result with our IV approach presented next.

4.2.4 IV Strategy Using Loan Performance Data

The balance tests above provide strong evidence that lender selection is unlikely to bias our results. As a final check on our identification strategy, we turn to an instrumental variables approach de-

very similar average credit scores in 2007 (665 versus 672) and similar LTVs at origination (0.82 versus 0.83), and have similar mortgage payment to income ratios (0.35 versus 0.33).

signed to address potential *borrower* selection into modification. In particular, we replicate our main short-run analyses in a separate sample where we can exploit an additional instrument, variation in loan servicers, to isolate the causal effects of modification. For this exercise, we use publicly available performance data on 30-year fixed-rate mortgages purchased by Fannie Mae, carefully structuring the sample to align with key features of our Experian analyses.²¹ To ensure sufficient coverage of severely delinquent loans at each servicer, we use the full national Fannie Mae dataset. Including a broader set of geographies in this analysis also has the benefit of demonstrating that our main estimates are similar at the national level.²²

We follow Agarwal et al. (2017), who use variation in mortgage servicers' pre-HAMP modification experience as an instrument to estimate the short-term impact of HAMP on foreclosure.²³ While we cannot utilize only pre-HAMP modification experience due to the minimal modification activity pre-HAMP in our Fannie Mae sample, we implement a similar two-step estimation procedure. Specifically, we first estimate the likelihood of individual i receiving a modification m on their primary home mortgage h from servicer s by 2013 as a function of loan attributes and servicer fixed effects. The following equation describes this model:

$$m_{ihs} = f(X_{ih}) + \delta_s + \mu_{ihs} \quad (2)$$

The estimated servicer fixed effects (δ_s) are then used as instruments for modification. In particular, we estimate the following two-stage least squares model:

$$m_{ihs} = \alpha \hat{\delta}_s + h(X_{ih}) + \tilde{\mu}_{ihs} \quad (3)$$

$$y_{ihs} = \beta \hat{m}_{ihs} + g(X_{ih}) + \tilde{\epsilon}_{ihs} \quad (4)$$

As noted above, the first stage modification equation of this simple IV model uses the modification outcomes from the same loans that are used in equation (2) to estimate the servicer modification

²¹We restrict the sample to Fannie Mae mortgages originated between 2004 and 2008 and purchased between 2004 and the first quarter of 2009. We then limit it to mortgages active in June 2010 that had experienced a 90-day delinquency by that time. We define modification as having received a mortgage modification by June 2013, and our second-stage outcome as avoiding adverse termination of the mortgage (i.e., foreclosure, short sale, or property surrender). Although the data do not identify the exact termination mechanism, most are almost certainly foreclosures or short sales.

²²This sample has two primary limitations: we cannot follow borrowers over time to study long-term outcomes, and it includes only conforming, 30-year fixed-rate mortgages purchased by Fannie Mae. By contrast, our main sample includes a more representative mix of mortgages originated between 2004 and 2008, including subprime loans.

²³Other identification strategies used by Agarwal et al. (2017), such as comparing owner-occupied and investment properties, are not feasible given the mortgages in our samples.

propensities. To avoid a potential endogeneity bias, we modify the basic IV approach in two ways. First, we use a leave-one-state-out approach that produces a unique set of servicer fixed effects estimates, $\tilde{\delta}_s - j$, for explaining the termination outcomes of mortgages in a given state j that do not depend on any mortgages in that state. This approach deals with the possibility that unobservables at the local- or state-level may influence our servicer fixed effect estimates, particularly for servicers who are disproportionately concentrated in certain areas. Second, in order to implement the leave-one-out strategy, we randomly split the sample in half between an analysis and hold-out sample, using the analysis sample to estimate equation (2) and the hold-out sample to estimate equation (4) to avoid specification search bias - see Leamer (1983).

To assess the stability of our estimates using the loan performance data, we estimate the OLS model from equation (1) and the IV models from (3) and (4), first without any controls in place of f , g , and h , and then by sequentially adding them. We begin by including fixed effects for origination quarter and year by state and metropolitan area by purchase versus refinance status, followed by the underwriting controls from Fannie Mae.²⁴ In the fourth specification, we exploit the timing information in the Fannie Mae data to add month by year fixed effects for the first observed 90-day delinquency, thereby controlling for the timing of the income shock — a feature unavailable in our credit history data. Finally, for the OLS model in equation (1), we add servicer and lender fixed effects, so the model is identified entirely from within-servicer variation, in contrast to the IV models, which are identified entirely from variation across servicers.

5 Homeownership Results

5.1 Homeownership in the Short Run — Main Sample

We begin by estimating the short-run impact of mortgage modification on homeownership. Figure 4 plots the share of individuals with an open mortgage on their credit report — our measure of homeownership — for treated and control groups. By construction, because everyone in our sample had an open mortgage in 2010, homeownership rates were initially 100 percent for all groups. Among treated borrowers (blue triangles), homeownership declined to roughly 85 percent by 2013, which is nearly identical to the decline observed among borrowers who never became delinquent (orange

²⁴These controls include debt to income ratio, credit score, combined loan to value ratio, whether the borrower has subordinate debt, logarithm of the loan amount, whether there is a co-borrower, whether a first time buyer, and property type.

squares).²⁵ In contrast, homeownership rates fell much more sharply for the control group (green circles), dropping to about 49 percent in 2013 — a 36 percentage-point gap relative to treated households.

Table 3 reports estimates of this short-run difference based on equation (1). Column (1) reports estimates without controls, corresponding to the descriptive statistics shown in Figure 4. Column (2) adds fixed effects for the factors defining our stratified sample — metropolitan area by year of origination by loan type (purchase or refinance).²⁶ Column (3) adds additional controls for fixed household and housing attributes measured at the time of mortgage origination, including the house size, loan amount, and loan-to-value ratio; indicators for racial/ethnic groups and the presence of jumbo loans, subordinate debt, rate spread, and a coborrower; and tract-level neighborhood variables. Column (4) incorporates borrowers’ prior credit histories (2004 and 2007 credit scores, credit card balances, and auto loans). Finally, Column (5) adds lender fixed effects. Across all five specifications, the estimated treatment effect remains remarkably stable, ranging from 35.4 to 35.9 percentage points. This stability — even after adding controls strongly correlated with homeownership — highlights the quasi-random nature of receiving a mortgage modification between 2010 and 2013. Moreover, the similarity of estimates with and without lender fixed effects indicates that the short-run impact of modification is consistent whether the identifying variation occurs within or across lenders.

5.1.1 The Role of Lender Foreclosure Actions

To assess how much of this homeownership gap reflects lender foreclosure activity, we examine two measures of foreclosure. The first captures any foreclosure notice sent to a credit bureau within the past 36 months, covering both open and settled first mortgages. These mortgages may ultimately cure (with or without modification), remain in loss mitigation, resolve via sale (including short sale), or terminate with an administrative or judicial foreclosure. The second measure, much narrower in scope, captures foreclosure proceedings initiated within the past 36 months, reflecting mortgages further along in the formal foreclosure process.²⁷

²⁵This 15% decline over three years may overstate the effect of the Great Recession itself, as some households likely transitioned out of homeownership for reasons unrelated to financial distress, such as job or family moves.

²⁶With seven housing markets, five origination years, and two loan types, there are 70 groups in total; standard errors are clustered at this level.

²⁷Foreclosure proceeding definitions vary by state and likely by lender, but they generally indicate a later stage of loss mitigation. Fewer than 20% of loans with foreclosure notices also have proceedings, while over 90% of those with proceedings also have a notice. Our main dataset does not record final settlement outcomes, but the Fannie Mae loan performance dataset, which we use for robustness, does report whether a loan was resolved via short sale or foreclosure. For causal evidence on the effect of foreclosure itself, see Diamond, Guren, & Tan (2025), who exploit judicial leniency in foreclosure proceedings.

Table 4 reports estimates of the impact of modification on these foreclosure outcomes using the full specification from Column (5) of Table 3. Column (1) shows that, by 2013, treated households were 18.9 percentage points less likely to receive a foreclosure notice. This estimate is approximately half the estimated short-term treatment effect for homeownership, suggesting that half of the homeownership effect is likely driven by home sales that occurred prior to any foreclosure notice. Column (2) shows that the difference in foreclosure proceedings is just 9.5 percentage points, implying that only about a quarter of the homeownership effect arises from borrowers advancing deeper into the formal foreclosure process. Together, these results indicate that many additional home sales — likely including short sales — occurred after a foreclosure notice but before any final judicial foreclosure.

5.2 Homeownership in the Short Run — IV Analysis of Fannie Mae Mortgages

We next turn to the Fannie Mae loan performance data. As described in the previous section, this dataset allows us to exploit variation in loan servicers’ modification activity as an instrument for mortgage modification, providing a test of whether OLS estimates are biased by the potential endogeneity of within-servicer modification decisions.

Table 5 reports the results of homeownership models that parallel those in Table 3. The top panel presents OLS estimates using the full Fannie Mae sample. The middle panel presents IV estimates, using the full sample both to estimate servicer fixed effects and to implement the second-stage regression. Finally, the bottom panel presents IV estimates based on the split-sample approach described earlier. The columns correspond as closely as possible to those in Table 3: Column (1) is unconditional; Column (2) adds origination quarter and year by metropolitan area by purchase/refinance fixed effects; Column (3) adds underwriting controls; Column (4) adds fixed effects for the month and year of initial delinquency (a control not available in our main sample); and Column (5) adds lender and servicer fixed effects.

The estimates from the Fannie Mae data closely mirror those from our main sample. In the baseline specification with no controls, the OLS estimates in the top panel imply that modification reduces the likelihood of foreclosure or short sale by about 40 percentage points, compared with 36 percentage points for retaining homeownership in our main sample. As controls are added, the OLS point estimates remain stable, ranging narrowly from 40.0 to 41.0 percentage points. The IV estimates in the middle and bottom panels exhibit wider confidence intervals, as expected, and fluctuate somewhat across specifications ranging between 36 and 42 percentage points, but remain both similar to the OLS estimates and to each other. Taken together, these IV results indicate that

residual selection on unobservables is unlikely to meaningfully bias the estimated effects in our main research design.

5.3 Homeownership in the Long Run

We now extend our homeownership analysis through 2022. Figure 5 plots cross-sectional point estimates and confidence intervals for the difference in homeownership between the modification (treatment) and the no modification (control) samples based on Equation (1). Each set of estimates in the figure corresponds to a specification of the model, as we successively include controls first for metro area by origination year by loan type (home purchase by refinance); household and housing attributes; credit variables; and lender fixed effects. All models reveal similar results in every year.

The estimates for the period between 2004 and 2013 mirror the short-run results shown in Figure 4: pre-trends are close to zero, then the treatment and control groups experienced a 36pp gap in homeownership in 2013. This homeownership gap gradually narrows over the subsequent decade, to about 30 percent in 2016, 22 percent in 2019, and about 19 percent in 2022. In this way, the estimated effect of remaining a homeowner by receiving a mortgage modification in the Great Recession is remarkably persistent, with a half-life that appears to last about a decade.²⁸

5.4 Heterogeneity

Figure 6 examines two key dimensions of heterogeneity. First, because our main analysis is based on individual-level observations, coborrowers on the same mortgage are treated as separate observations. Panel (a) separates the sample by whether a coborrower is present. Pre-treatment differences between treatment and control groups are small in both subsamples. The no-coborrower group shows persistently larger treatment effects, suggesting that having a coborrower may offer some protection against home loss. Nonetheless, both groups exhibit large and persistent effects of modification on homeownership. Second, the effects of the housing crisis and Great Recession varied substantially across U.S. regions. Panel (b) reports separate estimates for each of our seven study sites. The largest effects appear in regions that experienced steep house price declines during the crisis — notably the Los Angeles, San Francisco, and Miami regions. However, we observe substantial and persistent effects of modification on homeownership in every location.

²⁸By 2022, these estimates based on open mortgages may be a lower bound on homeownership effects, as some households may have fully repaid the mortgages they originated in the mid-2000s. This is especially plausible for the relatively small group of borrowers with 15-year mortgages.

We explore other sources of heterogeneity in the Appendix. Appendix Figure 2 examines variation by race/ethnicity and income. The effects are largest for Hispanic and Asian borrowers but are substantial across all racial and ethnic groups, with no meaningful differences by income. Appendix Figure 3 compares older and younger borrowers; the estimated effects of modification are nearly identical across generations.²⁹ Finally, Appendix Figure 4 analyzes an earlier period, focusing on borrowers who became severely delinquent by June 2007 and estimating the effect of receiving a modification by June 2010. Here, the initial effects are about 40 percentage points over time, narrowing to 10 percentage points by 2022. However, due to the smaller sample size, the standard errors are much larger, and the differences are not statistically distinguishable from our main effects.

