

Essays on immigration and homeownership

by

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Abstract

This dissertation consists of three essays. The first essay examines how European immigrants to the United States between 1900 and 1930 assimilated through homeownership, a key marker of economic stability, integration, and convergence with native living standards. Using the Multigenerational Longitudinal Panel (MLP) data, I track individual households over time to account for return migration and focus on genuine gains in ownership. Cohort-level and age-cohort-period regressions show that immigrant cohorts began with substantial homeownership gaps relative to natives but made steady gains, averaging an increase of 8.1 percentage points per decade. Younger arrivals generally start with smaller gaps and close a larger share of their initial disadvantage, while regional differences among European immigrants converge to native rates by 1930. Ethnic enclave analysis shows that higher city-level concentrations of co-ethnics increase homeownership, whereas higher neighborhood-level concentrations reduce it. Panel-based transition analysis reveals that immigrants are more likely to enter and less likely to exit homeownership than natives, with these dynamics explaining part of the observed convergence, highlighting the multifaceted process of immigrant assimilation.

The second essay examines the effects of immigration shocks on native homeownership in the United States during the early twentieth century. Using full-count and linked U.S. Census data from IPUMS, together with a shift-share instrument for immigrant inflows, we examine how changes in city-level immigrant populations affect native homeownership and transitions into and out of homeownership. We find that a 5-percentage point increase in the fraction of a city's population made up of recent immigrants raises native homeownership rates by 2.28 percentage points, while a 5-percentage point increase in the change in the immigrant share increases the probability that natives transition into homeownership by 9.82 percentage points and reduces transitions out of homeownership by 4.59 percentage

points. We find suggestive evidence that immigration lowered home prices at the city level, while the estimates for rents are statistically insignificant. Together, these findings are consistent with housing costs being one possible channel through which immigration affects native housing outcomes. We also find that immigration increases household formation among natives, raising the probability that an individual who was not a household head in one census becomes a household head in the next. Newly formed households are also more likely to enter homeownership when immigration increases. Some of our results may reflect preexisting city characteristics correlated with immigration, raising concerns about the validity of the instrument in those specifications.

The third essay examines the impact of the legalization provisions of the Immigration Reform and Control Act (IRCA) of 1986 on U.S. housing markets. Using quarterly national data on immigration flows, housing demand, prices, and supply, we estimate Bayesian Structural VAR models identified using sign and narrative sign restrictions to recover the causal effects of IRCA-induced immigration shocks. We find that the legalization shock generated a persistent increase in immigration, leading to heterogeneous adjustments across housing markets. Rental markets adjust primarily through quantities, with sharp declines in vacancy rates and only modest increases in rents. In contrast, owner-occupied markets exhibit stronger responses along both quantity and price margins, including declines in vacancies, increases in single-family home sales, and sustained appreciation in house prices. Despite these demand pressures, we find little evidence of a meaningful supply response through housing permits or starts. Overall, legalization-induced immigration generated persistent housing market pressures, with adjustment occurring mainly through tighter market conditions and rising prices rather than supply expansion.

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“Did We not relieve you of your burden which [almost] broke your back?”

— Quran - Surah Ash-Sharh (The Opening-Up of the Heart) (94:2-3)

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Chapter 1

Immigrant Assimilation in Homeownership in Early 20th Century

1.1 Introduction

Housing represents a central component of household economic welfare, as it accounts for a substantial share of income and wealth ([Saiz, 2010](#)). Within this context, homeownership plays a particularly important role, allowing households to build equity through mortgage repayment and housing appreciation ([Di et al., 2007](#); [McCabe, 2018](#)), which can be used to smooth consumption during economic downturns and finance investments in education, entrepreneurship, and retirement ([Herbert et al., 2013](#)). For immigrants, homeownership carries additional significance as an indicator of long-term settlement and a marker of economic integration in the host country ([Dustmann and Mestres, 2010](#); [Owusu, 1998](#); [Ray and Moore, 1991](#)). Achieving homeownership is also closely linked to attaining middle-class status and reflects immigrants' ability to translate labor market progress into durable wealth accumulation ([Myers and Lee, 1998](#)). As a result, homeownership is commonly used in the literature as a marker of immigrant assimilation and convergence toward native-born living standards.

Immigration to the United States reached its peak in the early twentieth century, with

over 30 million European immigrants arriving during the Age of Mass Migration (1850–1913), and by 1910, foreign-born workers accounted for 22% of the U.S. labor force (Abramitzky et al., 2014). Numerous studies have examined the assimilation of these immigrants across various dimensions, including earnings and labor market integration (Abramitzky et al., 2014; Fouka, 2020; Price et al., 2020), intergenerational mobility (Abramitzky et al., 2019; Collins and Wanamaker, 2017; Parman, 2015), and language acquisition (Ward, 2020). These studies rely on algorithms that link individuals across historical censuses, creating longitudinal panel datasets that allow assimilation to be studied over time while addressing potential biases from return migration, re-migration, and mortality. Despite these advances, literature has largely overlooked homeownership, a vital aspect of an immigrant’s life cycle that reflects economic stability, integration into the host society, and convergence in living standards.

This essay studies how immigrants who arrived between 1900 and 1930 assimilated into American society through homeownership. One of the main objectives of this essay is to assess the extent to which the observed rise in immigrant homeownership over time reflects genuine economic integration rather than selective attrition due to return migration. To address this, I use the Multigenerational Longitudinal Panel (MLP) from IPUMS, which probabilistically links individuals across the full-count 1910, 1920, and 1930 U.S. censuses (the Linked Census), enabling the same individuals to be followed over time. By construction, linked individuals appear in at least two censuses, allowing the analysis to focus on those who did not return migrate. After explicitly accounting for return migration, the study examines whether assimilation patterns vary by birthplace and whether the size of ethnic enclaves at the city or neighborhood level influences immigrants’ likelihood of owning a home. A major contribution of this essay is to document how homeownership entry and exit rates differ between immigrants and natives and to show how these dynamics jointly contribute to the narrowing of the immigrant–native homeownership gap. This is made possible by the ability to directly observe changes in household socioeconomic circumstances in the MLP.

To evaluate the extent to which return migration affects assimilation, I first estimate cohort-level regressions using repeated cross-sectional data constructed from the three full-count censuses, and then repeat the same regressions using only households that are part of

the linked census. Results from both sets of regressions indicate that every immigrant arrival cohort began with a sizable homeownership gap relative to natives of the same age, but all cohorts experienced meaningful gains in homeownership over time, reflecting a clear pattern of assimilation. For example, in the repeated cross-sectional data, immigrants in the 1905–09 cohort were initially 23.5 percentage points less likely to own homes than similarly aged natives in 1910, yet by 1930 they had fully closed this gap. Controlling for city of residence reduces the measured initial gaps (for the 1905–09 cohort, from 23.5 to 12.8 percentage points; for the 1915–19 cohort, from 22.4 to 16.6 percentage points), but the pattern of convergence remains strong. Regressions using the linked census, which control for both age and city fixed effects, show that the initial gaps shrink further, particularly for later arrival cohorts (e.g., for the 1910–14 cohort, from 8.0 to 3.1 percentage points; for the 1915–19 cohort, from 16.6 to 9.2 percentage points). This pattern reflects selective return migration: households that do not attain homeownership are more likely to return migrate and therefore do not appear in the linked census. Thus, the linked sample contains a more homeowners subset of each arrival cohort, which mechanically reduces the measured initial gaps. Because the repeated cross section does not account for this selection, analyses based on it would be biased.

Moving forward with the linked census, I estimate age–cohort–period regressions to measure how immigrant homeownership evolves over time, accounting for age, cohort, age-at-arrival, and census year effects. Identification follows standard assumptions in the literature, including common period effects across immigrants and natives ([Borjas, 1985](#)) and common age profiles ([Friedberg, 1992](#)). The results show that, on average, immigrants increase their likelihood of homeownership by 8.1 percentage points per decade.

Heterogeneity analysis also reveals that immigrants who arrived at younger ages begin with smaller homeownership disadvantages relative to similarly aged natives than those who arrived at older ages. For instance, within the 1905–09 cohort, immigrants arriving between ages 25–34 had an initial gap of 12.2 percentage points in 1910, whereas those arriving between ages 45–54 faced a gap of 23.9 percentage points, controlling for city of residence. Nevertheless, assimilation over time is broadly similar across age-at-arrival groups,

and younger immigrants close a larger share of their initial gap.

Focusing on European-born immigrants, who comprised the majority of arrivals to the United States in the early twentieth century, reveals pronounced differences in initial homeownership disadvantage by region of origin and consistent assimilation over time. In 1910, Northern Europeans held a slight advantage over similarly aged natives, with homeownership rates two percentage points higher, whereas immigrants from the United Kingdom and Ireland faced the largest initial disadvantage of 12.6 percentage points. Over the subsequent two decades, all regional groups steadily increased their homeownership, with Southern Europeans entering more rapidly than others. By 1930, every European immigrant group had effectively caught up to native homeownership rates, highlighting substantial convergence across regions.

These regional trends in assimilation raise important questions about the role of immigrant communities and neighborhood context in shaping economic outcomes. To further understand the role of immigrant communities in shaping housing outcomes, I examine how the size of ethnic enclaves relates to homeownership. Two definitions of enclave size are used: at the city level, enclave size is defined as the share of a city's population made up of immigrants from a specific birthplace; at the enumeration-district level, it is defined as the share of a neighborhood's population (within a city) from a particular birthplace. Higher concentration of immigrants from a given birthplace at the city level is associated with higher homeownership rates. In contrast, greater co-ethnic concentration within neighborhoods or districts is associated with lower homeownership, even when comparisons are made within the same city and birthplace. Overall, the results show that the relationship between ethnic concentration and homeownership depends critically on the geographic scale at which enclave exposure is defined.

Lastly, I exploit the panel structure of the data to study immigrant homeownership assimilation through direct transitions into and out of ownership rather than changes in average ownership rates alone. I find that immigrants are 2.6 percentage points more likely than similarly aged natives to enter homeownership and 1.5 percentage points less likely to exit. Combining these transition probabilities with initial ownership differences shows

that immigrant homeownership rises by about 11.9 percentage points per decade relative to natives, with roughly 19% of this convergence driven by higher entry and lower exit rates and the remaining 81% driven by immigrants starting with much lower ownership rates. I then examine how ethnic enclave size affects these transitions and find that larger enclaves at both the city and neighborhood levels slightly reduce entry into homeownership and modestly increase exit. However, these enclave effects are quantitatively small, typically accounting for less than one percent of mean transition probabilities, indicating that enclave size plays only a minor role in shaping homeownership dynamics relative to entry-exit behavior and initial conditions.

The remainder of the essay is structured as follows. Section 1.2 reviews the related literature, and Section 1.3 describes the data and sample construction. Section 1.4 documents patterns of homeownership assimilation, while Section 1.5 examines heterogeneity by birthplace. Section 1.6 analyzes the role of ethnic enclaves in shaping immigrant homeownership. Section 1.7 studies how differences in entry into and exit from homeownership between immigrants and natives contribute to assimilation over time. Section 1.8 concludes.

1.2 Literature Review

There are several reasons why homeownership may be preferred over renting. Owning a home can promote economic mobility, enhance feelings of stability and security, symbolize status attainment, and provide opportunities to live in more desirable neighborhoods (McCabe, 2018).

Homeownership is widely acknowledged as a crucial factor in building wealth and ensuring financial stability for American families. Di et al. (2007) note that owning a home promotes upward mobility by functioning as a long-term investment; as property values rise, homeowners accumulate equity that can be used to support opportunities like education, starting a business, or retirement. Likewise, McCabe (2018) emphasizes that the capacity to build equity, both by reducing mortgage debt and benefiting from property appreciation, makes purchasing a home one of the most significant financial decisions for many house-

holds. This accumulated equity often serves as a financial cushion during times of economic difficulty.