Overall, the heterogeneity results presented here imply that, while there is some variation across households and locations, the effect of receiving a mortgage modification was quite broad-based, with a substantial, persistent impact on homeownership for all categories of households who became delinquent on their mortgages during the Great Recession.

6 Broader Outcomes — Housing Wealth, Credit, Consumption, and Neighborhood Quality

The results presented in Section 5 imply that mortgage modifications were critically important for households trying to maintain homeownership during the Great Recession. We now broaden our analysis to explore the trajectories of a wide variety of other outcomes, including housing wealth, credit and consumption measures, and neighborhood outcomes. Conceptually, there are two ways to think about the meaning and magnitude of these results. First, the most straightforward interpretation is that the estimated differences directly reflect the effect of receiving a mortgage modification between 2010 and 2013 for a group of marginal homeowners hit hard by the recession. Second, and more interestingly, any long run differences in trajectories can be viewed as reflecting the impact of being able to remain a homeowner through the Great Recession, made possible through the quasi-random availability of a mortgage modification. This “instrumental” perspective requires the assumption that the short-run impact of receiving a mortgage modification came only through its impact on homeownership status.³⁰ In this case, long-run differences in outcomes need to be divided

²⁹The similarity of results across generations provides further support for using the presence of an open mortgage as a proxy for homeownership in our recent-borrower context.

³⁰The plausibility of this additional assumption is bolstered by the fact that we find no other short-run effects on credit, consumption, or other outcome variables, as shown below.

by 0.36, the estimated impact on homeownership in the short run, to provide a causal estimate the long-run impact of maintaining homeownership status through 2013 on other outcome variables.

6.1 Housing Wealth

We begin by examining how receiving a mortgage modification affected housing wealth accumulation in the decade following the Great Recession. To do so, we exclude refinances and focus solely on new home purchases, allowing us to observe both the purchase price and initial equity position. Because detailed mortgage terms are unavailable, we estimate changes in housing wealth due solely to house price appreciation and transitions into and out of homeownership.

Figure 7 reports two sets of estimates. Panel (a) calculates potential housing wealth assuming all homeowners kept their homes from purchase (2004–2008) through 2022. We compute wealth as the initial down payment plus ZIP-level FHFA price appreciation applied to the original purchase price. Panel (b) incorporates actual homeownership transitions between 2013 and 2019, resetting negative equity to zero when households lose homes and assuming a 20% down payment for those who repurchase. When households move, subsequent price changes reflect their new ZIP code. These assumptions are intentionally conservative: they (i) remove any negative equity from homeowners following sales or foreclosures, and (ii) ignore any wealth gains that may have accrued to individuals in the modification process.

Not surprisingly, given national trends in house prices, Panel (a) shows that all groups built positive housing wealth by 2007 and lost much of it by 2010. If they had remained in their initial homes, they would have largely recovered by 2016, with further gains through 2022. Price paths are virtually identical across treatment and control groups, ruling out selection into modification on the basis of local price trends, current equity, or expectations of future appreciation. By 2022, had they stayed in their homes, all groups would have held similar average housing wealth (\$289,000–\$322,000).

Panel (b) reveals the role of changes in homeownership for the treatment and control group. Initially, resetting negative equity to zero boosts the wealth of the control group (who were much more likely to lose their homes) in 2013. Over time, however, modified borrowers’ higher rates of homeownership allow them to capture a much larger share of the post-2013 price recovery. By 2022, they hold roughly \$83,000 more in housing wealth than comparable borrowers who did not receive a modification. Scaling by the short-run impact of modification on homeownership (0.36) yields an estimated causal effect of roughly \$231,000 — a number that is consistent with the overall magnitude of house price appreciation from 2013–2022.

One could attempt to further refine this estimated difference in wealth accumulation by accounting for other differences between owners and renters. Such calculations would require strong assumptions and we expect large wealth difference to remain for several key reasons. First, rents and home values experienced very similar rates of appreciation from 2007-2022 in the markets included in our sample.³¹ Second, homeowners had many opportunities between 2004-2022 to refinance their mortgages at very low interest rates, potentially sharply increasing the benefits of owning versus renting during the sample period. And, third, relative to investments in stocks, bonds, or other financial instruments, the housing market represents the primary way most households can take advantage of leverage when investing. Given the high rate of home price appreciation from 2013-2022, it is unlikely that alternative, unleveraged investments would have yielded anywhere near the same level of returns for the vast majority of households. Together, these patterns indicate that the ability to remain a homeowner during the housing recovery produced large, lasting wealth gains.

6.2 Creditworthiness and Availability

The next set of results that we explore characterize the trajectories of a wide variety of measures of creditworthiness from 2004-2022, as well as a key measure of credit availability that is considered closely tied to housing equity. Figure 8 reports the long-run trajectory of credit scores through 2022. Both treatment and control groups experience very large declines in credit scores between 2007 and 2010, with a slow but full recovery by 2022. What is most remarkable about Figure 8 is how similar the average credit score is for the treatment and control groups. Throughout the entire sample period, the scores are essentially identical, suggesting at most a tiny long-run impact of retaining one's home on credit scores.³²

While these results for credit score may initially seem inconsistent with some of the evidence from the literature on foreclosures, such as Brevoort & Cooper (2013), it is crucial to highlight a key methodological difference. In particular, most existing estimates compare individuals experiencing foreclosure to a much broader population of homeowners who did not receive a foreclosure notice. In contrast, by conditioning our analysis on a set of individuals with a 90-day mortgage delinquency,

³¹We calculate ZIP-level percent changes in house value and rent using the 5-year ACS from 2007-2011 and 2018-2022. We find that in our sample areas, the mean percent change in home value is 49.3%, while the mean change in rent is 48.1%. The mean percent change in FHFA housing price index is 44.5%.

³²Appendix Table 10 reports the point estimates, confirming that these differences are small and in many years not statistically significant. We also look at delinquencies in other credit markets, which are key factors in determining credit scores finding, as shown in Appendix Figure 5, virtually identical trajectories over delinquencies on bank cards (panel a), auto loans (panel b), student loans (panel c) and unsecured personal debt (panel d). Mirroring changes in credit scores, delinquencies rose in all four credit markets by 2010 and largely began recovering by 2013.

our estimates isolate the impact of losing one's home from the broader impact of the income or life shock that likely preceded the delinquency.

Figure 9 shows the evolution of credit scores from 2010 to 2022 in relation to the presence of mortgage derogatories in 2010, highlighting another likely reason why mortgage modifications had a limited effect on credit scores. The presence of a first derogatory, typically a 90-day mortgage delinquency, is associated with a 150-point decline in credit score in 2010, relative to borrowers with no derogatories. However, subsequent mortgage derogatories, such as those due to a short sale or foreclosure, are associated with neither additional impacts in the short-term and or on the future trajectory of credit scores. These results suggest that the initial serious delinquency is the primary cause of credit score damage, with later negative events having a limited compounding effect.

A major way in which preserving homeownership might have influenced credit availability is by providing homeowners with additional collateral for housing-secured debt. A key mechanism for accessing this debt is a home equity line of credit (HELOC). Figure 10 (a) illustrates new HELOC openings from 2004 to 2022. While a substantial portion of homeowners opened HELOCs before the financial crisis, only a tiny percentage borrowers in the treatment and control groups opened HELOCs between 2010 and 2022. Even though the modification group had greater average housing wealth compared to the non-modification sample in 2019 and 2022, new HELOC openings increased only marginally and remained far below pre-crisis levels in those years. Moreover, the slight increases in 2019 and 2022 were comparable across both groups, suggesting that increased equity did not significantly translate into higher HELOC utilization during this period.

The finding that HELOC origination rates failed to recover, even as housing equity increased for the modification group, is likely explained by our focus on severely delinquent mortgage borrowers, a population with substantially lower pre-crisis credit scores and concentrated in lower-income neighborhoods compared to the average non-delinquent borrower. Panel (b) of Figure 10 shows that from 2010 onward, households opening new HELOCs had credit scores significantly higher than both our treatment and control groups. For instance, even in 2022, after a period of credit score recovery, our sample's Vantage scores were 150 points lower than the average new HELOC borrower and 40 points lower than the average non-HELOC individual. These findings indicate that our sample of marginal homeowners likely faced significant barriers to accessing their housing equity through standard debt instruments like HELOCs.³³

³³A limitation of our analysis is that the credit repository data do not allow for the identification of cash-out refinance mortgages, which may represent an alternative method of accessing housing equity.

6.3 Consumption Responses

We next investigate whether maintaining homeownership impacted non-housing consumption, using credit card and auto loan activity, two common proxies (see Figure 11). Panels (a) and (b) show that both the treatment and control groups experienced sharp declines in monthly credit card payments and balances, a response consistent with the income shock and subsequent credit tightening they experienced during the Great Recession.³⁴ Strikingly, the trajectories of both groups remained nearly identical from 2013 onward, suggesting that maintaining homeownership had little to no effect on consumption. Our use of both payments and balances strengthens this conclusion by allowing us to account for the effect of revolving credit card debt on these consumption measures.³⁵ Likewise, panels (c) and (d) show no meaningful differences in auto loan activity, indicating that durable consumption was similarly unaffected. A further check on revolving debt levels, measured by satisfactory bank card balances relative to payment history, also revealed no significant difference between the groups (see Appendix Figure 6). Ultimately, while both severely delinquent groups were clearly affected by the crisis, maintaining homeownership did not alter their average consumption or debt response over the following decade.

The apparent lack of a consumption response to housing capital gains might seem contrary to earlier research (Browning, Gørtz, & Leth-Petersen 2013; Disney, Gathergood, & Henley 2010; Guren et al. 2021), which often linked housing wealth to consumption. However, recent evidence highlights the role of housing wealth in relaxing collateral constraints, facilitating access to credit (DeFusco 2018; Sodini et al. 2023). A probable reason for the absence of consumption responses in our study is that the severely delinquent homeowners in our sample had consistently low credit scores and incomes, making it difficult for them to access cash-out mortgage refinances and HELOCs following the financial crisis (Chen, Michaux, & Roussanov 2020). This view is supported by data in Figure 10, which demonstrates that HELOC lending plummeted after the crisis, particularly for our severely delinquent group regardless of whether a modification was received, and remains highly restricted through 2022. Additionally, cash-out refinance mortgages were significantly curtailed during and after the crisis (Chen, Michaux, & Roussanov 2020; Federal Housing Finance Agency 2021;

³⁴These large declines in credit card balances in 2010 and 2013 are consistent with the significant deleveraging of household debt following the Great Recession (Albuquerque, Baumann, & Krustev 2015; Justiniano, Primiceri, & Tambalotti 2015) with that deleveraging arising in our sample only for households that experienced a significant shock to their ability to service their mortgage during the crisis.

³⁵Credit card balances may overstate consumption if an individual is accumulating revolving debt, while payments may understate it in that same scenario. Similar effects on both measures suggests minimal bias from changes in revolving balances.

Pennington-Cross & Chomsisengphet 2007).³⁶

6.4 Household Mobility and Neighborhood Outcomes

We next examine household mobility, which we expect to be higher in the non-modification sample due to both short-run relocation after home loss and higher long-run mobility among renters. Panel (a) of Figure 12 displays the evolution of mobility rates following the severe delinquency. From 2013-2022, the modification group exhibited mobility rates similar to non-delinquent borrowers, in line with their comparable homeownership rates (see Appendix Figure 7). In contrast, the control group showed substantially elevated mobility in 2013, with individuals almost twice as likely to change ZIP codes compared to the treatment group. This mobility gap narrows in subsequent years, as the cohorts age and the disparity in homeownership rates decreases.

Panels (b) and (c) of Figure 12 show trajectories for two common proxies for neighborhood quality: the percentile of neighborhood income, as measured in a nationwide sample of ZIP codes, and median housing price. Importantly, we fix both measures at their 2010 levels for each neighborhood, so that any changes in neighborhood quality shown here occur only through the mobility of the households in our sample across neighborhoods. Consistent with the slight negative selection of households into mortgage modifications, control households lived in neighborhoods that were about 3 percentile points higher in income in 2004, 2007, and 2010. In both cases, these households were living in neighborhoods at about the 50th percentile of the neighborhood income distribution, compared almost 70 percent for the non-delinquent sample.³⁷ Remarkably, these differences between the treatment and control groups change very little during and following the Great Recession, remaining at about 3-4 percentile points in every year through 2022. The results for median neighborhood house price follow an identical pattern, indicating that maintaining homeownership through the Great Recession had no short- or long-term effect on neighborhood quality.

Panel (d) illustrates a key reason why the non-modification group may have been able to maintain neighborhood quality even while a large share of these borrowers lost their homes. The panel reports the share of renter-occupied housing units in a borrower's ZIP code, fixing these at 2010 levels to isolate changes resulting from household mobility. The trajectories for both the treatment and control

³⁶This finding contrasts with borrowing patterns observed immediately before the crisis; between 2002 and 2006, households increased debt by an average of 25% of their housing equity gains Mian & Sufi (2011). This period appears, however, to be an outlier, as (DeFusco 2018) estimates that household borrowing increased by only 4-13% of collateral gains in the broader time frame of 1997-2012.

³⁷Interestingly, in the pre-period (2004-2010), when many of our sample households purchased new homes, we observe minimal changes in neighborhood income or prices. This suggests that the borrowers' purchases did not significantly alter neighborhood quality.

groups are again almost identical, with households residing in neighborhoods with approximately one-third rental housing from 2007 onwards. The relative abundance of rental housing in these neighborhoods suggests that borrowers who lost their homes typically had many options to remain in neighborhoods of similar quality even as they switched tenure status.

7 Conclusion

In this paper, we provide the first causal evidence on the long-term effects of homeownership in the United States. We leverage the Great Recession as a natural setting that offers two critical factors for studying this question: widespread negative income shocks and the chaotic implementation of relief initiatives. This allows us to examine a large group of distressed homeowners, whose quasi-random receipt of a mortgage modification led to significant short-term differences in their ability to retain their homes. These differences in homeownership persist more than a decade later, resulting in a large accumulation of housing wealth for households who were able to retain their homes. Despite these long-term effects in homeownership and housing wealth, we find no lasting disparities in other key financial outcomes, including consumption, access to credit, creditworthiness, and neighborhood environment. Such absence of secondary homeownership effects in our context is likely due, in large part, to the sharp tightening of cash-out refinancing and HELOC availability following the financial crisis, which appears to have been especially binding for our sample of seriously delinquent homeowners.

The structure of our research design has important implications for interpreting our results and their connection to the existing literature. Both the treatment and control groups in our sample experienced similar economic shocks. Consequently, our analysis captures the impact of an intervention aimed at preserving homeownership rather than the shock itself.³⁸ In this way, our estimates measure the costs of losing a home rather than the broader financial distress associated with mortgage delinquency — such as its impact on credit scores, which is identical for the treatment and control groups. A key strength of this approach is its direct relevance for evaluating policies that support homeownership during economic downturns, including temporary assistance and mortgage forbearance programs such as those introduced in response to the COVID-19 pandemic (Cherry et al. 2021; Goodman, Kaul, & Neal 2020; RADAR Group 2023). Because the majority of our sample consists of new mortgage borrowers who faced income shocks, our findings offer valuable insights for policies

³⁸For example, Finkelstein et al. 2025, who study the impact of the Great Recession unemployment shock on mortality and welfare.

targeting those most vulnerable to similar future disruptions.

Moreover, our population of homeowners are lower income and have lower credit scores than the typical homeowner. Thus, we specifically provide estimates of homeownership effects with a focus on a population of marginal homeowners. These distressed borrowers are highly policy-relevant, particularly in the Great Recession setting, in which a much larger fraction of homeowners experienced distress than in normal times (18% of our sample). For this population, our findings suggest that the main long-term benefit of avoiding home loss after a major income or employment shock is preserving homeownership as a means of building wealth. Other outcomes — credit, consumption, and neighborhood quality — follow similar trajectories, indicating that losing a home does not necessarily cause lasting financial ruin.

Therefore, our findings suggest that some of the potential downside risks of homeownership, frequently discussed in the literature but not yet causally estimated, may be less severe than previously feared. While buying a home certainly has important implications for potential capital gains and losses, our estimates suggest that losing one’s home following a negative economic shock does not necessarily lead to more extensive long-term financial hardship. Knowing that these broader downside risks are limited is important both for individuals deciding whether to buy a home and for assessing potential risks of policy initiatives aimed at expanding homeownership.

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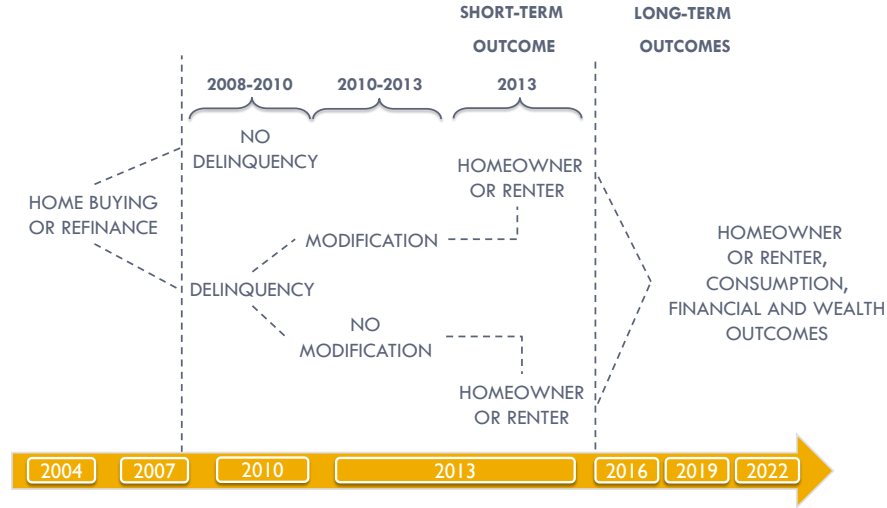
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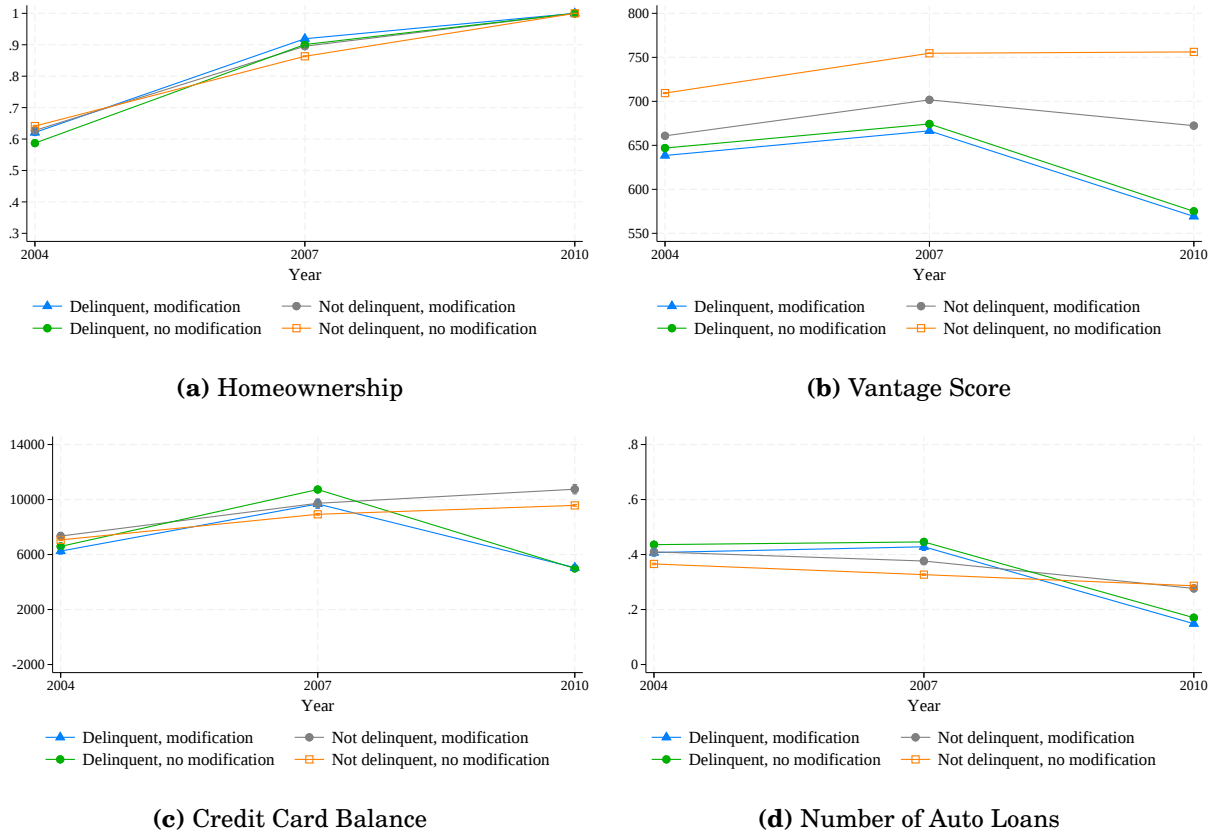
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Figure 1: Timeline



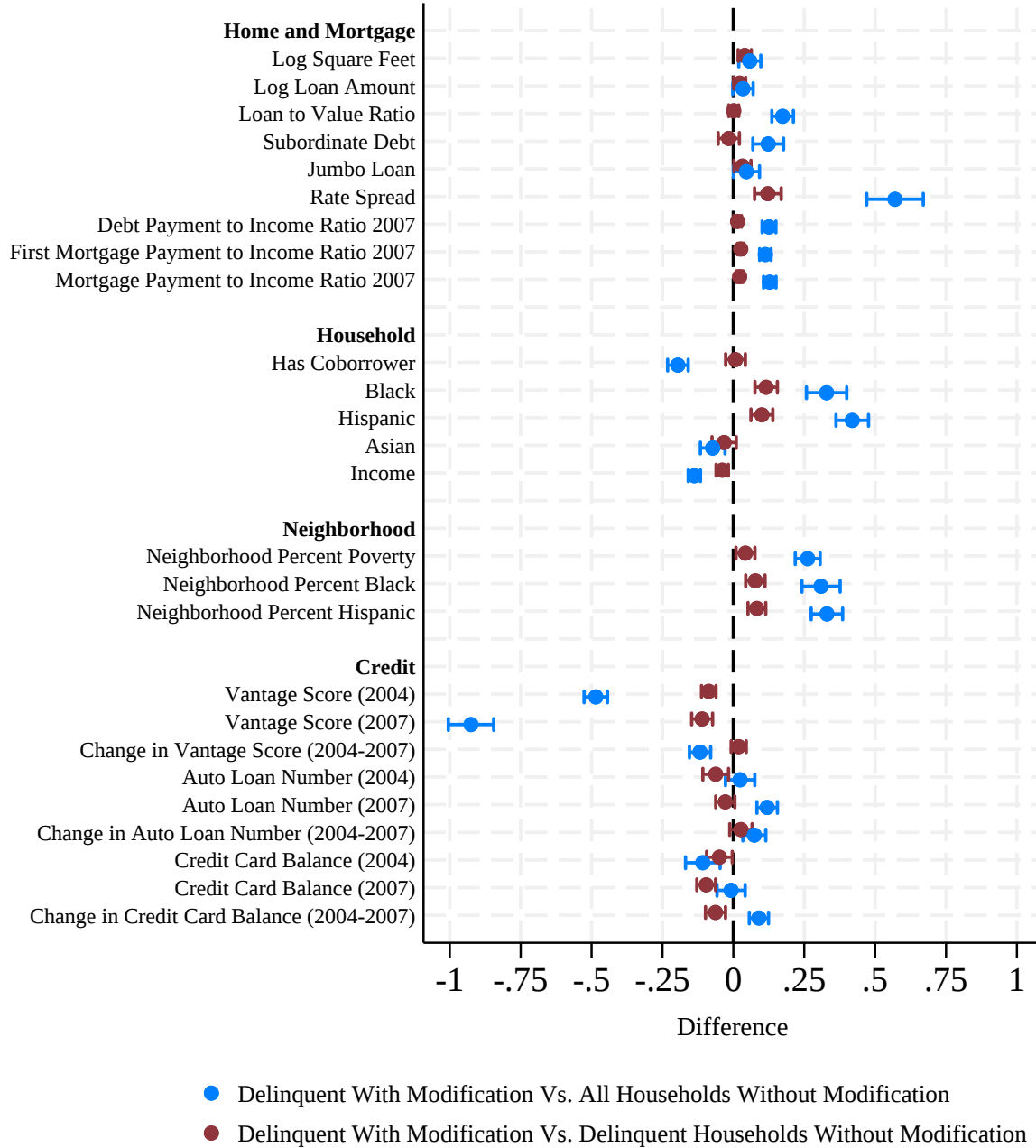
Notes: Figure displays the timeline of events for our 7 waves of credit data, every 3 years from 2004 to 2022. Borrowers purchase homes or refinance existing mortgages from 2004-2008. Delinquencies for our sample occur from 2008-2010. Modifications are received between 2010 and 2013.