The impact of homeownership on wealth accumulation varies by income level. [Turner and Luea \(2009\)](#) find that between the mid-1990s and early 2000s, each additional year of homeownership increased household wealth by approximately \$15,000 annually for high-income households. In contrast, for low- and middle-income households, the annual gain ranged from \$6,000 to \$10,000 per year. Even during economic downturns, homeownership can contribute to wealth accumulation for certain groups. [Herbert et al. \(2013\)](#) show that despite the sharp decline in housing prices and the rise in foreclosures beginning in 2007, homeownership remained an important source of household wealth—particularly for lower-income and minority households. While the Great Recession introduced significant risks due to housing market volatility, many homeowners were able to retain their homes and build substantial wealth, at least through 2009.

Homeownership also impacts individuals’ sense of freedom and strengthens feelings of financial stability and security. Unlike renters, homeowners have the assurance of avoiding involuntary moves and enjoy greater autonomy to customize their living environment ([Zumbro, 2014](#)). Renters depend on the rental market and must contend with annual fluctuations in rent prices, while homeowners secure a long-term asset that offers stable housing for a fixed upfront cost. Beyond typical investments, owning a home provides ongoing benefits, such as shielding owners from rising rental costs. For example, when rents increase, all households face higher future housing expenses; however, homeowners can benefit from a corresponding rise in their property’s value, which offsets these costs by increasing the home’s annual returns ([Sinai and Souleles, 2005](#)).

Research highlights that beyond financial benefits, homeownership is closely linked to social status and subjective well-being. It often serves as a meaningful symbol of personal achievement and upward mobility within many cultures. [Diaz-Serrano \(2006\)](#) points out that owning a home strongly signals social standing, which contributes to greater life satisfaction. Moreover, [Rohe et al. \(2002\)](#) find that this status signaling plays a significant role in enhancing happiness, while [Zumbro \(2014\)](#) shows that homeownership improves subjective

well-being even when controlling for the quality and size of the home.

Building on its role as a symbol of social status, homeownership also profoundly influences how individuals perceive themselves, their homes, and their communities. The process of buying a home often enhances confidence, self-esteem, and overall life satisfaction by fostering a strong emotional connection to the property, fueled by pride and the enjoyment of ownership (Dietz and Haurin, 2003). Empirical evidence supports these effects: for example, Balfour and Smith (1996) found that low-income families involved in a lease-purchase program in Cleveland experienced increased self-esteem and a greater sense of security. Likewise, Rakoff (1977) observed that many people equate homeownership with personal success, linking it to feelings of accomplishment and happiness. These insights emphasize the complex and multifaceted ways in which homeownership contributes to social standing, well-being, and individual identity.

1.2.1 Literature on Immigrant Homeownership in the U.S.

Homeownership can be an indicator of immigrants' long-term stability and their intention to settle in the host country. Immigrants planning to return to their home countries often focus on investments abroad, while those aiming to stay are more likely to invest in housing in the host nation (Dustmann and Mestres, 2010). Homeownership represents a major financial commitment and serves as a symbol of integration into the host society, signifying an important milestone on the path to stability and assimilation (Owusu, 1998; Ray and Moore, 1991). Homeownership is also associated with attaining middle-class living standards, making it a valuable measure of immigrants' progress and convergence with native-born populations (Myers and Lee, 1998).

Research consistently shows that immigrants generally have lower homeownership rates compared to native-born individuals (Borjas, 2002; Elmelech, 2004). Several factors contribute to this gap. Many immigrants arrive with limited financial resources and struggle to find suitable employment. They often take lower-paying jobs than natives with similar skills, especially during their early years of settlement (Borjas, 2015). Immigrants also face

challenges like restricted access to credit and banking services. For example, native-born Americans are 13% more likely to have access to banking than immigrants, which impacts their ability to invest in housing (Bohn and Pearlman, 2013). The homeownership gap is not uniform and varies significantly across immigrant sub-populations. Differences arise based on factors such as country of origin, ethnicity, race, and the region in which immigrants settle (Painter et al., 2003; Yu, 2006).

Researchers have pointed to a range of factors that help explain disparities in housing consumption between immigrants and natives. These include institutional barriers such as credit constraints (Bohn and Pearlman, 2013), regional influences like housing costs and the overall cost of living (McConnell and Akresh, 2010; Schill et al., 1998), and a variety of socioeconomic characteristics—including age, marital status, education, income, wealth, and citizenship status (Chatterjee and Zahirovic-Herbert, 2011; Mundra and Uwaifo Oyelere, 2017).

1.2.2 Literature on Immigrant Assimilation in the U.S.

While this essay focuses on homeownership assimilation, prior research in economics has explored immigrant integration along multiple dimensions. A large body of literature has examined immigrant earnings assimilation, beginning with the influential study by Chiswick (1978), which used data from the 1970 U.S. Census and found that immigrants experienced rapid earnings growth, eventually catching up to and even surpassing the earnings of native-born workers. Borjas (1985) introduced a more nuanced cohort-based method and found lower assimilation rates, attributing the difference to changes in the composition of immigrant cohorts over time. This approach shaped subsequent work (Abramitzky et al., 2014; Borjas, 2015; Lubotsky, 2007), which continues to refine how earnings trajectories are estimated and interpreted.

In addition to earnings, scholars have investigated cultural assimilation using various markers, including English proficiency (Bleakley and Chin, 2004; Chiswick, 1991), intermarriage (Alba and Golden, 1986; Lieberman and Waters, 1985; Pagnini and Morgan, 1990),

naming practices ([Abramitzky et al., 2016](#)), religious affiliation ([Gagliarducci and Tabellini, 2022](#)), and residential patterns ([South et al., 2005](#)). For example, language acquisition has been linked to both cultural and economic outcomes, while studies on naming patterns and intermarriage shed light on how immigrants navigate identity and integration over time. Religion and neighborhood choice have also been used to trace immigrants' social incorporation. Together, these studies highlight that assimilation is a multidimensional process shaped by economic, linguistic, and social factors. Homeownership assimilation fits within this broader framework as an important marker of both economic stability and long-term settlement.

1.2.3 Literature on Immigrant Homeownership Assimilation in the U.S.

The research on immigrant homeownership assimilation shows mixed findings about whether homeownership rates increase or decrease with longer stays in the host country. Some studies suggest that the gap between immigrant and native homeownership widens over time, while others indicate it narrows. These differences are often due to variations in datasets, time periods, and regional contexts, highlighting how research outcomes are shaped by specific factors.

For example, [Myers and Lee \(1998\)](#) observe that immigrants in southern California make significant progress in homeownership. Among Asians, and to a lesser degree whites, new immigrants quickly achieve high homeownership rates, sometimes surpassing native-born residents of the same age. In contrast, Hispanic immigrants start with low homeownership rates but show rapid growth over two decades, advancing faster than native-born individuals of similar age, though they never completely close the gap.

[Borjas \(2002\)](#), using census data from 1980 to 2000, finds that the homeownership gap between immigrants and natives has widened over time. Similarly, [Haan \(2007\)](#) reports that homeownership rates among immigrant groups in both Canada and the U.S. have slowed, suggesting that assimilation in homeownership is decelerating. On the other hand, [Yu and Myers \(2007\)](#), studying the greater Los Angeles area, find that homeownership increases

significantly over immigrants' life course, emphasizing the importance of considering factors like birth cohorts and aging in such analyses. [Painter and Yu \(2008\)](#) further show that immigrants moving to newer gateway cities have lower homeownership rates, and this gap persists for 15 to 20 years.

These findings underscore that the path to homeownership for immigrants is complex and influenced by multiple factors such as region, period, and economic conditions. Understanding these dynamics requires careful consideration of both individual and contextual influences on immigrant integration.

1.2.4 Literature on the use of Linked Census Data to Study Assimilation in the U.S.

Early research on assimilation relied heavily on cross-sectional data analyzed through linear regression models. A major advancement came with [Borjas \(1985\)](#), who introduced the synthetic panel data method, which became the foundation for many assimilation studies. In the context of homeownership assimilation, [Myers and Lee \(1998\)](#) refined this approach further by developing the double cohort method. Despite these advancements, these methods face criticism for their inability to address issues like return migration, multiple migrations, and mortality over time. However, due to the lack of robust panel datasets, these models remain widely used in assimilation studies.

The release of digitized full-count censuses (1850–1950) by the IPUMS opened new possibilities for creating individual-level panel data. Researchers have designed algorithms to link individuals across census years, enabling them to track changes in socioeconomic circumstances over time. If linking errors are minimal, this approach allows for a consistent analysis of individual trajectories.

[Abramitzky et al. \(2021\)](#) examine the challenges of linking individuals across censuses, comparing automated algorithms with manual (hand) linking methods. They outline key goals for any matching method: accuracy (minimizing false positives), efficiency (maximizing true matches), representativeness (ensuring the linked sample reflects the population), and

feasibility (ease of implementation given computational constraints). Their findings reveal that automated methods produce low false positive rates (5%) and perform as well as hand-linking while being significantly faster. This study underscores the potential of automated matching to improve research on assimilation while maintaining accuracy and efficiency.

These matched longitudinal datasets have enabled scholars to explore questions previously out of reach. Research using these datasets spans a wide range of topics, including the selection patterns of immigrants ([Abramitzky et al., 2012](#); [Kosack and Ward, 2014](#)), immigrant earnings assimilation ([Abramitzky et al., 2014](#); [Fouka, 2020](#); [Price et al., 2020](#)), and variations in intergenerational mobility across groups and over time ([Abramitzky et al., 2019](#); [Collins and Wanamaker, 2017](#); [Feigenbaum, 2015, 2018](#); [Parman, 2015](#)). Scholars have also examined topics like English fluency and language acquisition in the early 20th century U.S. ([Ward, 2020](#)), the long-term effects of government programs and historical events ([Aizer et al., 2016](#); [Bleakley and Ferrie, 2016](#); [Eli and Salisbury, 2016](#); [Eriksson, 2019](#); [Parman, 2015](#); [Salisbury, 2014](#)), geographic and economic mobility of African Americans ([Collins and Wanamaker, 2014, 2015](#); [Hornbeck and Naidu, 2014](#)), and returns to human capital ([Feigenbaum and Tan, 2019](#)). These studies highlight the immense value of linked longitudinal data in deepening our understanding of historical and contemporary trends in social and economic assimilation.

1.2.5 Literature on 20th Century Homeownership in the U.S.

The literature on homeownership in early 20th century America is limited, with only a few studies addressing topics relevant to this area. [Fetter \(2013, 2014\)](#) examines the surprising rise in non-farm homeownership among the U.S. population during the mid-20th century. His findings show that much of this increase was due to a decline in the age at which individuals became homeowners. Key factors identified to explain this trend include changes in population age structure, rising real incomes, reductions in the cost of homeownership due to higher marginal tax rates in the early 1940s, improved mortgage financing terms, reduced transportation costs, evolving city structures, and assistance programs for the elderly. While

Fetter’s work provides valuable insights, it does not directly address the focus of this study, which explores immigrant homeownership assimilation between 1910 and 1930.

A more closely related study is by [Sutch \(2012\)](#), which investigates immigrant assimilation in non-farm homeownership from 1890 to 1930. However, our research differs from Sutch’s study in four key ways. First, Sutch employs a synthetic cohort approach using cross-sectional data, whereas I use linked census data alongside repeated cross sections. Second, his analysis relies on samples drawn from manuscript enumeration schedules, while I use full-count census data. Third, his study spans 1890–1930, whereas I focus on 1910–1930. Fourth, Sutch examines only non-farm homeownership, whereas I consider both farm and non-farm homeownership, reflecting the settlement patterns of many immigrants in rural areas.

Additionally, [Tsymbal \(2023\)](#) offers a recent contribution through a master’s thesis comparing homeownership gaps between natives and immigrants in 1910 and 2019. His analysis focuses on the role of English proficiency in homeownership outcomes. While Tsymbal’s study is broader in scope, it provides context for understanding the historical and contemporary dimensions of immigrant homeownership assimilation.

1.3 Data Sources and Sample Construction

Three sets of full-count U.S. censuses (1910, 1920 and 1930) were obtained from the IPUMS website for the purpose of this study. Although the 1940 full-count census is available, it is not included because it does not record immigrants’ year of arrival which is a vital variable for this study.