Figure 2: Pre-Trends in Homeownership and Credit Variables



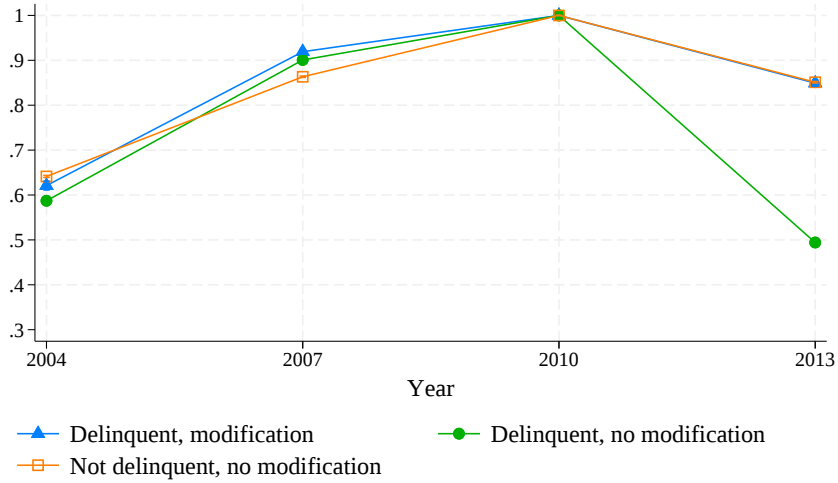
Notes: Figures display groupwise averages for homeownership (measured by presence of an open mortgage), credit score, credit card balance over the last 3 months, and number of auto loans. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013.

Figure 3: Balance Figure



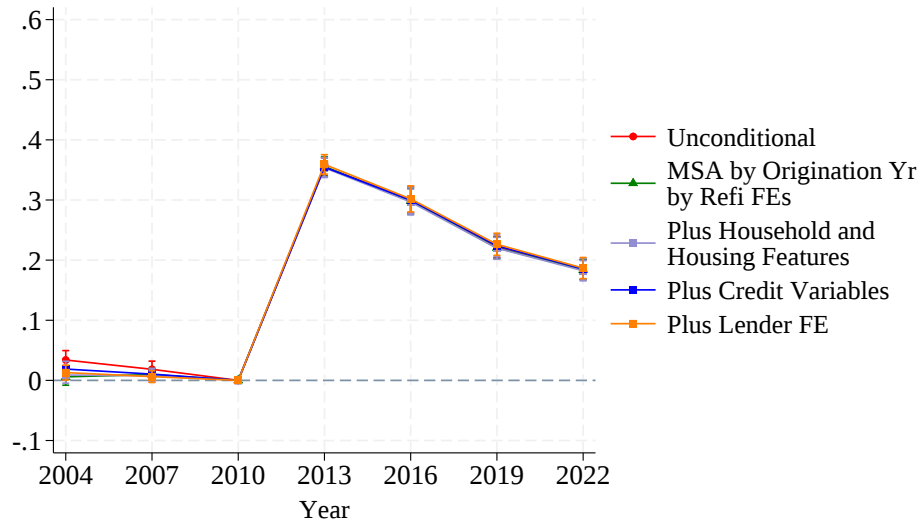
Notes: Figure displays balance on observables for two comparisons. Each row represents a regression of the displayed variable on the left on an indicator for receipt of a mortgage modification. Dots show point estimates and bars show 95% confidence intervals. The horizontal axis is in standard deviation terms for non-binary variables. Blue dots show a comparison of delinquent households with mortgage modifications (our treatment group) to a comparison group of all households with no mortgage modification, and show substantial selection. Brown dots show a comparison of delinquent households with a mortgage modification (our treatment group) to other delinquent households with no modification (our control group), largely abating selection. Full results are shown in Appendix Table 1.

Figure 4: Short-Run Changes in Homeownership



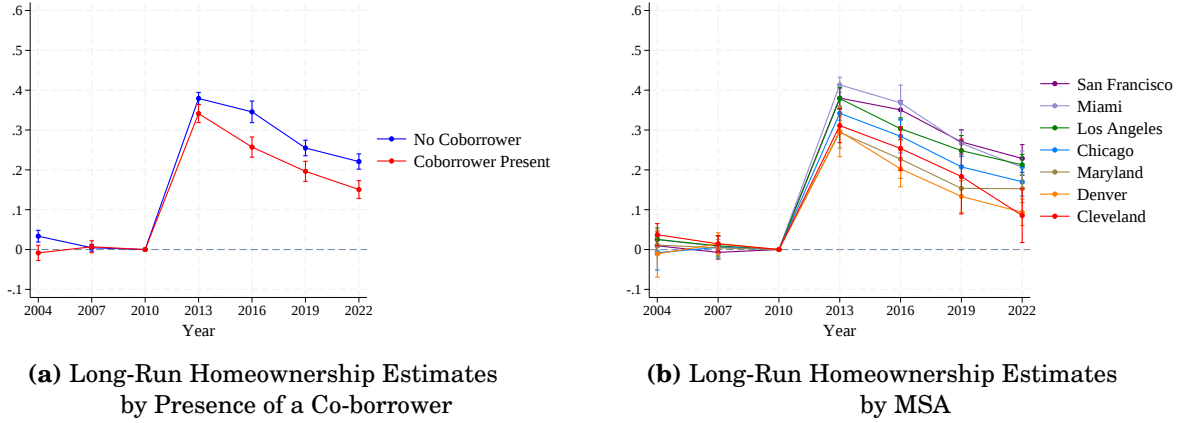
Notes: Figure displays groupwise averages for homeownership (measured by presence of an open mortgage). Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013.

Figure 5: Long-Run Impact on Homeownership by Model



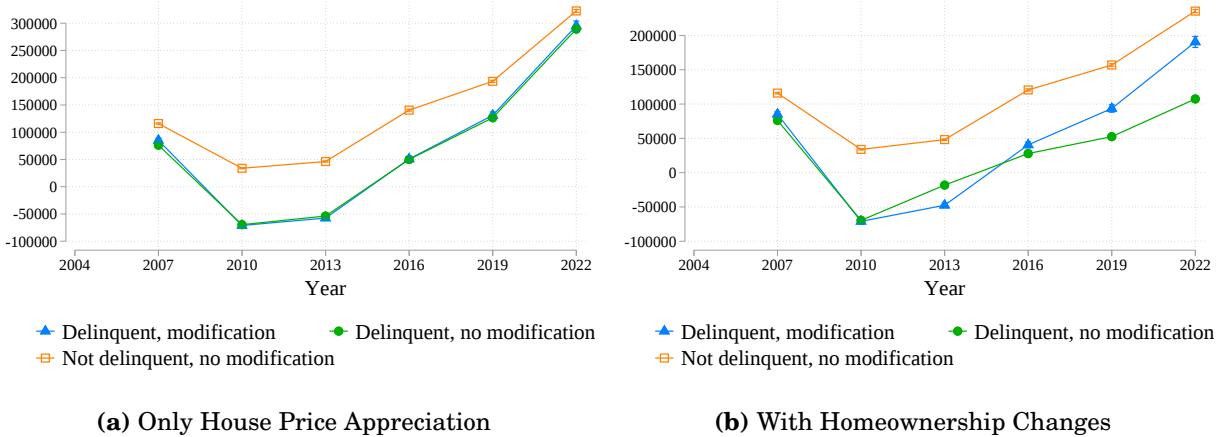
Notes: Each series displays the point estimates and confidence intervals for an estimation of Equation (1). The red dots display unconditional differences in homeownership between treatment and control groups in each year. Green triangles add fixed effects for bins defined by MSA, origination year of loan, and loan type (purchase versus refinance). Lavender squares add a vector of observable characteristics for the household and house, including square feet (log), initial loan amount (log), loan-to-value ratio, subordinate debt, jumbo loan, rate spread dummy, debt payment to income ratio in 2007, first mortgage payment to income ratio in 2007, total mortgage payment(s) to income ratio in 2007, presence of a co-borrower (spouse), race and ethnicity (Black, Hispanic, Asian), initial income, neighborhood poverty rate, neighborhood percentage of African Americans, and neighborhood percentage of Hispanics. Blue rectangles add a vector of observable characteristics for the household's recent credit. Credit variables include Vantage score, number of auto loans, and credit card balance, for both 2004 and 2007. Orange rectangles add fixed effects for each lender. Standard errors are clustered by MSA, year of loan origination, and type of loan. Appendix Table 2 displays full results.

Figure 6: Long-Run Homeownership Estimates by Presence of a Co-borrower and MSA



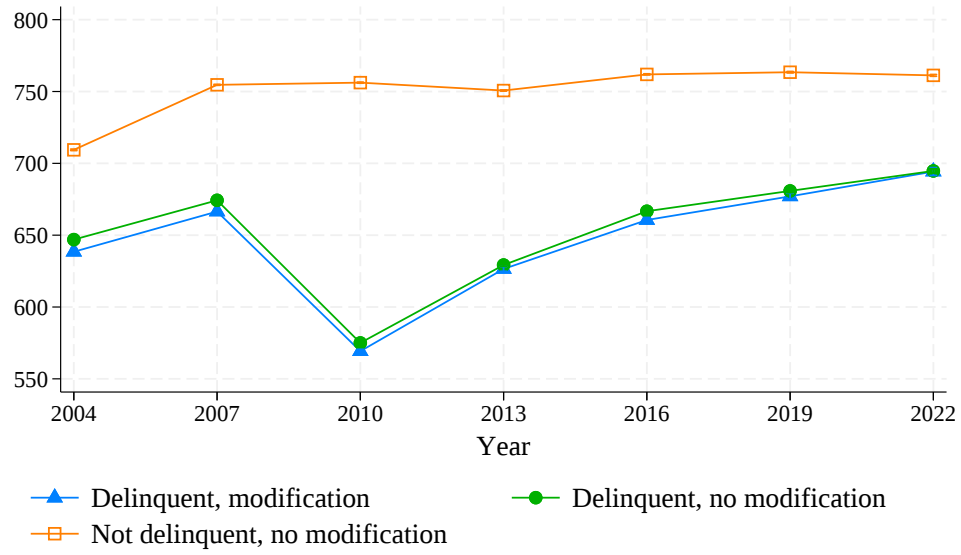
Notes: The figure displays point estimates and confidence intervals for an estimation of Equation (1), separately by either presence of a co-borrower (left) or location (right). The model of heterogeneity by MSA includes fixed effects for bins defined by year of loan origination and type of loan (purchase or refinance), as well as lender fixed effects. The model for co-borrower presence additionally includes MSA fixed effects. The controls are consistent with the main setup in Figure 5. The only difference is that the presence of a co-borrower (spouse) is only included in the MSA model. Standard errors are clustered by MSA (only in the co-borrower model), year of loan origination, and type of loan. Full results are displayed in Appendix Table 3 for heterogeneity by MSA and Appendix Table 4 for the presence of a co-borrower.

Figure 7: Evolution of Housing Wealth



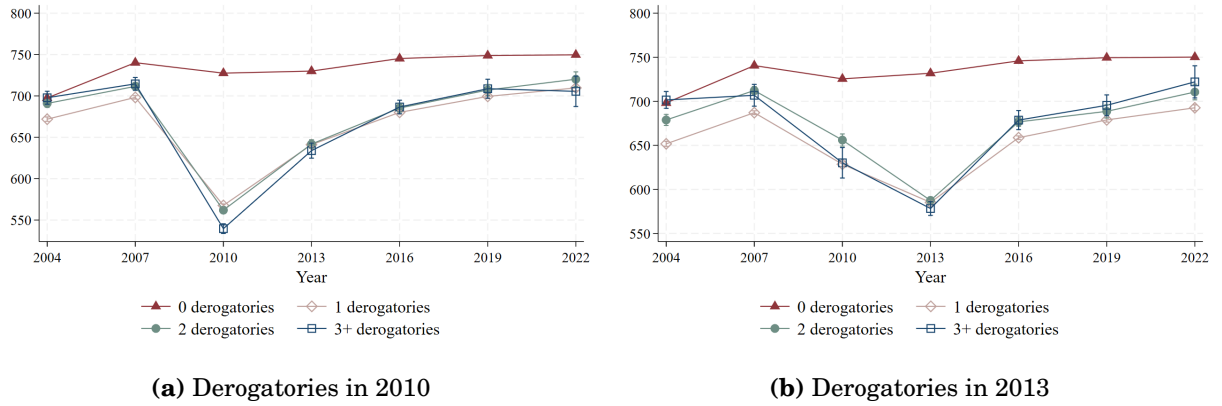
Notes: Panel (a) displays average house price appreciation by group, which estimates the evolution of housing wealth under the assumption that all households remain homeowners. Each year displays groupwise averages of the sum of initial equity (initial home value minus initial loan amount), times average house price appreciation in the ZIP code of residence in each year. Households that change ZIP codes receive appreciation in their new ZIP code. Panel (b) accounts for home loss in all years later than 2010. For those who lose homeownership status, if capital gain is positive, we attribute it the household but allow no further wealth accumulation. If capital gain is negative, we treat it as zero. Panel (b) also adds capital gain back for households who lose homeownership status then regain it in a later period. We assume a 20% down payment for such households. Households with coborrowers have gains split in half. Descriptive levels of wealth by group are summarized in Table 6, with Panel (a) corresponding to Column (1) and Panel (b) corresponding to Column (4), with intermediate calculations in between. Regression results of differences are shown in Appendix Table 9.