The literature defines the household as the unit of analysis, classifying a household as immigrant if the household head is foreign-born, and as native otherwise. Commonly, studies exclude individuals living in group quarters such as institutions, dormitories, or military barracks, and often restrict the sample to male-headed households ([Borjas, 2002](#)). To implement sample restrictions, I combined data from all three censuses into a single pooled dataset. This dataset includes both individuals who can be linked across censuses and those

who cannot.

For the repeated cross-sections, these restrictions are straightforward. From all three census years, I retain male household heads who are between 18 and 74 years of age and are not living in group quarters.

However, additional care is needed for individuals who are linked across multiple census years. For example, a person may be recorded as the head of a household in one census but not in another. To ensure consistency, I retained only those individuals who are listed as household heads in all years in which they are linked.

Similarly, for linked household heads, their group quarters status can vary across census years. For example, a person may be classified as living in a group quarter in one census but in a household in another. I therefore retained only those household heads who are consistently classified as living in a household in all years in which they are linked.

I also observed instances where the recorded sex of a linked household head differed across censuses, likely due to errors in the linkage process. To address this, I kept only those linked household heads whose sex remained consistent across all linked census years. In addition to these restrictions, I excluded all household heads who were younger than 18 or older than 74. For linked individuals, the age restriction ensured that household heads were at least 18 years old in their first observed year and no older than 74 in their final observed year.

Lastly, I excluded individuals born in non-contiguous U.S. territories. The final sample therefore includes only native-born individuals from the 48 contiguous U.S. states and foreign-born individuals residing in those same states.

To further address concerns raised in the literature regarding assimilation bias, I focus only on immigrant household heads who arrived in the U.S. as adults (age 18 or older). [Borjas \(2002\)](#) points out that, in repeated cross sections, immigrants migrating as children in one census may end up being included as adults in any of the subsequent censuses and their assimilation pattern may be different from those who migrate as adults. I, therefore, limit the analysis to only include household heads who arrived in the U.S. as adults (18 years or older) in all three censuses. Additionally, to focus on the assimilation patterns of more recent immigrants, I exclude immigrant household heads who arrived before 1900.

By 1910, these individuals would have resided in the U.S. for over 10 years and may have already completed much of their economic assimilation, making them less informative for the dynamics of interest in this study.

An important aspect of this study is to compare the findings from the linked census data with those from repeated cross-sections data, to highlight the extent of selectivity bias due to mortality and return migration in the repeated cross-sections data. The repeated cross-sections dataset is obtained by pooling observations from the 1910, 1920, and 1930 censuses, and it aims to represent a typical cross section of native and immigrant households at each point in time. In contrast, the linked census dataset includes only those individuals who can be linked across at least two census years and is designed to follow the same native and immigrant households over time.

For the linked census, I use the IPUMS MLP data. It employs a probabilistic record linkage method to connect individuals across full-count census databases, using an open-source software known as Hlink. The linking process follows a two-step approach: Step I identifies individual links between two censuses based on demographic and name similarity, using a logistic regression model trained on historical data. Step II builds on these links to match remaining household members, refining connections using additional contextual characteristics. The methodology adapts to different census years by calibrating linking features based on available data, ensuring precision and minimizing selectivity bias. For further details see [Helgertz et al. \(2022\)](#).

Each full-count census includes a historical identification key (hik) assigned to each linked household and its members, generated through the two-step process described above. The household identification key is then used to link households across the three censuses to construct the panel data. Every household with an identification key may not necessarily be linked to all available censuses. In the dataset, households are linked across at least two censuses. The permutations of links that are obtained in this sample were: (i) households in 1910 census linked to 1920 census, (ii) households in 1910 census linked to 1930 census, (iii) households in 1920 census linked to 1930 census and, (iv) households in 1910 census linked to both 1920 and 1930 censuses. Thus, the linked sample may contain some households that

are linked in non-consecutive years (1910 and 1930). To be able to follow households across consecutive censuses, individuals linked across non-consecutive censuses were excluded.

To study homeownership assimilation over time, I divide immigrants into six arrival cohorts based on half-decade intervals ranging from 1900–1904 to 1925–1929. This approach enables me to examine how each group’s homeownership evolves as they age and spend more time in the United States. Tracking these cohorts across the 1910, 1920, and 1930 censuses highlights both the pace and extent of their economic integration during this period.

In addition to arrival cohort, I also account for immigrants’ age at migration. I classify immigrants into five age-at-arrival groups: 18–24, 25–34, 35–44, 45–54, and 55–64. The age at which immigrants arrive affects both their likelihood of owning a home upon arrival and the pace at which they narrow the ownership gap with comparable natives over time. For example, immigrants who arrive at younger ages (18–24) tend to be more similar to their native-born counterparts, as both groups typically exhibit low homeownership rates at that stage of life. In contrast, those who migrate in middle age (e.g., 40–44) often face a larger ownership gap, since many natives in that age group have already entered the housing market, while recently arrived immigrants are less likely to have done so. Therefore, in the forthcoming regression analysis, I study cohort effects while accounting for age at migration, which is essential for accurately interpreting both the size of initial homeownership gaps and the trajectory of assimilation across different immigrant arrival cohorts.

1.4 Evidence on Homeownership Assimilation

Before I discuss the formal econometric specification and show the results, I first present the evidence in the raw data. Table 1.1 reports raw homeownership rates by age group, census year, and arrival cohort for natives and immigrants in both repeated cross-sectional data and linked census data.

To make fair comparisons of homeownership between immigrants and natives, it is important to compare them at similar points in their life cycle because the likelihood of owning a home rises with age. For immigrants, in addition, the increase in age also means they

spend more time in the host country, which can affect their likelihood of homeownership.

Table 1.1: *Age Adjusted Homeownership of Natives and Immigrants by Cohort of Arrival*

Cohort	Age Group	Repeated Cross Sections			Linked Census		
		1910	1920	1930	1910	1920	1930
Natives	25–34 in 1910	30.6%	45.0%	56.3%	34.9%	49.8%	61.8%
	35–44 in 1910	46.1%	55.1%	65.7%	51.9%	61.2%	70.7%
	45–54 in 1910	57.2%	64.5%	–	64.5%	70.4%	–
	25–34 in 1920	–	29.9%	45.3%	–	32.1%	50.3%
	25–34 in 1930	–	–	27.1%	–	–	36.5%
Immigrants: 1900–1904 Arrivals	25–34 in 1910	17.1%	38.6%	59.4%	20.8%	43.1%	64.6%
	35–44 in 1910	23.4%	42.5%	61.6%	29.0%	48.9%	67.5%
	45–54 in 1910	28.2%	44.4%	–	36.5%	52.5%	–
Immigrants: 1905–1909 Arrivals	25–34 in 1910	10.3%	32.8%	56.4%	13.6%	38.0%	62.3%
	35–44 in 1910	15.5%	37.2%	58.5%	21.1%	44.5%	64.8%
	45–54 in 1910	20.0%	39.2%	–	28.5%	48.4%	–
Immigrants: 1910–1914 Arrivals	25–34 in 1920	–	18.2%	45.3%	–	21.5%	54.5%
	35–44 in 1920	–	24.2%	50.3%	–	31.4%	59.2%
	45–54 in 1920	–	26.7%	51.3%	–	37.8%	61.6%
Immigrants: 1915–1919 Arrivals	25–34 in 1920	–	13.7%	33.8%	–	19.0%	48.0%
	35–44 in 1920	–	17.4%	40.4%	–	26.3%	54.2%
	45–54 in 1920	–	20.1%	42.7%	–	32.5%	55.4%

Notes: The figures show average homeownership rates by age group and immigrant arrival cohort. The repeated cross-sectional averages are constructed from the 1910, 1920, and 1930 full-count Census data. Each row follows the same arrival cohort across census years, but the cohort composition may change from one census to the next. The linked Census averages are constructed using only households who are linked to at least one subsequent census. Each row also follows the same arrival cohort across census years, but the cohort composition remains the same because the same households are followed over time.

Three key patterns can be observed in the data. First, each immigrant arrival cohort starts with lower homeownership rates than natives of the same age. For example, in 1910, in the repeated cross-sectional data, immigrants aged 25–34 of the 1905–09 arrival cohort had a homeownership rate of 10.3%, compared with 30.6% for natives of the same age, a gap of 20.3 percentage points.

Second, within the same arrival cohort, older immigrants start out with a larger homeownership gap relative to natives of the same age compared to younger immigrants. For instance, among immigrants who arrived between 1905–09, the initial homeownership gap with similarly aged natives was 30.6 percentage points for those aged 35–44 and 37.2 per-

centage points for those aged 45–54. This means that, within the 1905–09 arrival cohort, immigrants aged 45–54 face an initial gap that is 16.9 ($37.2 - 20.3$) percentage points larger than those aged 25–34.

Third, all immigrant cohorts show clear evidence of assimilation. For example, among immigrants aged 25–34 of the 1905–09 arrival cohort, homeownership rose by 22.5 percentage points, from 10.3% in 1910 to 32.8% in 1920. In comparison, during the same period, homeownership among natives of the same age group increased by only 14.4 percentage points, thus this group of immigrants closed their initial homeownership gap by 8.1 percentage points, or by over one-third. This pattern appears consistent across other immigrant arrival cohorts and age groups as well, even though the exact magnitudes differ. These marked increases in homeownership rates among immigrants across age groups and arrival cohorts, compared with natives of similar age, indicate that immigrants are closing the initial homeownership gap.

Table 1.1 also shows that homeownership rates for both natives and immigrants are consistently higher in the linked census than in the repeated cross-sectional data. For example, in 1910, 46.1% of natives aged 35–44 owned homes in the repeated cross-sectional data, while 51.9% of the same group were homeowners in the linked census. Similarly, 15.5% of immigrants aged 35–44 in the 1905–09 arrival cohort were homeowners in the repeated cross-sectional data, compared with 21.1% in the linked census for the same year.

There are two important reasons for this pattern. First, if homeownership is related to the linking process and the likelihood of being linked increases with homeownership, the linked data will include more homeowners, regardless of the immigrant status. Second, for immigrants, this pattern is also explained by selective return migration. If immigrants who do not achieve homeownership are more likely to leave the United States, they are less likely to appear in the linked census, which raises the average homeownership rate among the immigrants who remain.

Return migration can therefore bias the results in the repeated cross-sectional data by changing the composition of immigrant arrival cohorts over time (Beenstock et al., 2010; Borjas and Bratsberg, 1994), meaning the same arrival cohort is not consistently tracked

across censuses. Since the linked census only includes immigrants who have not returned and have been in the country for at least a decade, it allows me to work with a sample where the cohort composition remains stable. This emphasizes the importance of using linked samples to measure assimilation more accurately, free from bias due to return migration.

1.4.1 Inverse Probability Weights and Linked Census

Using inverse probability weights (IPWs) in the linked sample is important because the process of linking individuals across census years is not random. Certain individuals such as those with uncommon names, more stable demographic characteristics, or native-born status are more likely to be successfully linked. As a result, the final linked sample tends to overrepresent some groups and underrepresent others, especially immigrants and renters. This selection bias means that unweighted estimates based on the linked sample may not accurately reflect the characteristics or trends of the full population.

IPWs correct for this problem by reweighting observations based on their estimated probability of being linked. Individuals who are underrepresented in the linked sample receive higher weights, while those who are overrepresented receive lower weights. Applying these weights helps to recover population-representative estimates and ensures that analyses such as trends in homeownership or assimilation are not biased by the non-random nature of the linkage process.

While the linked census provides a clearer view of immigrant assimilation by tracking the same individuals over time, it is important to emphasize that the figures reported in this, and subsequent sections are based on a subset of individuals who could be successfully linked across censuses. Because linkage success is not random, the resulting sample may differ systematically from the full census population potentially leading to biased estimates.