Figure 8: Long-Run Evolution of Credit Scores



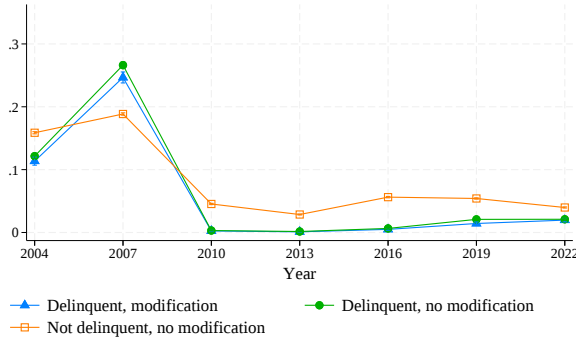
Notes: Figure displays groupwise averages for credit scores. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. Regression results for differences are shown in Appendix Table 10.

Figure 9: Evolution of Credit Score by Derogatories

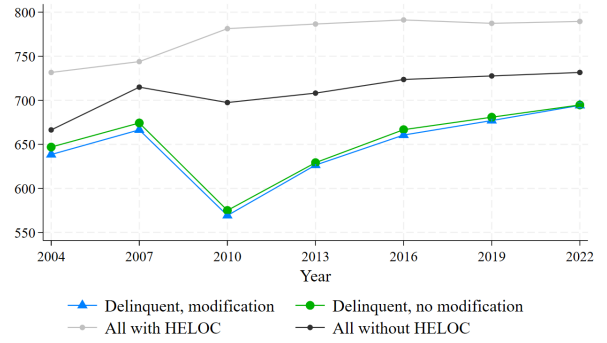


Notes: Each figure displays the average credit score of groups defined by the number of mortgage delinquencies accrued in a particular year. The left panel defines groups by the number of mortgage derogatories in 2010. The right panel defines groups by the number of mortgage derogatories in 2013.

Figure 10: Home Equity Lines of Credit



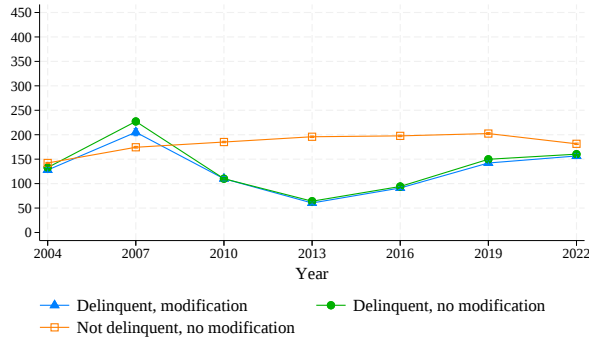
(a) New Home Equity Line of Credit



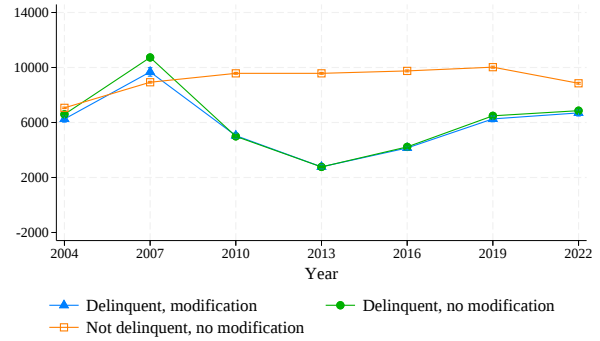
(b) Credit Score by New HELOC

Notes: The left panel displays trends in the share of households with a newly-opened home equity line of credit (HELOC) in each year. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. The right panel displays trends in credit scores among treatment and control groups, as well as (1) all households with a newly-opened HELOC and (2) all households without a newly-opened HELOC.

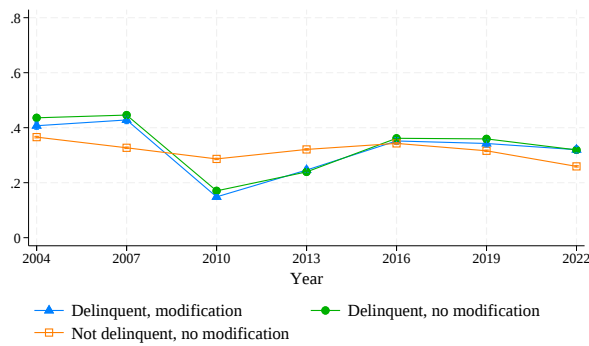
Figure 11: Evolution of Consumption



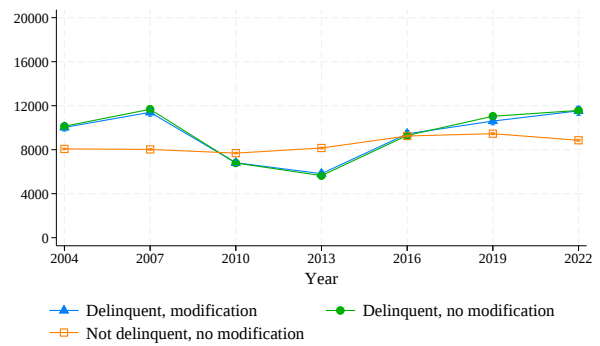
(a) Credit Card Monthly Payment



(b) Credit Card Balance



(c) Number of Auto Loans



(d) Auto Loan Balance

Notes: Figure displays groupwise average values of four measures of consumption: credit card monthly payments, credit card balances, number of auto loans, and auto loan balances. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. Results of regressions on differences are displayed in Appendix Table 12.

Figure 12: Evolution of Neighborhood Attributes



Notes: Figure displays groupwise mobility rates and average neighborhood characteristics. Mobility is defined by changes in ZIP code between years. Neighborhood income percentiles are nationwide income ranks, fixed at 2010 values. House prices are medians within neighborhoods, fixed at 2010 values. Percent renters measures the fraction of housing units that are renter-occupied, fixed at 2010 values. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. Results of regressions on differences are displayed in Appendix Table 13.

Table 1: Summary Statistics of Full, Matched, and Final Samples

	(1)	(2)	(3)
	Full Sample	Matched Sample	Final Sample
Refinance	0.538	0.535	0.527
Has Coborrower	0.593	0.600	0.597
Male	0.536	0.500	0.508
Black	0.096	0.091	0.088
Hispanic	0.166	0.146	0.132
Asian	0.079	0.084	0.088
HMDA Income (in \$1,000s)	109.603	111.146	113.470
Home Size (Log Sq. Ft.)	6.751	6.746	6.736
Loan Amount (Log \$)	12.320	12.340	12.346
LTV at Origination	0.770	0.767	0.763
Subordinate Debt	0.184	0.186	0.183
Jumbo Loan	0.223	0.230	0.233
Rate Spread	0.157	0.141	0.108
Neighborhood % Poverty	7.506	7.179	6.901
Neighborhood % Black	10.652	10.182	9.737
Neighborhood % Hispanic	15.725	14.900	13.935
Observations	375,524	294,185	231,128

Notes: Figure displays summary statistics of the sample. Column (1) uses the full sample after removing repeated borrowers (same name and address). Column (2) displays individuals who were matched to credit data and for whom housing attributes are available, dropping individuals with missing or invalid credit scores or house size. Individuals are dropped if they are missing credit scores in two or more years, unless the only missing years are 2019 and 2022. Column (3) restricts to individuals with an open mortgage in 2010 and with no modification until 2013.

Table 2: Balance for Homeownership Index For the Main Balance Table Sample

	No Modification	No Modification	Modification	Difference	Difference
	All	Delinquent	Delinquent	All	Delinquent
Homeownership Index Year	(1)	(2)	(3)	(3)-(1)	(3)-(2)
2013	0.79	0.71	0.70	-0.087***	-0.012***
2016	0.73	0.47	0.46	-0.253***	-0.010***
2019	0.69	0.48	0.47	-0.201***	-0.003
2022	0.65	0.52	0.52	-0.122***	-0.003

Notes: The homeownership index in a given year is estimated on the sample of borrowers with no mortgage modification. The index regresses homeownership in the year on a set of control variables defined 3 years prior. The regression uses MSA by year of loan origination by type of loan (purchase or refinance) fixed effects, and clusters standard errors by these variables. The control variables also include house square feet (log), initial loan amount (log), loan-to-value ratio, subordinate debt, jumbo loan, rate spread dummy, debt payment to income ratio in 2007, first mortgage payment to income ratio in 2007, mortgage payment to income ratio in 2007, presence of a co-borrower (spouse), race and ethnicity (Black, Hispanic, Asian), initial income, neighborhood poverty rate, neighborhood percentage of African Americans, and neighborhood percentage Hispanics. The control variables also include credit variables (Vantage score, number of auto loans, and credit card balance) before the current year. For each group, we calculate the average predicted value using the resulting coefficients and observables. These are in-sample predictions for non-delinquent, no-modification borrowers, and out-of-sample predictions for other groups. Columns (1)-(3) show groupwise averages of the index. Column (4) shows the difference between our treatment group and the sample of all non-modification households. Column (5) shows the difference between our treatment group and our control group.

Table 3: Short-Run Impact of Mortgage Modification on Homeownership

	(1)	(2)	(3)	(4)	(5)
Modification Indicator	0.355*** (0.008)	0.356*** (0.008)	0.354*** (0.008)	0.355*** (0.008)	0.359*** (0.008)
MSA by Origination Yr by Refi FEs	N	Y	Y	Y	Y
Household and Housing Features	N	N	Y	Y	Y
Credit Variables	N	N	N	Y	Y
Lender Fixed-Effects	N	N	N	N	Y
Mean of Dependent Variable	0.578	0.578	0.578	0.578	0.578
R2	0.093	0.102	0.111	0.114	0.121
Observations	41440	41440	41440	41440	41118

Notes: Column (1) displays unconditional differences in homeownership between treatment and control groups in 2013. Column (2) adds fixed effects for bins defined by MSA, origination year of loan, and loan type (purchase versus refinance). Column (3) adds a vector of observable characteristics for the household and house, including square feet (log), initial loan amount (log), loan-to-value ratio, subordinate debt, jumbo loan, rate spread dummy, debt payment to income ratio in 2007, first mortgage payment to income ratio in 2007, total mortgage payment(s) to income ratio in 2007, presence of a co-borrower (spouse), race and ethnicity (Black, Hispanic, Asian), initial income, neighborhood poverty rate, neighborhood percentage of African Americans, and neighborhood percentage of Hispanics. Column (4) adds a vector of observable characteristics for the household's recent credit. Credit variables include Vantage score, number of auto loans, and credit card balance in 2004 and 2007. Column (5) adds fixed effects for each lender. Standard errors are clustered by MSA, year of loan origination, and type of loan.

$p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4: Short-Run Impact of Mortgage Modification on Foreclosure Notice and Foreclosure Proceeding

	(1) Foreclosure Notice	(2) Foreclosure Proceeding
Modification Indicator	-0.189*** (0.010)	-0.095*** (0.007)
MSA by Origination Yr by Refi FEs	Y	Y
Household and Housing Features	Y	Y
Credit Variables	Y	Y
Lender Fixed-Effects	Y	Y
Mean of Dependent Variable	0.690	0.229
R2	0.137	0.116
Observations	41118	41118

Notes: Column (1) "Foreclosure Notice" is a binary variable showing indicating a foreclosure notice on a first mortgage trade, including settled first mortgages, in the 72 months preceding June 2013. Column (2) "Foreclosure Proceeding" is a binary variable indicating whether a foreclosure proceeding started in the 36 months preceding June 2013. Standard errors are clustered by MSA, year of loan origination, and type of loan. Control variables and fixed effects are as in Column (5) of Appendix Table 2. $p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 5: Short-Run Impact of Mortgage Modification in Fannie Mae Sample

	(1) No Controls	(2) MSA by Orig Date by Purch/Refi FE	(3) Plus Underwriting Controls	(4) Plus Delinquency Timing FE	(5) Plus Service and Lender FE
Full Sample OLS Estimates					
Modification	0.400*** (0.018)	0.410*** (0.023)	0.408*** (0.023)	0.407*** (0.023)	0.407*** (0.023)
R-Squared	0.193	0.296	0.317	0.320	0.321
Observations	318,466	316,033	316,033	316,033	316,032
Full Sample IV Estimates					
Modification	0.396 (0.043)	0.418*** (0.074)	0.382*** (0.047)	0.373*** (0.050)	
R-Squared	0.193	0.209	0.232	0.228	
First Stage	1.185*** (0.039)	1.050*** (0.043)	1.000*** (0.032)	0.983*** (0.037)	
Observations	318,466	316,033	316,033	316,033	
Hold-Out Sample IV Estimates Omit Own State					
Modification	0.390 (.)	0.407*** (0.066)	0.370*** (0.044)	0.362*** (0.045)	
R-Squared	0.193	0.211	0.232	0.230	
First Stage	1.184*** (0.056)	1.049*** (0.050)	1.005*** (0.036)	0.985*** (0.042)	
Observations	158,825	155,830	155,830	155,830	

Notes: The top panel displays OLS estimates of equation (1), in our parallel sample of Fannie Mae loans. The middle panel implements our IV strategy using the full sample. The bottom panel implements the split-sample IV. In all cases, columns successively add controls to parallel our main analysis. Sample construction, covariates, and estimating equations are detailed in Sections 4.2.4 and 5.2.

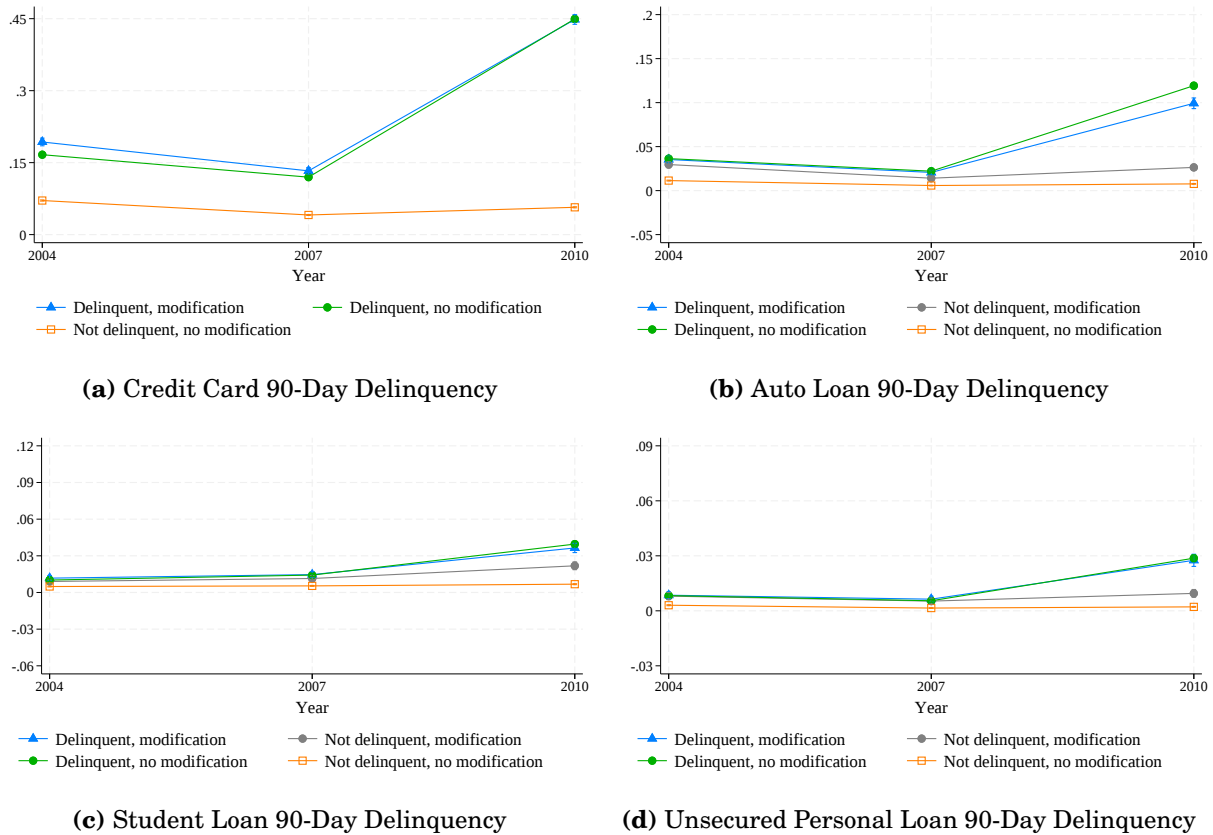
Table 6: Estimated House Wealth Differences in 2022 under Alternative Assumptions (in 2022 dollars)

Group	(1) House Price Appreciation	(2) Plus Not A Homeowner 2013	(3) Plus Not A Homeowner 2016-2022	(4) Plus Home- owner Again 2016-2019
Delinquent, modification	295,144	255,651	179,086	190,526
Delinquent, no modification	289,265	149,936	84,482	107,478
Not delinquent, no modification	322,519	281,971	224,272	235,590
Difference Delinquent Mod VS No Mod	5,879	105,716	94,604	83,048

Notes: The table displays estimated house wealth differences in 2022 for different groups of homeowners. The first row are our treatment group. The second row are our control group. The third row represent other, non-distressed homeowners. The fourth row shows differences between treatment and control. Column (1) estimates the evolution of housing wealth under the assumption that all households remain homeowners. Each year displays groupwise averages of the sum of initial equity (initial home value minus initial loan amount), times average house price appreciation in the ZIP code of residence in each year. Households that change ZIP codes receive appreciation in their new ZIP code. Column (2) accounts for home loss in the 2010-2013 period. For those who lose homeownership status, if capital gain is positive, we attribute it the household but allow no further wealth accumulation. If capital gain is negative, we treat it as zero. Column (3) follows the same procedure for home loss in later years. Column (4) adds capital gain back for households who lose homeownership status then regain it in a later period. We assume a 20% down payment for such households. Households with coborrowers have gains split in half.

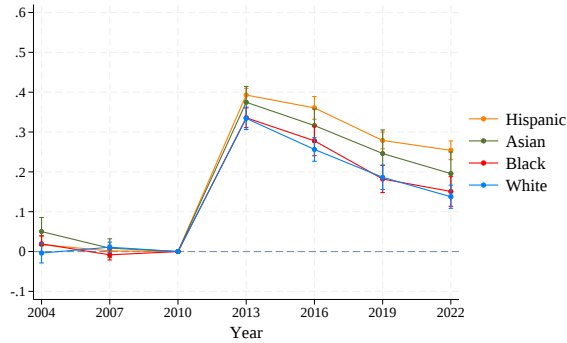
8 Appendix

Appendix Figure 1: Pre-Trends in Delinquencies

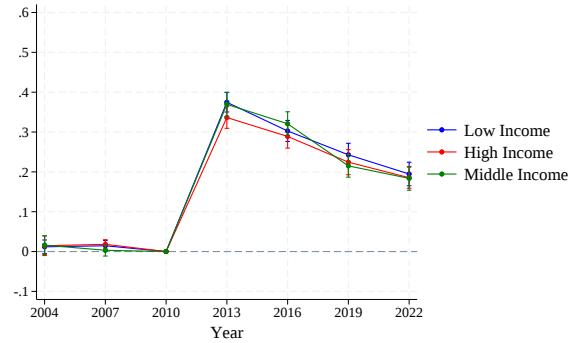


Notes: Figure displays groupwise average rates of 90-day delinquency in the last 24 months across four types of credit: credit cards, auto loans, student loans, and unsecured personal loans. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013.

Appendix Figure 2: Long-Run Homeownership Estimates by Race/Ethnicity and Income



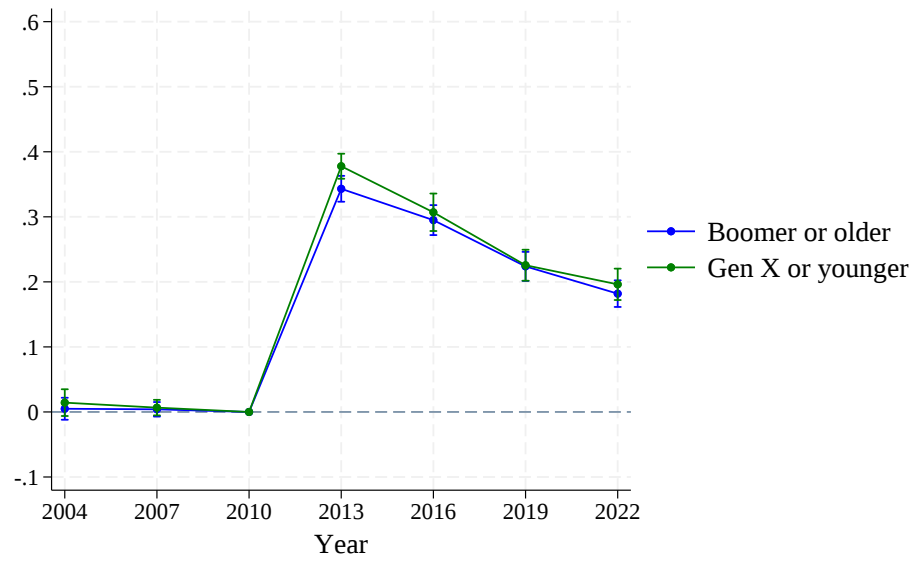
(a) Long-Run Homeownership Estimates by Race and Ethnicity



(b) Long-Run Homeownership Estimates by Income Terciles

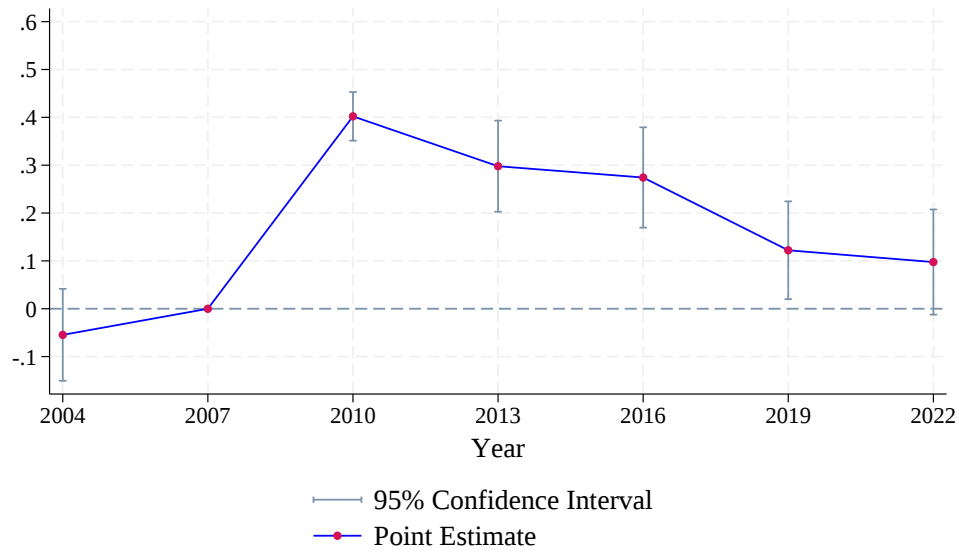
Notes: The figure displays point estimates and confidence intervals for an estimation of Equation (1), separately by either race/ethnicity (left) or income (right). Income groups are defined by terciles of HMDA-reported income at the time of loan origination. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that Panel (a) omits controls related to race and ethnicity, and Panel (b) omits controls related to income. Standard errors are clustered by MSA, year of loan origination, and type of loan. Full results are displayed in Appendix Table 5 for heterogeneity by race/ethnicity and Appendix Table 6 for income.

Appendix Figure 3: Homeownership by Generation



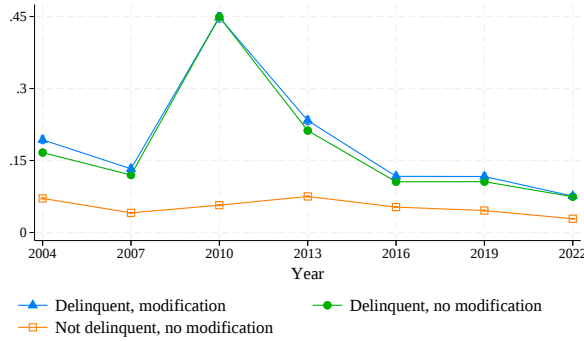
The figure displays point estimates and confidence intervals for an estimation of Equation (1), separately by borrower age. The model contains all fixed effects and controls from Column (5) of Appendix Table 2. Standard errors are clustered by MSA, year of loan origination, and type of loan. Full results are displayed in Appendix Table 7.