At this stage, no adjustments have been applied to correct for this selection bias, so these results should be interpreted with caution. To address this limitation, I am currently working on a revised draft that applies IPWs to the linked sample. These weights adjust for differences in linkage probabilities and help ensure that the results are more representative of

the overall population, thus improving the reliability of conclusions drawn about immigrant assimilation

1.4.2 Cohort Effects and Homeownership Assimilation

Up to this point, the analysis has focused on descriptive evidence of assimilation. This section shifts to an econometric approach to estimate cohort effects more formally. To do so, I adopt a modified version of the model presented in Borjas (1985):

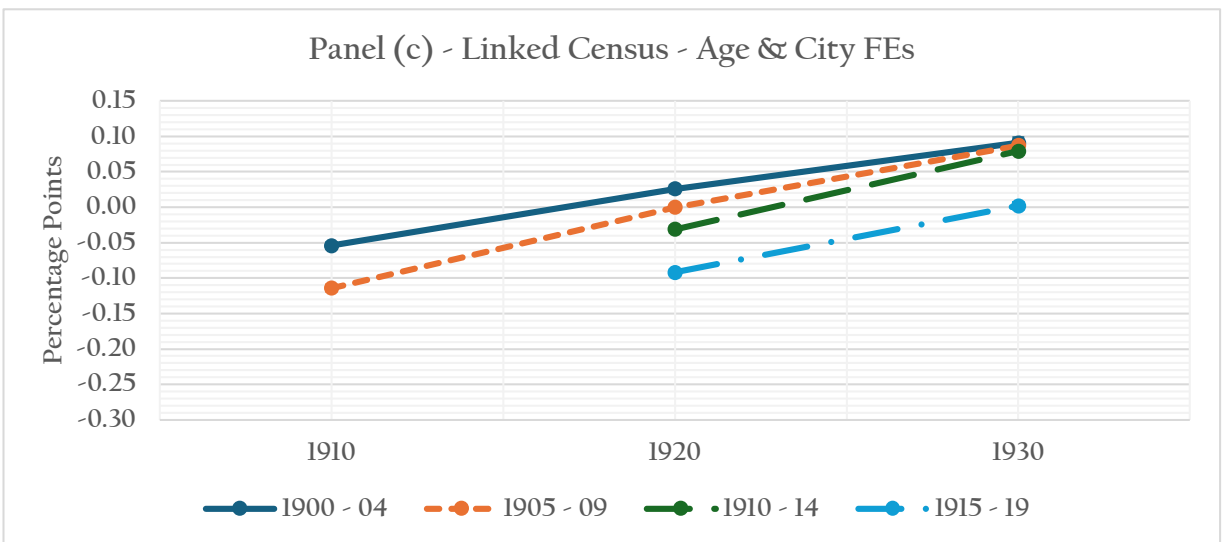
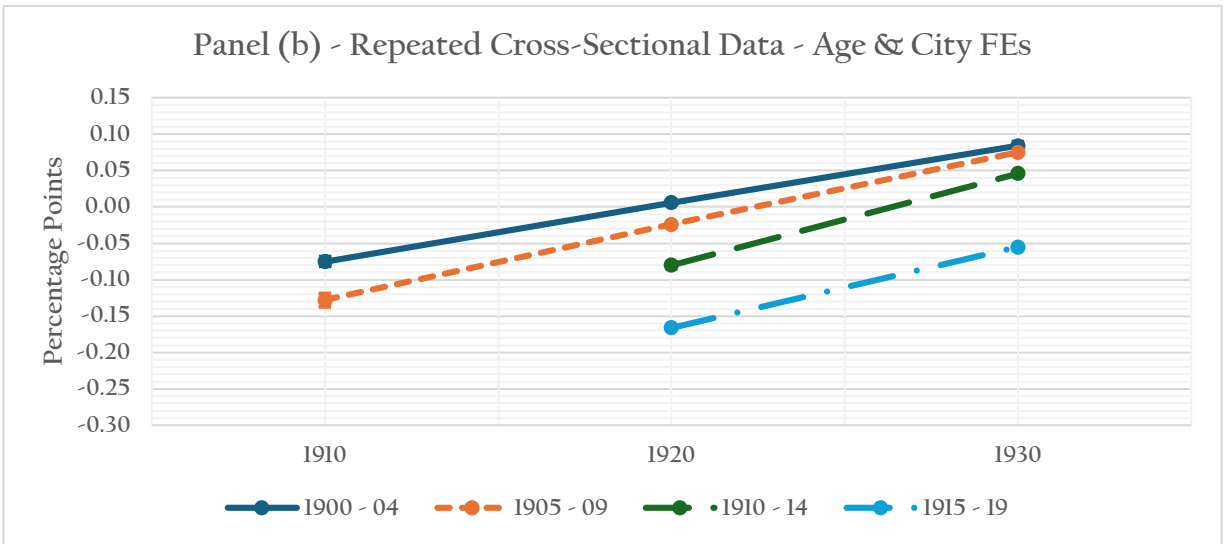
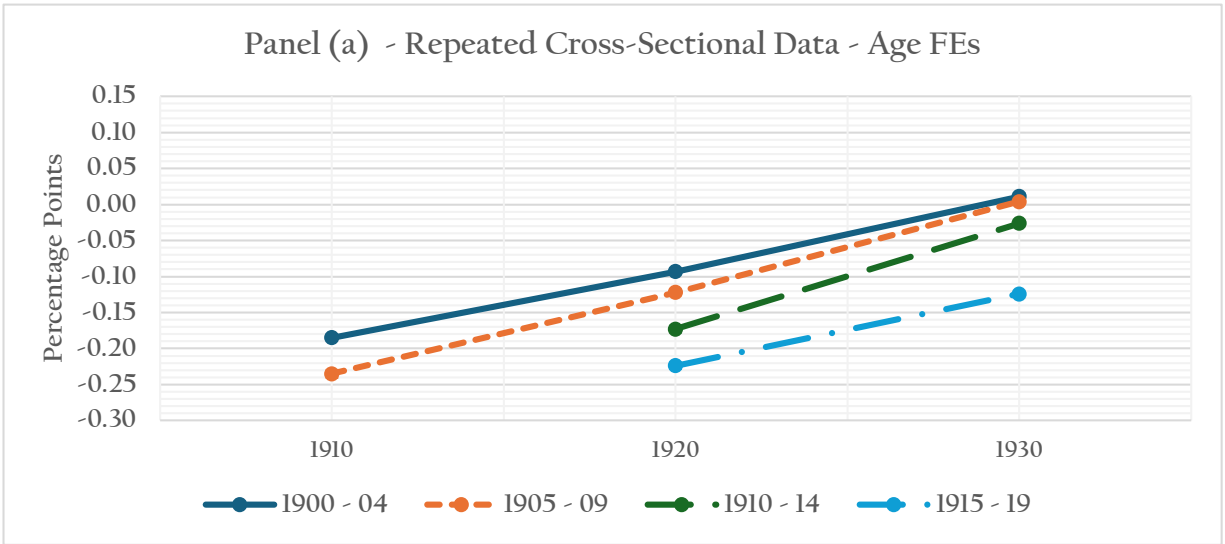
$$O_h = \sum_c \phi_c C_{hc} + Z'_h \gamma + \varepsilon_h \quad (1)$$

Where O_h is a binary indicator equal to 1 if household h owns its home and 0 if it rents. C_{hc} is a set of indicators for arrival cohorts, taking the value 1 if householder h belongs to cohort c . These indicators are defined only for immigrants, so at least one $C_{hc} = 1$ for immigrants, and all indicators equal 0 for natives. The coefficients ϕ_c therefore capture the homeownership gap between immigrants of arrival cohort c and natives. The vector Z_h contains household-level control variables such as the age of the householder and their city of residence. The model is estimated separately for each census year (1910, 1920, and 1930). To demonstrate the effect of return migration on estimates of cohort effects, I estimate this equation using both repeated cross-sectional and linked census data.

Two different specifications of equation (1) were estimated for repeated cross-sectional data. The first specification only includes the age fixed effects to control for the age of the household head while the second specification also includes the city of residence fixed effects. Age fixed effects are included to compare natives and immigrants at the same point in the lifecycle. City fixed effects are included because a higher proportion of immigrants tend to locate themselves in urban areas compared to natives, and homeownership rates are lower in urban than rural areas. [Borjas \(2002\)](#), for example, establishes that much of the differences in homeownership rates between immigrants and natives between 1980 – 2000 can be explained by the differences in their location choices.

The results of the two specifications are shown in Figure [1.1](#), where I plot the ϕ_c coeffi-

Figure 1.1: *Cohort Effects*



cients. Panel (a) presents the specification that controls only for age fixed effects. It reveals two important patterns. First, every arrival cohort begins with a homeownership disadvantage relative to natives of the same age. For example, in 1910, immigrants of the 1905–09 arrival cohort were 23.5 percentage points less likely to be homeowners than similarly aged natives. Likewise, in 1920, immigrants from the 1915–19 cohort were 22.4 percentage points less likely to own homes compared to natives of the same age.

Second, all arrival cohorts experienced substantial assimilation over time, and the rate of this assimilation is similar across cohorts. For instance, the initial homeownership gap for the 1905–09 cohort narrowed to 12.2 percentage points by 1920 and further improved to about 0.4 percentage points higher than natives in 1930. Thus, over these two decades, this group of immigrants fully closed their initial homeownership gap. Similarly, the 1915–19 cohort reduced its initial gap to 12.4 percentage points by 1930, closing more than half of its original disadvantage. Over these two decades, these groups of immigrants effectively eliminated their initial homeownership gaps, reflecting full assimilation.

Accounting for city of residence fixed effects in addition to age fixed effects produces the results shown in panel (b) of the figure. The initial gaps for each arrival cohort are noticeably smaller in this specification. For example, the initial gap between immigrants of the 1905–09 arrival cohort and similarly aged natives within the same city is 12.8 percentage points, compared with 23.5 percentage points in the previous specification. Likewise, for the 1915–19 arrival cohort, the initial gap with similarly aged natives within the same city falls to 16.6 percentage points, down from 22.4 percentage points in the earlier specification. Nevertheless, the assimilation pattern still shows a substantial reduction in the initial homeownership gap for all immigrant cohorts over time, even within cities.

To assess the impact of return migration on cohort effects, I estimate equation (1) using only households in the linked census data, with a specification that accounts for both age fixed effects and city of residence fixed effects. The results from this specification are shown in panel (c) of Figure 1.1. Comparing panel (c) to panel (b), the initial gaps for the first two arrival cohorts (1900–04 and 1905–09) are slightly lower in panel (c), while for the 1910–14 and 1915–19 cohorts, the reduction in the initial homeownership gap is substantial.

For example, the initial homeownership gap for immigrants in the 1910–14 arrival cohort, compared with similarly aged natives within the same city, shrinks from 8.0 percentage points in the repeated cross section to 3.1 percentage points in the linked census. Likewise, for the 1915–19 arrival cohort, the initial gap decreases from 16.6 percentage points in the repeated cross-sectional data to 9.2 percentage points in the linked census.

This pattern provides direct evidence of selective return migration: immigrants who do not attain homeownership are more likely to leave the United States and therefore do not appear in the linked census. As a result, the linked sample contains a more homeowners subset of each arrival cohort, which mechanically reduces the measured initial gaps. Because the repeated cross section does not account for this selection, analyses based on it would be biased.

Given this evidence of selective return migration, all subsequent analysis uses the linked census.

1.4.3 Within - Cohort Age-at-Arrival Effects and Homeownership Assimilation

Table 1.1 showed that, within each census year and within each arrival cohort, younger immigrants had smaller homeownership gaps with similarly aged natives than older immigrants. This suggests that immigrants who arrived at younger ages began with a smaller homeownership disadvantage relative to natives of the same age. To formally examine this pattern, I estimate the following regression model separately for each arrival cohort and for each census year:

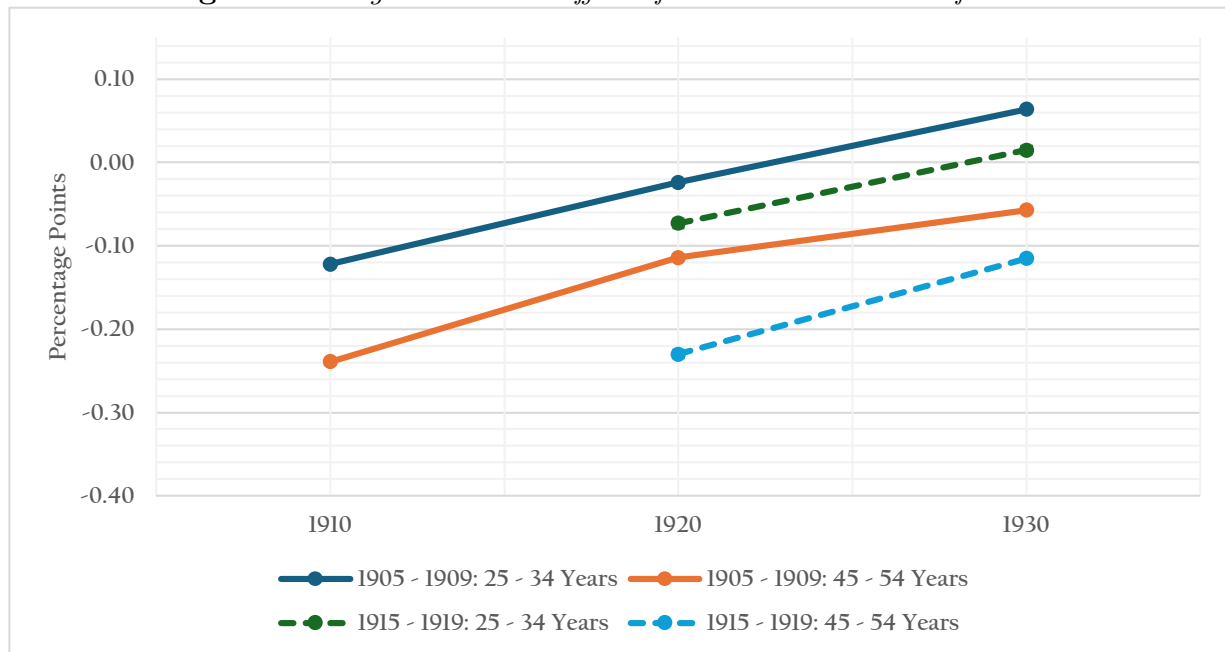
$$O_h = \sum_m \delta_m M_{hm} + Z'_h \gamma + \varepsilon_h \quad (2)$$

Where O_h is a binary indicator equal to 1 if household h owns its home and 0 if it rents. M_{hm} is a set of indicators for age-at-arrival groups, taking the value 1 if householder h belongs to group m . These indicators are defined only for immigrants, so at least one $M_{hm} = 1$ for

immigrants, and all indicators equal to 0 for natives. The coefficients δ_m therefore capture the homeownership gap between immigrants of age-at-arrival group m of a given cohort of arrival in a given census year and natives. Z_h is a vector that includes age fixed effects and city of residence fixed effects.

Figure 1.2 shows the results for two age-at-arrival groups within the 1905–09 and 1915–19 arrival cohorts. In both cohorts, the pattern is consistent: immigrants who arrived at younger ages start with smaller homeownership gaps relative to similarly aged natives (controlling for city of residence) than immigrants who arrived at older ages. For example, within the 1905–09 cohort, immigrants who arrived between ages 25–34 had an initial gap of 12.2 percentage points with similarly aged natives in 1910, controlling for city of residence. In contrast, those who arrived between ages 45–54 had an initial gap of 23.9 percentage points under the same controls.

Figure 1.2: *Age-at-Arrival Effects for Selected Cohorts of Arrival*



The assimilation patterns for younger and older arrivals within each cohort, however, are similar over time. As a result, younger arriving immigrants close a larger share of their initial homeownership gap with similarly aged natives than older arriving immigrants.

1.4.4 Measuring the Degree of Homeownership Assimilation

To consistently estimate the degree of homeownership assimilation, I follow the [Borjas \(1985\)](#) regression framework. The analysis uses the 1910, 1920, and 1930 linked census data, where I pool all three survey years, and the model is estimated jointly for immigrants and natives:

$$O_{ht} = \sum_{i=1}^3 \alpha_i Y_{ht}^i + \sum_c \beta_c C_{hc} + \sum_m \lambda_m M_{hm} + \sum_j \pi_{ij} T_j + Z'_{ht} \gamma_i + \varepsilon_{ht} \quad (3a)$$

$$O_{ht} = \sum_j \pi_{nj} T_j + Z'_{ht} \gamma_i + \varepsilon_{ht} \quad (3b)$$

Here, O_{ht} is a binary variable equal to 1 if household h owns its home in census year t and 0 if it rents. Y_{ht} is the number of years since migration for the household head, divided by 10 so that coefficients can be interpreted as the effect per decade since migration. A third-order polynomial in Y_{ht} is included to allow for non-linear assimilation patterns. C_{hc} is a set of dummy variables indicating the immigrant's arrival cohort, with each C_{hc} equal to 1 if the householder belongs to cohort c . M_{hm} denotes the householder's age-at-arrival group m , also included as a set of indicator variables. T_j represents census year fixed effects (e.g., for 1910, 1920, and 1930). Z'_{ht} is a vector of age fixed effects and city of residence fixed effects, and ε_{ht} is the error term.

Equation (3a) models how homeownership evolves over the years for the immigrant sample. By setting Y_{ht} , C_h , and M_h to 0 in equation (3a), I obtain the corresponding homeownership function for natives, shown in equation (3b). The sum of α_i 's captures the total effect of spending an additional decade in the U.S. on immigrant homeownership.

The set of coefficients β_c measure cohort effects while λ_m coefficients capture differences in homeownership across age-at-arrival groups within a cohort. The reference group for age-at-arrival is immigrants who arrived between ages 55–64. Given the previous results, I should therefore expect the λ_m coefficients to be positive, as younger-arriving immigrants have smaller initial homeownership gaps than older-arriving immigrants.

Lastly, π_{ij} and π_{nj} capture the period-specific shocks or trends that affect native and

immigrant households. They reflect changes in national housing market conditions, macroeconomic trends such as unemployment and interest rates, policy changes affecting homeownership or access to credit, and inflation or deflation effects not captured by other coefficients.

Estimating equations (3a) and (3b) presents two types of age-cohort-period identification issues. Firstly, the parameters of decades since migration, cohort, and census year are not separately identified¹. To break this multicollinearity, I follow [Borjas \(1985\)](#) and assume that census year effects are the same for immigrants and natives². Secondly, age at migration can be computed using current age and decades since migration ($A_{ht} = M_h + Y_{ht}$). To break this multicollinearity, I follow [Friedberg \(1992\)](#) and assume that the age profiles of natives and immigrants are the same ($A_{ht}^i = A_{ht}^n$).

[Friedberg \(1992\)](#) emphasized the importance of age-at-arrival when studying cohort effects and assimilation. She points out that, in the labor market, there is systematic truncation of ages at immigration for cohorts who have been in the U.S. for different lengths of time which results in age-at-arrival to be negatively correlated with years since migration. Thus, not accounting for age-at-arrival makes immigrants who stayed longer in the U.S. to appear of better quality compared to immigrants who have been in the country for short amount of time.

Table [1.2](#) reports the results from estimating equation (3a) using a linear probability model on the pooled linked census sample. The sum of the coefficients on the linear, squared, and cubic terms for decades since migration shows that, on average, homeownership increases by 8.1 percentage points in their first decade in the U.S. ($0.046 + 0.047 - 0.012$).

Setting decades since migration to zero gives the cohort effects, which align with the earlier findings. Each arrival cohort begins with a sizable homeownership gap relative to similarly aged natives within the same city, as shown by the negative coefficients. The age-at-arrival coefficients are all positive and are measured relative to the omitted reference category of immigrants who arrived between ages 55–64. These positive coefficients indi-

¹Given information on the census year and the cohort of arrival, the number of decades since migration is known $Y_{ht} = \tau_1 + T_j(\tau_j - \tau_1) - C_{hc}$, where τ_1 is the base year (e.g., 1910 or 1920) and τ_j is either 1920 or 1930.

²This implies that aggregate trends in the housing market and the economy in general affect both natives and immigrants in the same manner $\pi_{ij} = \pi_{nj}$.

Table 1.2: Degree of Homeownership Assimilation

Decades Since Migration	0.046 (0.039)
Decades Since Migration Squared	0.047 (0.028)
Decades Since Migration Cubed	-0.012 (0.006)
Total Decade Effect	0.081 (0.018)
Cohort of Arrival	
1900–1904	-0.305*** (0.018)
1905–1909	-0.285*** (0.018)
1910–1914	-0.253*** (0.017)
1915–1919	-0.248*** (0.016)
Age at Arrival	
18–24 years	0.206*** (0.010)
25–34 years	0.149*** (0.010)
35–44 years	0.084*** (0.011)
45–54 years	0.037*** (0.010)
55–64 years	Omitted
Year FE	Yes
Age FE	Yes
City FE	Yes
Mean Homeownership	0.537
R-squared	0.133
Observations	22,154,342

Notes: The results are OLS estimates of equation (3a) using the pooled linked 1910, 1920, and 1930 Census data. The sample includes all native and immigrant male-headed households linked to at least one subsequent census, with household heads aged 18–74. The dependent variable in all specifications is a homeownership indicator (1 if the household owns the home and 0 otherwise). The specification controls for cohort effects and age-at-arrival effects, in addition to age, city, and census year fixed effects. Natives serve as the reference category for the cohort-of-arrival coefficients. The 55–64 age-at-arrival group is omitted and serves as the baseline category. Age fixed effects are defined using 18 categories: 11 two-year intervals from ages 18–39 (e.g., 18–19, 20–21, ..., 38–39), followed by 7 five-year intervals from ages 40–74 (e.g., 40–44, 45–49, ..., 70–74). City fixed effects are based on IPUMS city identifiers. Standard errors are clustered by year-of-migration and age-at-migration groups. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

cate that, within a cohort, immigrants who arrived at younger ages start with a smaller initial homeownership gap relative to similarly aged natives in the same city. Moreover, the magnitudes show a clear pattern: the younger the age at arrival, the smaller the initial gap compared with the reference group.

1.5 Do Assimilation Patterns vary by Birthplace?

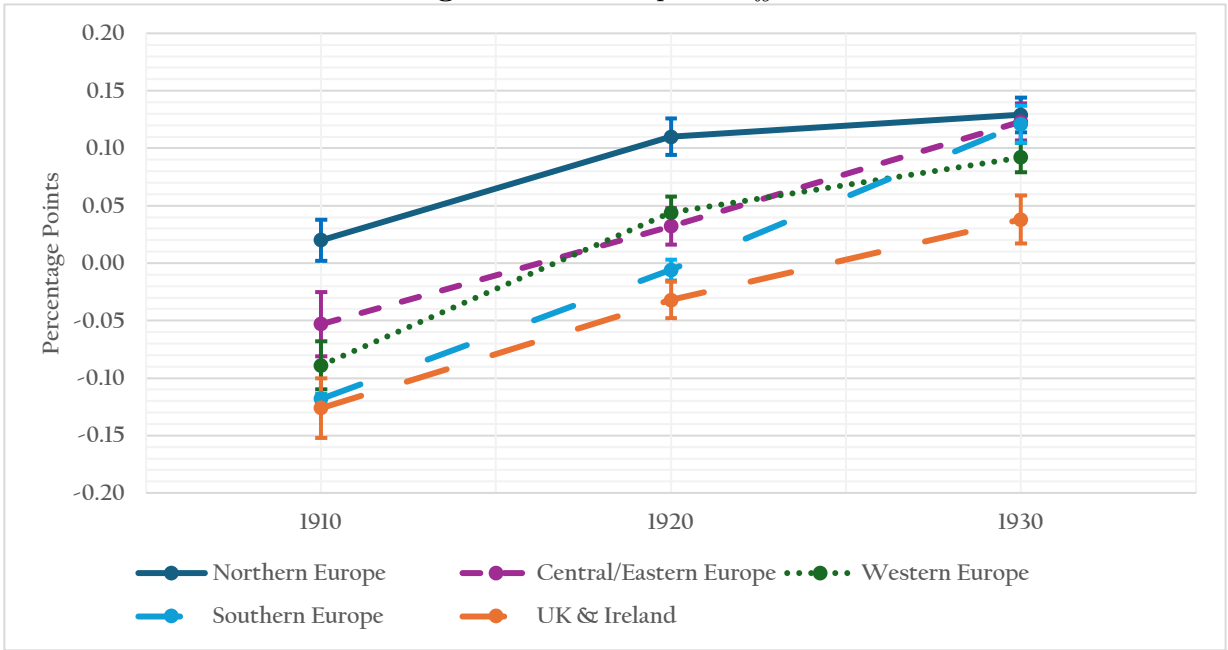
Immigrants' homeownership decisions have been found to be highly related to the housing norms and ownership culture in their countries of origin ([Gorback and Schubert, 2023](#)). This suggests that assimilation into the U.S. housing market may vary across birthplace groups. In this section, I explore whether gaps in homeownership relative to natives differ by region of origin and whether some groups experience faster convergence over time.