Appendix Figure 4: Long-Run Impact on Home-Ownership—Earlier Delinquency and Modification

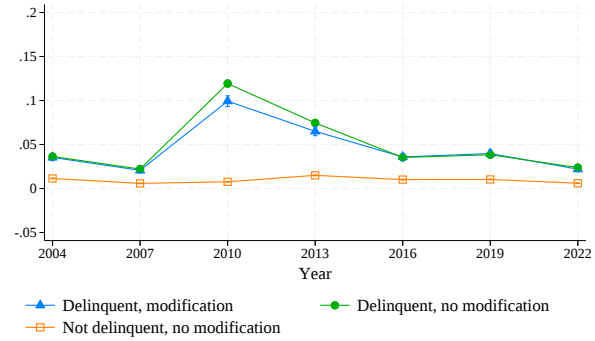


Notes: The figure displays point estimates and confidence intervals for an estimation of Equation (1), for an earlier sample of distressed borrowers. Delinquencies occur in the 24 months preceding June 2007, and modifications are received by 2010. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that 2010 variables are replaced by their 2007 counterparts and 2007 variables are replaced by their 2004 counterparts. Standard errors are clustered by MSA, year of loan origination, and type of loan. Full results are displayed in Appendix Table 8.

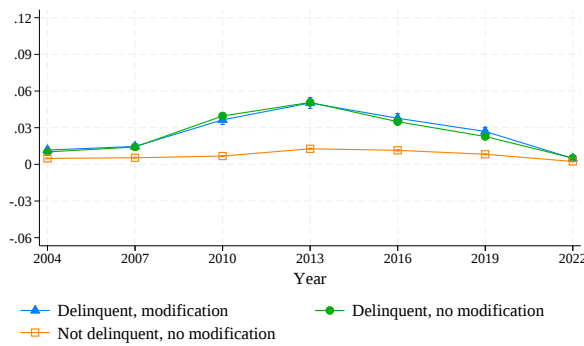
Appendix Figure 5: Evolution of Delinquencies



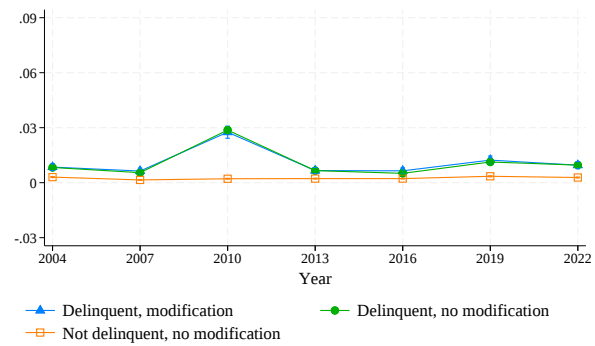
(a) Credit Card 90-Day Delinquency



(b) Auto Loan 90-Day Delinquency



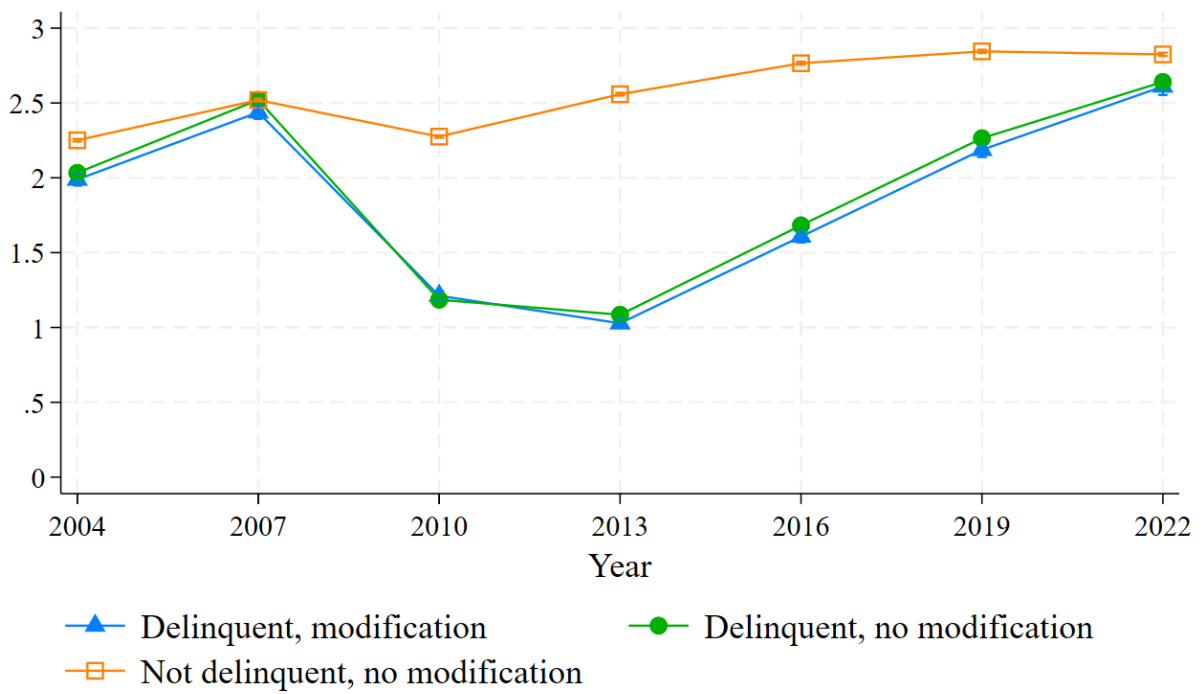
(c) Student Loan 90-Day Delinquency



(d) Unsecured Personal Loan 90-Day Delinquency

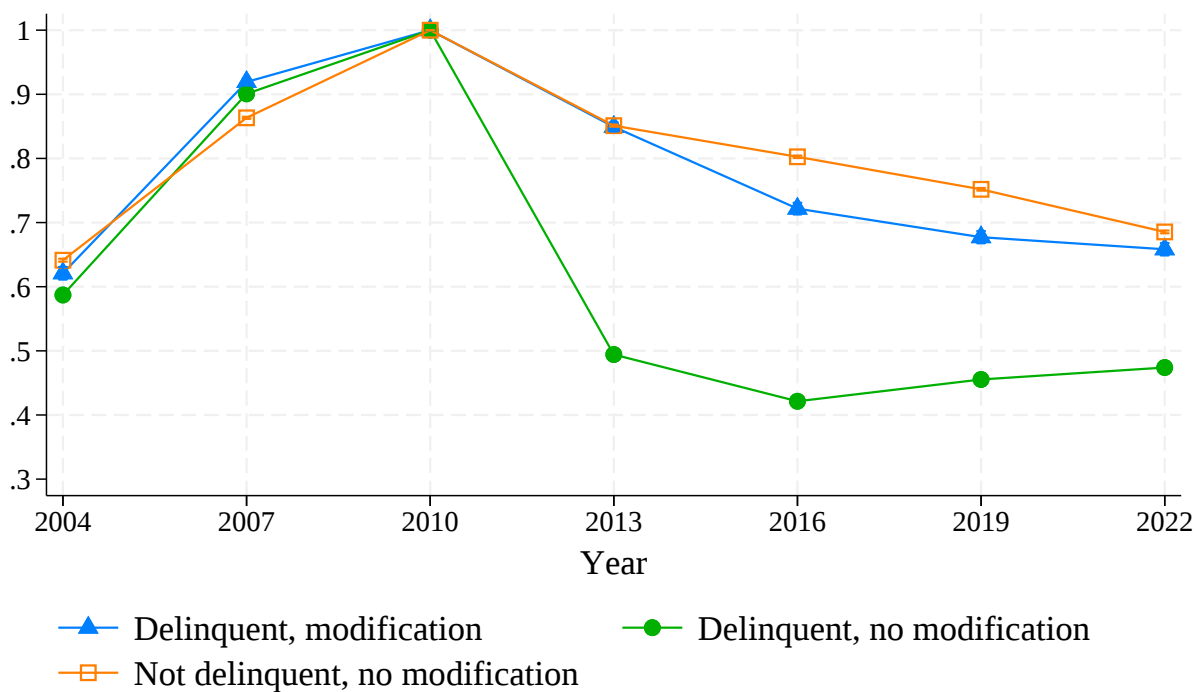
Notes: Figure displays groupwise average rates of 90-day delinquency in the last 24 months across four types of credit: credit cards, auto loans, student loans, and unsecured personal loans. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. Results of regressions on differences are displayed in Appendix Table 11.

Appendix Figure 6: Long-Run Evolution of Bankcards with Satisfactory Balance Activity to Payment History Ratio



Notes: Figure displays groupwise averages in the number of bankcards that have a satisfactory ratio of balance activity to payment history. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013.

Appendix Figure 7: Long-Run Evolution of Homeownership



Notes: Figure displays groupwise rates of homeownership. Groups are defined by delinquency in the 24 months preceding June 2010 and by the receipt of a mortgage modification by 2013. Regression results for differences are shown in Appendix Table 2.

Appendix Table 1: Balance Table

	No Modification All (1)	No Modification Delinquent (2)	Modification Delinquent (3)	Difference All (3)-(1)	Difference Delinquent (3)-(2)
Home and Mortgage					
Log Square Feet	6.75	6.83	6.92	0.124***	0.086***
Log Loan Amount	12.33	12.43	12.48	0.024*	0.016**
Loan to Value Ratio	0.76	0.83	0.82	0.049***	0.000
Subordinate Debt	0.18	0.29	0.26	0.048***	-0.006
Jumbo Loan	0.21	0.27	0.30	0.019*	0.013**
Rate Spread	0.14	0.34	0.39	0.204***	0.044***
Debt Payment to Income Ratio (2007)	0.31	0.42	0.44	0.113***	0.013**
First Mort Payment to Income Ratio (2007)	0.21	0.29	0.32	0.082***	0.019***
Mort Payment to Income Ratio (2007)	0.23	0.33	0.35	0.098***	0.017***
Household					
Has Coborrower	0.57	0.44	0.46	-0.097***	0.004
Black	0.12	0.22	0.26	0.110***	0.039***
Hispanic	0.15	0.31	0.34	0.153***	0.037***
Asian	0.08	0.07	0.06	-0.019***	-0.009
Income	114	109	106	-17***	-5***
Neighborhood					
Neighborhood Percent Poverty	7.54	10.03	10.20	1.922***	0.313**
Neighborhood Percent Black	11.21	17.09	18.63	6.383***	1.602***
Neighborhood Percent Hispanic	14.76	22.74	24.59	6.269***	1.576***
Credit					
Credit Score (2004)	690	635	630	-61***	-11***
Credit Score (2007)	737	672	665	-74***	-9***
Change in Credit Score (2004-2007)	48	36	35	-13***	2
Number of Auto Loans (2004)	0.36	0.40	0.37	0.014	-0.037***
Number of Auto Loans (2007)	0.34	0.44	0.42	0.070***	-0.017*
Change in Auto Loans (2004-2007)	-0.02	0.03	0.04	0.056***	0.020
Credit Card Balance (2004)	6550	5836	5516	-1,172***	-535**
Credit Card Balance (2007)	8563	9414	8649	-107	-1,280***
Change in Credit Card Balance (2004-2007)	2013	3578	3133	1,065***	-746***
Number of Observations	213007	31686	9754		

Notes: This table is visualized in Figure 3. Incomes used to generate income ratios are from the year of mortgage origination. Column (1) displays the mean of each variable for all households with no mortgage modification by 2013. Column (2) restricts to households with no mortgage modification who also experienced a 90-day mortgage delinquency, and represents our key control group. Column (3) displays variable means for households with a mortgage modification who also experienced a 90-day delinquency, and represents our treatment group. The final two columns show differences between groups, representing point estimates for a regression that has the variable of interest on the left-hand side, and a group indicator on the right-hand side, as well as fixed effects for bins defined by MSA, year of loan origination, and loan type (purchase or refinance). Standard errors are clustered by these bins.