To examine the role of birthplace in shaping homeownership outcomes, I estimate the following regression specification using linked census data. The analysis focuses on immigrants who arrived between 1900 and 1909. Since they came just before the 1910 census, they are likely to be well-represented in the data up to the 1930 census. This allows us to study their long-term outcomes over 10 to 20 years. The regression is run separately for each year using the following model:

$$O_h = \sum_b \omega_b B_{hb} + Z'_h \gamma + \varepsilon_h \quad (4)$$

Where O_h is a binary indicator equal to 1 if household h owns its home and 0 if it rents. B_{hb} is a set of birthplace indicators equal to 1 if householder h belongs to birthplace region b . These indicators are defined only for immigrants, so for any immigrant at least one $B_{hb} = 1$, while all indicators equal 0 for natives. The coefficients ω_b therefore capture the homeownership gap between immigrants of birthplace region b and comparable natives. The vector Z_h includes age and city of residence fixed effects. The model is estimated separately for each census year (1910, 1920, and 1930). The coefficients ω_b from equation (4) are illustrated in [Figure 1.3](#), while the full set of regression results is presented in the appendix.

Figure 1.3: *Birthplace Effects*



Given that the early 20th century was a period of large-scale European immigration to the United States, I restrict the sample to European-born immigrants. These individuals are grouped into five broad regional categories: Northern Europe, the United Kingdom and Ireland, Western Europe, Central/Eastern Europe, and Southern Europe. This regional classification follows the [IPUMS](#) birthplace coding scheme. While each region contains countries with distinct cultures and homeownership norms, this broader grouping allows for a clearer analysis of overarching trends. To account for within-region heterogeneity, I also estimate the regression using finer birthplace classifications; those results are presented in the appendix. For the main analysis, however, the goal is to highlight the evolution of birthplace-based homeownership gaps with natives across regions over time.

A clear and consistent pattern of homeownership assimilation emerges across all birthplace groups. In 1910, every group except Northern Europeans had lower homeownership rates than same-aged natives after accounting for city of residence, though all groups made gains over the following two decades. Northern Europeans began with a small homeownership advantage of 2 percentage points, while immigrants from the UK and Ireland had the largest initial disadvantage at 12.6 percentage points. Although the initial gap narrows at

a similar rate across groups, Southern Europeans appear to enter homeownership somewhat faster than others. Nonetheless, over the next twenty years, all immigrant groups show complete catch-up with natives.

1.6 Do Ethnic Enclaves Facilitate Assimilation?

The literature presents differing views on the role of ethnic enclaves in economic success. Some argue that enclaves promote economic advancement by shielding immigrants from discrimination and offering a more supportive environment (Damm, 2009). Others suggest that enclaves may hinder progress by slowing the acquisition of human capital, such as language skills, critical for success in the host country’s labor markets (Chiswick and Miller, 2005; Eriksson, 2020).

A similar debate can be applied to homeownership. On the one hand, immigrants might find it easier to purchase homes within their own ethnic enclaves, where housing markets may be less discriminatory, thus promoting homeownership. On the other hand, traditional enclaves may face saturated housing markets, leading to higher home prices. In this scenario, immigrants who choose to settle in enclaves might struggle to achieve homeownership, implying that enclaves could reduce homeownership rates.

To examine the impact of immigrant enclaves on homeownership in the early 20th century, I estimate the following regression model for immigrant households using the linked census, following the approach in Borjas (2002):

$$O_{hbct} = \mu_t S_{bct} + \beta_{bt} + C_{ct} + \gamma_{at} + \eta_t Y_{hbct} + \chi_t Y_{hbct}^2 + \varepsilon_{hbct} \quad (5a)$$

In this specification, O_{hbct} denotes a homeownership indicator for immigrant household h , originating from country b , living in U.S. city c , and observed in census year t . The term β_{bt} captures birthplace fixed effects, which account for average differences in homeownership rates across immigrant groups. For example, if Irish immigrants typically have higher homeownership rates than Italian immigrants, this group-level difference is absorbed

by these fixed effects. The city fixed effects, C_{ct} , control for variation in housing markets and housing costs across cities, capturing location-specific homeownership conditions that affect all groups equally within a given city. Age fixed effects, γ_{at} , adjust for life-cycle patterns in homeownership associated with the age of the household head. Finally, Y_{hbt} and Y_{hbt}^2 represent the linear and squared terms for decades since migration for immigrant household h , allowing for a flexible, non-linear relationship between time spent in the United States and the likelihood of homeownership.

Lastly, S_{bct} is an index that measures the proportion of immigrants from birthplace b living in city c (P_{bc}) relative to the total population of city c (P_c) in census year t . Formally, it can be expressed as:

$$S_{bct} = \frac{P_{bct}}{P_{ct}} \quad (5b)$$

[Borjas \(2002\)](#) investigates the link between homeownership and ethnic concentration using data at the city level, acknowledging that more granular geographic data would be preferable for such an analysis. I extend his approach by conducting the analysis at the most detailed geographic unit available: the enumeration district. I construct enclave exposure indices at both the city and enumeration-district-levels and estimate equation 5(a) under multiple specifications.

It is useful to consider what the coefficient μ_t captures in each specification. In the first specification, the exposure index is constructed at the city–birthplace level for each census year, and the regression is estimated separately for each year while controlling for age, city and birthplace fixed effects. This setup means that the coefficient is identified from variation across city–birthplace pairs. For example, in 1910, Irish immigrants made up 12.6% of Boston’s population but only 5.2% of New York City’s population, while Italians accounted for 10.8% of Boston and 13% of New York City. Such variation in the relative size of immigrant groups across cities drives identification of μ_t . Thus, in this specification, the coefficient μ_t measures whether immigrant groups that are more concentrated within a city tend to have higher homeownership rates than less concentrated groups, holding constant

city-wide factors and group-specific characteristics.

The first three columns of Table 1.3 report results from the first specification of equation 5(a), each estimated separately for the 1910, 1920, and 1930 censuses. The coefficients are positive across all three census years, indicating a consistent positive association between ethnic concentration and homeownership. That is, immigrant groups that make up a larger share of a city’s population tend to have higher homeownership rates than less concentrated groups, after controlling for city-level fixed effects (which capture housing market conditions), birthplace fixed effects (which account for average differences across immigrant origins), and age. For example, if an Irish immigrant moved from New York City to Boston in 1910, their probability of homeownership would increase by 0.81 percentage points. This is calculated by multiplying the exposure index coefficient from specification 1 for 1910 (0.11) by the difference in the exposure index between Boston and New York ($0.126 - 0.052$).

In the second specification, the exposure index is defined at the enumeration-district-birthplace level for each census year, and the regression is estimated separately for each year while controlling for age, city and birthplace fixed effects. This setup identifies the coefficient μ_t using variation across enumeration-district–birthplace pairs. In other words, it asks: within the same city, do immigrants living in enumeration districts with higher concentrations of co-ethnics tend to have higher or lower homeownership rates than those living in less concentrated districts, regardless of their country of origin?

The coefficients from the second specification are negative across all three census years, indicating that, within a given city, immigrant households residing in enumeration districts with higher concentrations of co-ethnics tend to have lower homeownership rates than those in less concentrated enumeration districts.

In the third specification, the exposure index is again defined at the enumeration district level for each birthplace, but the regression includes age, enumeration district and birthplace fixed effects instead of city fixed effects. This means the coefficient μ_t is identified using variation in ethnic concentration across immigrant groups within the same enumeration district.

Table 1.3: *Impact of Ethnic Enclaves on Immigrant Homeownership in Linked Census by Census Year*

	City-Level			ED-level, with city FE			ED-level, with ED FE		
	1910	1920	1930	1910	1920	1930	1910	1920	1930
Enclave Exposure Index	0.110* (0.047)	0.022 (0.047)	0.060 (0.078)	-0.038*** (0.010)	-0.163*** (0.016)	-0.260*** (0.031)	0.006 (0.006)	-0.031*** (0.005)	0.011 (0.008)
Decades Since Migration	0.102*** (0.019)	0.168*** (0.013)	0.125*** (0.019)	0.104*** (0.019)	0.172*** (0.013)	0.134*** (0.018)	0.092*** (0.014)	0.126*** (0.007)	0.089*** (0.013)
Decades Since Migration ²	0.034* (0.013)	-0.019*** (0.004)	-0.013** (0.004)	0.032* (0.013)	-0.022*** (0.004)	-0.015*** (0.004)	0.031** (0.011)	-0.010*** (0.003)	-0.007* (0.003)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birthplace FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes			
Enumeration District FE							Yes	Yes	Yes
Mean Homeownership	0.142	0.306	0.571	0.142	0.306	0.571	0.140	0.306	0.571
R-squared	0.170	0.185	0.167	0.171	0.189	0.172	0.345	0.346	0.342
Observations	205,138	621,993	556,422	205,138	621,993	556,422	202,292	619,627	553,273

Notes: The results are OLS estimates of equation (5a), estimated separately for each census year using the linked 1910, 1920, and 1930 Census data. The sample includes only immigrant male-headed households linked to at least one subsequent census, with household heads aged 18–74. The dependent variable in all specifications is a homeownership indicator (1 if the household owns the home and 0 otherwise). The enclave exposure index in the first three columns is defined as the proportion of immigrants from birthplace (b) living in city (c) relative to the total population of that city in census year (t). In the remaining six columns, the enclave exposure index is defined at the enumeration-district level. Age fixed effects are defined using 18 categories: 11 two-year intervals from ages 18–39 (e.g., 18–19, 20–21, ..., 38–39), followed by 7 five-year intervals from ages 40–74 (e.g., 40–44, 45–49, ..., 70–74). City, birthplace, and enumeration district fixed effects are based on the city, bpl, and enumdist variables from the IPUMS data. Standard errors are clustered by city of residence and birthplace in the first six columns, and by enumeration-district and birthplace in the last three columns. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In this case, the coefficient μ_t answers: within a given enumeration district, do immigrant groups that are more concentrated (i.e., make up a larger share of that enumeration district's population) have higher homeownership rates than those that are less represented?

The coefficients from the third specification show mixed signs across the three census years. In 1910 and 1930, the coefficients are positive, indicating that immigrant groups with a higher representation within a given enumeration district tend to have higher homeownership rates than less represented groups in the same enumeration district. However, in 1920, the coefficient turns negative, indicating a reversal in this pattern for that census year.

To examine how ethnic enclaves affect homeownership across cities and neighborhoods within cities over time, I estimate the following pooled regression using the linked:

$$O_{hbct} = \mu_t S_{bct} + \beta_{bt} + C_{ct} + \gamma_{at} + \eta_t Y_{hbct} + \chi_t Y_{hbct}^2 + \lambda_t + \lambda_{rt} + \varepsilon_{hbct} \quad (5c)$$

In equation (5c), dummy variables for each census year (λ_t) are included, along with interaction terms between region of residence fixed effects and census years (λ_{rt}). These interactions capture changes in housing market conditions within regions of residence over time. I estimate equation (5c) under four different specifications using the linked data.

Again, consider what the coefficient μ_t captures in each specification. In the first specification, the exposure index is constructed at the city–birthplace level for each census year, and each regression includes fixed effects for age, birthplace and city $\times$ year. The inclusion of city $\times$ year fixed effects is crucial as it absorbs all time-varying city-level shocks such as changes in housing supply, economic conditions, or policy shifts that affect all groups equally within a city. This means that the coefficient μ_t is identified from variation in the ethnic concentration of immigrant groups within a city across census years, relative to other ethnic groups in the same city. In other words, it captures whether immigrant groups that become more (or less) concentrated within a city over time tend to experience corresponding changes in homeownership rates, holding constant age, birthplace and changes in citywide housing conditions.

The positive coefficient on the Enclave Exposure Index in Table 1.4, column (i), im-

plies that as immigrants from a given birthplace become more concentrated in a city over time, their likelihood of owning a home rises, holding constant age, birthplace and city-level housing conditions.

Table 1.4: *Impact of Ethnic Enclaves on Immigrant Homeownership in Linked Census*

	City-Level	Enumeration-District Level		
	(i)	(ii)	(iii)	(iv)
Enclave Exposure Index	0.103* (0.047)	-0.160*** (0.020)	-0.011** (0.004)	-0.102*** (0.004)
Decades Since Migration	0.185*** (0.014)	0.185*** (0.014)	0.160*** (0.003)	0.162*** (0.003)
Decades Since Migration ²	-0.026*** (0.004)	-0.026*** (0.004)	-0.022*** (0.001)	-0.022*** (0.001)
Age FE	Yes	Yes	Yes	Yes
Birthplace FE	Yes	Yes	Yes	
Enumeration District FE				Yes
City $\times$ Year FE	Yes	Yes		
Enumeration-District $\times$ Year FE			Yes	
City $\times$ Birthplace $\times$ Year FE				Yes
Mean Homeownership	0.388	0.388	0.388	0.387
R-squared	0.262	0.265	0.413	0.393
Observations	1,383,553	1,383,553	1,375,192	1,368,843

Notes: The results are OLS estimates of equation (5c) using the pooled linked 1910, 1920, and 1930 Census data. The sample includes only immigrant male-headed households linked to at least one subsequent census, with household heads aged 18–74. The dependent variable in all specifications is a homeownership indicator (1 if the household owns the home and 0 otherwise). The enclave exposure index in column (i) is defined as the proportion of immigrants from birthplace (b) living in city (c) relative to the total population of that city in census year (t). In columns (ii)–(iv), the enclave exposure index is defined at the enumeration district level. Age fixed effects are defined using 18 categories: 11 two-year intervals from ages 18–39 (e.g., 18–19, 20–21, ..., 38–39), followed by 7 five-year intervals from ages 40–74 (e.g., 40–44, 45–49, ..., 70–74). City, birthplace, and enumeration-district fixed effects are based on the city, bpl, and enumdist variables from the IPUMS data. Standard errors are clustered by city and birthplace in column (i) and by enumeration district and birthplace in columns (ii)–(iv). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In the second specification, the exposure index is defined at the enumeration district level for each birthplace in each census year, and the regression is again estimated using pooled data with fixed effects for age, birthplace and city $\times$ year. As before, the city $\times$ year fixed effects net out changes in housing market conditions that vary across cities and over time. The coefficient mu_t is thus identified from changes in the ethnic composition of neighborhoods (enumeration districts) within cities across census years. It answers the question: within a given city, do immigrants from a given birthplace living in enumeration districts that become more (or less) concentrated with co-ethnics over time, relative to other districts in the same city that did not experience such changes, tend to experience higher or

lower homeownership rates, after accounting for origin-specific characteristics, city factors, and shifts in local housing markets across time?

The negative coefficient on the exposure index in Table 1.4, column (ii), shows that immigrants living in districts that become more concentrated with co-ethnics over time, compared with other districts in the same city, tend to have lower homeownership rates.

In the third specification, the exposure index is again defined for each enumeration district by birthplace in each census year. The regression includes fixed effects for age, birthplace and enumeration-district $\times$ year. The coefficient mu_t is identified from within-enumeration-district changes in concentration of immigrant groups from specific birthplaces across census years. It answers the question: within the same enumeration district, do immigrant groups whose representation increases or decreases over time tend to experience higher or lower homeownership rates compared to other groups in the same district, after accounting for birthplace, district, and time-specific effects? This comprehensive set of controls accounts for time-invariant characteristics of enumeration districts, average differences across immigrant groups, overall time trends, and importantly, any time-varying shocks or changes specific to each city–birthplace combination.

The coefficient for this specification is also negative as reported in Table 1.4, column (iii), indicating that immigrants residing in neighborhoods that become more concentrated with co-ethnics over time tend to have lower homeownership rates.

In the fourth specification, the exposure index continues to be defined at the enumeration-district–birthplace level for each census year. The regression includes fixed effects for age, enumeration district and a city $\times$ birthplace $\times$ year interaction. This rich set of controls absorbs overall period effects, time-invariant characteristics of both neighborhoods and immigrant groups, and group-specific trends within each city over time. With these controls in place, the coefficient on the exposure index is identified from changes in the relative concentration of immigrant groups within the same enumeration district across census years, after subtracting out each group’s broader trend in that city. Put differently, this specification answers the question: within a given enumeration district, do immigrant groups whose local representation rises or falls relative to their expected group-specific trend in that city expe-

rience corresponding changes in homeownership, once neighborhood, group, and city trends have been accounted for?

The coefficient for this specification is also negative (Column (iv), Table 1.4), indicating that immigrant groups whose local representation increases within a neighborhood tend to experience lower homeownership rates relative to other groups in the same city, after accounting for neighborhood, group, and city-level group-specific trends.

A potential concern with interpreting the estimated effects across all specifications of this section is the endogeneity of immigrants' residential location choices. Endogeneity arises because immigrants do not choose cities or neighborhoods at random. They may select locations for reasons that also affect homeownership, such as job access, housing affordability, or local credit networks (Borjas, 2002). This concern is strongest in specification 1, where identification comes from variation across cities by birthplace. City $\times$ year fixed effects remove city-level shocks and trends but cannot remove sorting of groups into particular cities. In specification 2 the exposure index is at the enumeration district-by-birthplace level so identification uses variation across districts within a city. City $\times$ year fixed effects still absorb city wide shocks, but sorting across neighborhoods within a city can still bias the estimates.

Specifications 3 and 4 use richer controls and so reduce the endogeneity concern. Specification 3 adds enumeration district $\times$ year fixed effects, which remove all time varying district level shocks, leaving only group specific sorting within the same district as a remaining concern. Specification 4 adds city $\times$ birthplace $\times$ year interactions, which remove each group's city-level trend, so identification comes from changes in a group's local share relative to its city trend. As fixed effects become more detailed, they rule out more sources of bias, though residual selection within district by group can still remain and should be acknowledged.

One possible explanation for the negative relationship between neighborhood ethnic concentration and homeownership is reverse causation. Many immigrants may initially settle in ethnic enclaves because they provide social networks, information, and access to rental housing. As immigrants become more established, they may purchase homes outside ethnic enclaves while remaining in the same city. If leaving the enclave is part of the homeowner-

ship assimilation process, then neighborhoods with a higher concentration of co-ethnics will naturally have lower homeownership rates. This could generate the negative relationship observed in the neighborhood-level specifications. Ideally, this explanation could be tested by following immigrants as they move across neighborhoods over time. However, this is not possible because the boundaries of the enumeration district can change between censuses, making it difficult to build consistent neighborhood measures over time.

1.7 Empirical Evidence on Homeownership Dynamics by Nativity

In this section, I take advantage of the panel aspect of the linked data to study immigrant assimilation in homeownership more closely. It allows me to track the same households over time making it possible to go beyond average trends and examine the transitions between ownership states directly. While earlier sections established broad patterns of assimilation, the focus here is on decomposing those patterns into two key components: homeownership entry and homeownership exit. By analyzing how these two processes interact, I aim to provide a clearer understanding of the dynamics that shape the observed assimilation patterns over time.

To do so, I estimate separate models for entry into and exit from homeownership. The entry regressions are restricted to households that are renters in their first observed census year, while the exit regressions include only households that are homeowners in their first observed census year. I then estimate the probability of entering or exiting homeownership across observed inter-census transitions, including 1910–1920 and 1920–1930, using the following regression specification.

$$\text{Outcome}_h = \alpha + \beta \text{Immigrant}_h + \lambda_{rt} + A_{ht} + \varepsilon_h \quad (6)$$

Where Outcome_h is a binary indicator equal to one if household h experiences a transition in homeownership status between two consecutive census years. Specifically, in the entry

specification, $Outcome_h = 1$ if household h transitions from renting to owning between successive census observations (e.g., 1910–1920 or 1920–1930), and in the exit specification, $Outcome_h = 1$ if household h transitions from owning to renting over the same intercensal interval.

Table 1.5: *Entry into and Exit from Homeownership (Immigrants vs. Natives)*

	Entry Specifications			Exit Specifications		
	(i)	(ii)	(iii)	(i)	(ii)	(iii)
Immigrants	0.026*** (0.001)			-0.015*** (0.001)		
Enclave Exposure Index – City		-0.272** (0.084)	-0.123 (0.070)		0.093** (0.033)	0.082** (0.028)
Enclave Exposure Index – ED			-0.080*** (0.015)			0.008 (0.011)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Birthplace FE		Yes	Yes		Yes	Yes
State $\times$ Year FE	Yes			Yes		
City $\times$ Year FE		Yes	Yes		Yes	Yes
Mean of Outcome	0.392	0.394	0.394	0.156	0.145	0.145
Mean Enclave Exposure (City)		0.013	0.013		0.007	0.007
Mean Enclave Exposure (ED)		0.045	0.045		0.020	0.020
R-squared	0.031	0.054	0.054	0.034	0.032	0.032
Observations	6,633,559	2,831,764	2,831,764	6,199,716	1,577,085	1,577,085

Notes: The results are OLS estimates of equation (6). The sample consists of native- and immigrant-headed households linked either between 1910–1920 or between 1920–1930, whose heads are at least 18 years old when first observed and no older than 74 years old when last observed. For the entry specification, the sample is restricted to households that were renters in the first census in which they are observed. The dependent variable is an indicator equal to one if a household transitions from renting in its initial observed census to homeownership in the subsequent census, and zero if it remains a renter. For the exit specification, the sample is restricted to households that were homeowners in the first census in which they are observed. The dependent variable is an indicator equal to one if a household transitions from owning in its initial observed census to renting in the subsequent census, and zero if it remains a homeowner. Immigrant is an indicator variable equal to one if the household head is foreign-born and zero if native-born. Standard errors are clustered at the appropriate geographic level in each specification. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

$Immigrant_h$ is an indicator variable equal to 1 if the head of household h is foreign-born and 0 if native-born. The term λ_{rt} represents state-by-census-year fixed effects that account for conditions varying across states and over time that may affect housing markets (e.g., local economic shocks or changes in housing supply). A_{ht} denotes age fixed effects, which control for life-cycle differences in housing behavior. Finally, ε_h is the error term.

For both the entry and exit specifications, the coefficient β on the immigrant indicator captures the difference in transition probabilities between immigrants and natives, holding constant age and location-by-time factors.

- In the exit regressions, a negative coefficient on $Immigrant_h$ indicates that immigrants are less likely than natives to leave homeownership over time.
- In the entry regressions, a positive coefficient on $Immigrant_h$ suggests that immigrants are more likely than natives to become homeowners over time.

Column (i) of Table 1.5 reports the estimates from equation (6) for the entry and exit regressions. In the entry regression, the coefficient on the immigrant indicator is positive (0.026), implying that immigrants are, on average, 2.6 percentage points more likely to transition from renting to owning than similarly aged natives, conditional on state-by-census-year fixed effects. Relative to the mean entry probability of 39.2 percent, this corresponds to immigrants being about 6.6% more likely to enter homeownership. In the exit regression, the coefficient on the immigrant indicator is negative (-0.015), indicating that immigrants are, on average, 1.5 percentage points less likely to transition from owning to renting than similarly aged natives, conditional on the same fixed effects. Given a mean exit probability of 15.6%, this corresponds to immigrants being about 9.6% less likely to exit homeownership.