$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2: Table of Long-Term Estimate Homeownership by Model

	(1)	(2)	(3)	(4)	(5)
2004	0.034*** (0.008)	0.006 (0.007)	0.009 (0.007)	0.019*** (0.007)	0.013** (0.006)
2007	0.018*** (0.007)	0.010* (0.005)	0.008 (0.005)	0.010** (0.005)	0.006 (0.005)
2010	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
2013	0.355*** (0.008)	0.356*** (0.008)	0.354*** (0.008)	0.355*** (0.008)	0.359*** (0.008)
2016	0.300*** (0.011)	0.298*** (0.011)	0.297*** (0.011)	0.300*** (0.011)	0.302*** (0.011)
2019	0.222*** (0.009)	0.220*** (0.009)	0.220*** (0.009)	0.224*** (0.009)	0.226*** (0.009)
2022	0.184*** (0.008)	0.184*** (0.009)	0.182*** (0.009)	0.185*** (0.009)	0.187*** (0.009)
MSA by Origination Yr by Refi FEs	N	Y	Y	Y	Y
Household and Housing Features	N	N	Y	Y	Y
Credit Variables	N	N	N	Y	Y
Lender Fixed-Effects	N	N	N	N	Y
Observations	41440	41440	41440	41440	41118

Notes: This table is visualized in Figure 5. Column (1) displays unconditional differences in homeownership between treatment and control groups in each year. Column (2) adds fixed effects for bins defined by MSA, origination year of loan, and loan type (purchase versus refinance). Column (3) adds a vector of observable characteristics for the household and house, including square feet (log), initial loan amount (log), loan-to-value ratio, subordinate debt, jumbo loan, rate spread dummy, debt payment to income ratio in 2007, first mortgage payment to income ratio in 2007, total mortgage payment(s) to income ratio in 2007, presence of a co-borrower (spouse), race and ethnicity (Black, Hispanic, Asian), initial income, neighborhood poverty rate, neighborhood percentage of African Americans, and neighborhood percentage of Hispanics. Column (4) adds a vector of observable characteristics for the household's recent credit. Credit variables include Vantage score, number of auto loans, and credit card balance, in both 2004 and 2007. Column (5) adds fixed effects for each lender. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3: Table of Long-Term Estimate Homeownership by MSA

	Chicago	Cleveland	Denver	LA	Maryland	Miami	SFBA
2004	-0.008 (0.022)	0.037** (0.014)	-0.011 (0.030)	0.025 (0.015)	0.011 (0.010)	0.025** (0.010)	0.009 (0.011)
2007	0.006 (0.014)	0.014 (0.011)	0.014 (0.014)	0.009 (0.013)	0.004 (0.015)	0.008 (0.009)	-0.007 (0.009)
2010	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
2013	0.342*** (0.009)	0.311*** (0.022)	0.297*** (0.033)	0.379*** (0.014)	0.294*** (0.020)	0.413*** (0.010)	0.380*** (0.013)
2016	0.284*** (0.021)	0.254*** (0.028)	0.202*** (0.023)	0.303*** (0.014)	0.227*** (0.025)	0.368*** (0.023)	0.351*** (0.012)
2019	0.207*** (0.014)	0.183*** (0.048)	0.133*** (0.021)	0.248*** (0.019)	0.154*** (0.010)	0.266*** (0.016)	0.270*** (0.016)
2022	0.170*** (0.018)	0.085** (0.035)	0.094*** (0.017)	0.213*** (0.013)	0.153*** (0.018)	0.206*** (0.021)	0.229*** (0.018)
Origination Yr by Refi FEs	Y	Y	Y	Y	Y	Y	Y
MSA FE	N	N	N	N	N	N	N
Household and Housing Features	Y	Y	Y	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y	Y	Y	Y
Observations	5075	2840	3401	8805	5707	7893	7397

Notes: This table is visualized in Figure 6. The model is estimated separately by location. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that fixed effects for MSA are omitted. Standard errors are clustered by year of loan origination and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 4: Table of Long-Term Estimate Homeownership by Coborrower

	Coborrower Present	No Coborrower
2004	0.034*** (0.007)	-0.008 (0.010)
2007	0.004 (0.004)	0.007 (0.008)
2010	0.000 (0.000)	0.000 (0.000)
2013	0.379*** (0.008)	0.341*** (0.012)
2016	0.346*** (0.014)	0.257*** (0.013)
2019	0.255*** (0.010)	0.196*** (0.013)
2022	0.221*** (0.010)	0.151*** (0.011)
MSA by Origination Yr by Refi FEs	Y	Y
Household and Housing Features	Y	Y
Credit Variables	Y	Y
Lender Fixed-Effects	Y	Y
Observations	22637	18481

Notes: This table is visualized in Figure 6. The model is estimated separately by the presence of a co-borrower (spouse). Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that controls for the presence of a coborrower are omitted. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 5: Table of Long-Term Estimate Homeownership by Race and Ethnicity

	White	Black	Hispanic	Asian
2004	-0.004 (0.013)	0.019* (0.010)	0.017 (0.011)	0.050*** (0.018)
2007	0.011* (0.007)	-0.008 (0.007)	0.002 (0.008)	0.009 (0.012)
2010	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
2013	0.334*** (0.014)	0.336*** (0.013)	0.393*** (0.009)	0.375*** (0.020)
2016	0.256*** (0.015)	0.278*** (0.019)	0.361*** (0.014)	0.316*** (0.021)
2019	0.186*** (0.016)	0.182*** (0.017)	0.279*** (0.011)	0.246*** (0.030)
2022	0.138*** (0.015)	0.151*** (0.019)	0.254*** (0.012)	0.196*** (0.028)
MSA by Origination Yr by Refi FEs	Y	Y	Y	Y
Household and Housing Features	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y
Observations	14262	9344	12910	2705

Notes: This table is visualized in Appendix Figure 2. The model is estimated separately by race/ethnic group. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that controls for race are omitted. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 6: Table of Long-Term Estimate Homeownership by Income

	Low Income	Middle Income	High Income
2004	0.012 (0.009)	0.016 (0.012)	0.015 (0.013)
2007	0.015** (0.007)	0.003 (0.007)	0.018*** (0.006)
2010	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
2013	0.375*** (0.013)	0.370*** (0.015)	0.336*** (0.014)
2016	0.303*** (0.013)	0.321*** (0.015)	0.289*** (0.015)
2019	0.243*** (0.015)	0.215*** (0.014)	0.224*** (0.016)
2022	0.195*** (0.015)	0.184*** (0.015)	0.185*** (0.014)
MSA by Origination Yr by Refi FEs	Y	Y	Y
Household and Housing Features	Y	Y	Y
Credit Variables	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y
Observations	14037	13379	13702

Notes: This table is visualized in Appendix Figure 2. The model is estimated separately by terciles of HMDA-reported income at loan origination. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that controls for income are omitted. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 7: Table of Long-Term Estimate Homeownership by Generation

	Boomer or older	Gen X or younger
2004	0.005 (0.009)	0.014 (0.010)
2007	0.004 (0.006)	0.007 (0.006)
2010	0.000 (0.000)	0.000 (0.000)
2013	0.343*** (0.010)	0.378*** (0.010)
2016	0.295*** (0.012)	0.307*** (0.015)
2019	0.224*** (0.011)	0.226*** (0.012)
2022	0.182*** (0.010)	0.196*** (0.012)
MSA by Origination Yr by Refi FEs	Y	Y
Household and Housing Features	Y	Y
Credit Variables	Y	Y
Lender Fixed-Effects	Y	Y
Observations	21443	18735

Notes: This table is visualized in Appendix Figure 3. The model contains all fixed effects and controls from Column (5) of Appendix Table 2, separately by borrower age. Control variables and fixed effects are as in Column (5) of Appendix Table 2. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 8: Table of Long-Term Estimate Homeownership Conditioning On Delinquency in 2007

	Modification in 2010
2004	-0.055 (0.049)
2007	0.000 (0.000)
2010	0.402*** (0.026)
2013	0.298*** (0.049)
2016	0.274*** (0.054)
2019	0.122** (0.052)
2022	0.098* (0.056)
MSA by Origination Yr by Refi FEs	Y
Household and Housing Features	Y
Credit Variables	Y
Lender Fixed-Effects	Y
Observations	6562

Notes: This table is visualized in Appendix Figure 4. The model contains all fixed effects and controls from Column (5) of Appendix Table 2 except the credit variables in 2007, estimated for an earlier sample of distressed borrowers. Delinquencies occur in the 24 months preceding June 2007, and modifications are received by 2010. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except that 2010 variables are replaced by their 2007 counterparts and 2007 variables are replaced by their 2004 counterparts. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 9: Table of Long-Term Effects on Wealth Measures

	House Price Appreciation	Plus Home Loss 2010-2013	Plus Home Loss 2016, 2019, or 2022	Plus Home Acquisition 2016, 2019, or 2022
2004	1165 (1270)	1165 (1270)	1165 (1270)	1165 (1270)
2007	-301 (1356)	-301 (1356)	-301 (1356)	-301 (1356)
2010	696 (1605)	696 (1605)	696 (1605)	696 (1605)
2013	-705 (1614)	-28945*** (3894)	-28945*** (3894)	-28945*** (3894)
2016	-995 (1823)	11716* (6693)	10424 (6977)	10424 (6977)
2019	-1351 (2182)	42499*** (9428)	40173*** (8877)	37239*** (8872)
2022	-6201 (4375)	98807*** (13034)	89667*** (12742)	77769*** (12735)
MSA by Origination Yr by Refi FEs	Y	Y	Y	Y
Household and Housing Features	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y
Observations	19375	19375	19375	19331

Notes: This table is visualized in Figure 7. The model contains all fixed effects and controls from Column (5) of Appendix Table 2. Variable construction is described in Table 6. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 10: Table of Long-Term Effects on Vantage Score

	Vantage Score
2004	-5.924*** (1.448)
2007	-1.394 (1.189)
2010	-2.616*** (0.894)
2013	2.178* (1.174)
2016	-0.354 (1.459)
2019	2.128 (1.526)
2022	5.119** (2.051)
MSA by Origination Yr by Refi FEs	Y
Household and Housing Features	Y
Credit Variables	Y
Lender Fixed-Effects	Y
Observations	41118

Notes: This table displays regression differences of credit scores, descriptively shown in Figure 8. Control variables and fixed effects are as in Column (5) of Appendix Table 2, except Vantage score in the current or future years. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 11: Table of Long-Term Delinquency Estimates

	Credit Card 90-Day Delinquency	Auto Loan 90-Day Delinquency	Student Loan 90-Day Delinquency	Unsecured Personal Loan 90-Day Delinquency
2004	0.004 (0.005)	-0.007*** (0.002)	0.000 (0.001)	-0.001 (0.001)
2007	0.001 (0.005)	-0.003* (0.002)	-0.002 (0.002)	-0.000 (0.001)
2010	0.000 (0.007)	-0.020*** (0.005)	-0.005* (0.003)	-0.001 (0.003)
2013	0.018** (0.007)	-0.012*** (0.004)	-0.004 (0.003)	-0.000 (0.001)
2016	0.005 (0.005)	-0.003 (0.003)	-0.000 (0.002)	0.000 (0.001)
2019	0.002 (0.005)	-0.002 (0.003)	0.002 (0.002)	0.000 (0.002)
2022	-0.004 (0.003)	-0.004* (0.002)	-0.001 (0.001)	-0.001 (0.002)
MSA by Origination Yr by Refi FEs	Y	Y	Y	Y
Household and Housing Features	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y
Observations	41118	41118	41118	41118

Notes: Control variables and fixed effects are as in Column (5) of Appendix Table 2. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 12: Table of Long-Term Effects on Consumption Measures

	Credit Card Payment	Credit Card Balance	Number of Auto Loans	Auto Loan Balance
2004	0.647 (1.424)	-101.614 (209.289)	-0.019 (0.013)	242.468 (182.844)
2007	-2.923 (2.625)	-318.538 (193.079)	-0.012 (0.009)	50.329 (172.380)
2010	6.709* (3.573)	423.817*** (152.845)	-0.013** (0.006)	149.254 (134.961)
2013	0.938 (2.107)	213.993** (96.002)	0.011 (0.008)	251.325 (186.113)
2016	1.806 (2.759)	172.112 (127.685)	-0.001 (0.009)	279.390 (268.989)
2019	-0.999 (4.914)	71.331 (186.242)	-0.007 (0.009)	-325.768 (207.881)
2022	0.809 (4.964)	33.715 (200.137)	0.008 (0.008)	49.998 (301.180)
MSA by Origination Yr by Refi FEs	Y	Y	Y	Y
Household and Housing Features	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y
Observations	41118	41118	41118	41118

Notes: Control variables and fixed effects are as in Column (5) of Appendix Table 2. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 13: Table of Long-Term Effects on Neighborhood Quality

	Mobility	Percentile of Income	Percentage of Renters	Median Home Value
2004		-1.125*** (0.342)	0.001 (0.002)	-4539** (1763)
2007		-0.739** (0.309)	-0.006** (0.002)	-2721 (1966)
2010		-1.102*** (0.359)	-0.005** (0.002)	-2814* (1446)
2013	-0.168*** (0.007)	-1.280*** (0.419)	-0.007*** (0.002)	-2529 (1881)
2016	-0.126*** (0.007)	-1.063** (0.403)	-0.008*** (0.002)	116 (1745)
2019	-0.095*** (0.007)	-1.051** (0.406)	-0.006*** (0.002)	1741 (2353)
2022	-0.048*** (0.006)	-1.042** (0.428)	-0.004** (0.002)	1982 (2079)
MSA by Origination Yr by Refi FEs	Y	Y	Y	Y
Household and Housing Features	Y	Y	Y	Y
Credit Variables	Y	Y	Y	Y
Lender Fixed-Effects	Y	Y	Y	Y
Observations	41118	40578	40684	40683

Notes: Control variables and fixed effects are as in Column (5) of Appendix Table 2. Standard errors are clustered by MSA, year of loan origination, and type of loan.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$