While the differences in transition probabilities between natives and immigrants are modest, these probabilities, together with the initial distribution of renters and owners, determine the change in homeownership over a decade. Even if immigrants and natives had identical entry and exit probabilities, differences in initial ownership rates would mechanically imply convergence over time. Since immigrants are more likely to start the decade as renters, the same entry probability generates a larger increase in their homeownership rate. Thus, observed assimilation in homeownership can emerge even in the absence of differential transition behavior. This intuition is summarized formally by the following expression for the change in group-specific homeownership rates over a decade:

$$\Delta H_g = (1 - H_g) \rho_g^E - H_g \rho_g^X \quad (7)$$

Here ΔH_g denotes the change in the homeownership rate of group g , and H_g is the initial ownership rate of group g at the start of the decade. ρ_g^E is the probability of entering

homeownership, while ρ_g^X is the probability of exiting homeownership for group g . Using equation (7), the difference in the change in homeownership between immigrants and natives can be written as:

$$\Delta H_I - \Delta H_N = (1 - H_I)\rho_I^E - H_I\rho_I^X - (1 - H_N)\rho_N^E + H_N\rho_N^X \quad (8)$$

By adding and subtracting the terms $(1 - H_I)\rho_N^E$ and $H_I\rho_N^X$ and rearranging, this expression becomes:

$$\Delta H_I - \Delta H_N = (1 - H_I)(\rho_I^E - \rho_N^E) - H_I(\rho_I^X - \rho_N^X) + (H_N - H_I)\rho_N^E + (H_N - H_I)\rho_N^X$$

In this decomposition, the first two terms capture the contribution of differences in transition probabilities to the immigrant–native change in homeownership, while the last two terms capture the contribution from differences in initial homeownership rates. To implement this decomposition, I need information on the initial ownership rates (H_N and H_I) as well as the probabilities of entering and exiting homeownership for natives and immigrants (ρ_N^E , ρ_N^X , ρ_I^E , and ρ_I^X). Table 1.6 presents these values for the sample.

The first three columns of Table 1.6 report raw homeownership rates for natives and immigrants in the linked census for 1910, 1920, and 1930. The last two columns present estimated probabilities of transitioning into and out of homeownership for natives and immigrants, derived from the entry and exit regressions estimated on pooled 1910–1920 and 1920–1930 transitions and conditional on age and state-by-census-year fixed effects. The difference between immigrant and native probabilities corresponds exactly to the coefficient on the immigrant indicator in the regressions. The fourth column reports homeownership rates weighted by the number of observations at the start of each transition period³.

³Because entry and exit probabilities are estimated using inter-census transitions starting in 1910 and 1920, ownership rates are measured at the beginning of each transition period and averaged using weights equal to the number of households contributing to each transition. Formally, for group g , the weighted average is computed as:

$$H_g = \frac{N_{g,1910}H_{g,1910} + N_{g,1920}H_{g,1920}}{N_{g,1910} + N_{g,1920}}$$

where $N_{g,t}$ denotes the number of households in group g observed at the start of the inter-census transition

Table 1.6: Homeownership Levels and Transition Probabilities by Nativity

	Homeownership by Nativity				Homeownership Transitions	
	1910	1920	1930	Weighted Avg.	Renters → Owners	Owners → Renters
Natives	0.484	0.531	0.625	0.512	0.389	0.156
Immigrants	0.218	0.372	0.612	0.334	0.415	0.141
Difference	-0.266	-0.159	-0.013	-0.178	0.026	-0.015

Notes: In the linked census, native households number 5,664,298 in 1910, 8,447,500 in 1920, and 6,112,479 in 1930; the corresponding counts for immigrant households are 293,230, 873,567, and 763,268.

Using the values reported in Table 1.6, equation (8) implies an immigrant–native difference in decadal homeownership growth of approximately 11.9 percentage points. About 18.7% of this gap is explained by differences in entry and exit patterns, with higher entry rates accounting for roughly 14.5% and lower exit rates accounting for an additional 4.2%. The remaining 81% reflects differences in initial homeownership levels, with immigrants beginning each decade substantially more likely to be renters. As a result, even modest differences in transition probabilities generate sizable convergence in ownership rates. This accounting exercise corroborates the assimilation pattern documented in Section 1.4: immigrant homeownership rises rapidly over time not only because immigrants are more likely to enter and less likely to exit homeownership, but also because their initially low ownership rates mechanically amplify the impact of these transition probabilities.

1.7.1 Do ethnic enclaves influence homeownership entry and exit decisions?

In Section 1.6, I examined the role of ethnic enclaves in immigrant homeownership assimilation and found that city-level enclaves are positively associated with homeownership, whereas neighborhood-level enclaves appear to have a negative effect. To better understand these patterns and address remaining questions, I now investigate how enclave size at both the city and neighborhood levels relates to homeownership entry and exit decisions. To do this, I estimate the following regression equation:

in year t .

$$Outcome_h = \alpha + \beta_c S_{bct} + \lambda_{rt} + B_{ht} + A_{ht} + \varepsilon_h \quad (9)$$

As before, $Outcome_h$ is a binary indicator equal to one if household h experiences a transition in homeownership status between two consecutive census years. Specifically, in the entry specification, $Outcome_h = 1$ if household h transitions from renting to owning between successive census observations, such as 1910 to 1920 or 1920 to 1930. In the exit specification, $Outcome_h = 1$ if household h transitions from owning to renting over the same inter-census interval.

S_{bct} denotes the enclave exposure index at the city level. λ_{rt} are city-by-census-year fixed effects that absorb time-varying local conditions affecting housing markets, such as local economic shocks or housing supply. City-by-year fixed effects are used because the exposure index is defined only for households residing in identifiable cities and their associated enumeration districts. B_{ht} are birthplace fixed effects, and A_{ht} are age fixed effects that control for life-cycle differences in housing behavior. ε_h is the error term.

I estimate two specifications for both the entry and exit regressions. The first specification includes only enclave exposure measured at the city level, while the second specification additionally incorporates neighborhood-level enclave exposure. The results from these specifications are reported in columns (ii) and (iii) of the entry and exit panels of Table 1.5.

Because both specifications include birthplace fixed effects, age fixed effects, and city-by-census-year fixed effects, the coefficient on the city-level exposure index is identified from variation in exposure across birthplaces within the same city and census year. In the specification that also includes neighborhood-level exposure, the coefficient on the enumeration-district exposure index is identified from differences in enclave size across birthplaces within the same city and census year.

In the first specification, the coefficient on city-level enclave exposure in the entry regression is negative at -0.272 , indicating that a larger city-level enclave is associated with a lower probability of transitioning from renting to owning. To give a sense of magnitude, the mean city-level enclave exposure is 0.013, so at this average level, the coefficient implies

that the probability of entering homeownership is reduced by roughly 0.35 percentage points (-0.272×0.013), holding birthplace, age, and city-by-census-year fixed effects constant. This corresponds to approximately a 0.9% reduction relative to the mean entry probability of 39.4 percent.

The coefficient on city-level enclave exposure in the exit regression is positive (0.093), indicating that a larger city-level enclave is associated with a higher probability of transitioning from owning to renting. The mean city-level enclave exposure is 0.007, so at this average level, the coefficient implies that the probability of exiting homeownership increases by roughly 0.065 percentage points (0.093×0.007), holding birthplace, age, and city-by-census-year fixed effects constant. This corresponds to an increase of approximately 0.45% relative to the mean exit probability of 14.5 percent.

In the second specification, which includes both city- and neighborhood-level enclave exposure, the results indicate that enclave size has only modest effects on homeownership transitions. For entry into homeownership, the city-level coefficient is -0.123 and the neighborhood-level coefficient is -0.080 . At the mean exposures, these correspond to reductions in the probability of transitioning from renting to owning of roughly 0.16 and 0.36 percentage points, or 0.4% and 0.9% relative to the mean entry probability of 0.394. For exit from homeownership, the city-level coefficient is 0.082 and the neighborhood-level coefficient is 0.008, implying very small increases in the probability of exiting, approximately 0.006 and 0.016 percentage points, or 0.04% and 0.1% relative to the mean exit probability of 0.145.

Taken together, these results indicate that city- and neighborhood-level immigrant enclave size have only a small effect on homeownership transitions. In the entry regression, larger enclaves slightly reduce the probability of transitioning from renting to owning, while in the exit regression, they slightly increase the probability of leaving homeownership. These effects are modest in magnitude, representing less than one percent of the mean transition probabilities. Overall, these findings suggest that city- and neighborhood-level enclave size plays only a minor role in shaping entry into and exit from homeownership.

1.8 Conclusion

This study examines how European immigrants who arrived in the United States between 1900 and 1930 assimilated into American society through homeownership. Using the MLP that probabilistically links households across these censuses, I show that immigrants started with substantially lower homeownership rates than natives but steadily increased ownership over time, with many cohorts narrowing most of the gap within two decades. Assimilation patterns vary by cohort, age at arrival, and birthplace. The analysis also examines ethnic enclaves, finding that higher concentrations of co-ethnics at the city level are associated with higher homeownership rates, whereas greater neighborhood-level concentration is linked to lower ownership even within the same city and birthplace.

A key contribution of this essay is the decomposition of assimilation into entry into and exit from homeownership. Using household-level panel data, I show that immigrants were 2.6 percentage points more likely than similarly aged natives to enter homeownership and 1.5 percentage points less likely to exit, conditional on age and local conditions. These transition probabilities, combined with initially lower ownership rates, account for most of the observed convergence over time. By highlighting these dynamic patterns, the study provides a more nuanced understanding of assimilation, showing how immigrant homeownership reflects both behavioral responses and structural conditions rather than static outcomes.

At the same time, it is important to acknowledge that the current analysis does not yet correct for biases introduced by the linkage process itself. The likelihood of being successfully linked across census years is not random, households with more stable characteristics, such as native-born status or property ownership, may be overrepresented in the linked data. Immigrants and renters, on the other hand, are more likely to be underrepresented. This imbalance can distort the conclusions I draw about the pace and extent of assimilation. Future drafts will incorporate inverse probability weights (IPWs) to reweight the sample based on linkage probabilities, thereby producing estimates that are more representative of the full population.

While this essay documents key patterns of immigrant assimilation in homeownership,

it does not yet examine the underlying mechanisms driving these outcomes. Prior research has offered limited insight into the roles played by local labor markets, co-ethnic networks, residential segregation, or policy environments due to data constraints. The linked panel data used here opens the door to addressing these gaps more directly. Future versions of this study will explore how individual, household, and contextual factors shaped the trajectory of immigrant assimilation in early 20th century America.

Chapter 2

Immigration Shocks and Homeownership: Evidence from Linked Census Data

2.1 Introduction

The early twentieth century was a period of high immigration to the United States. During the age of mass migration (1850 – 1913), more than 30 million European immigrants arrived, and by 1910 the foreign born accounted for about 22% of the U.S. labor force ([Abramitzky et al., 2014](#)). This period of mass immigration was followed by a collapse in immigrant inflows after World War I (WWI), which was further reinforced by the Immigration Acts of 1921 and 1924. These national shocks to immigration reduced inflows from different sending regions to different degrees and generated substantial variation in local immigrant populations across U.S. cities ([Tabellini, 2020](#)). We exploit this variation to study how immigration shocks affected homeownership outcomes for natives during this period.

Studying homeownership is important because it is an important channel for wealth accumulation and long-term financial security in the United States. Both natives and immigrants spend about one-third of their income on housing, with lower-income households spending

an even larger proportion (Saiz, 2010). Housing wealth constitutes a major portion of total household wealth, representing roughly 60% for American homeowners (Tracy et al., 1999). By owning a home, households build equity over time, which can be leveraged to finance education, start a business, or secure a stable retirement (Di et al., 2007; McCabe, 2018). In this way, homeownership functions not only as a durable investment but also as a critical vehicle for upward mobility and long-term economic security, providing families with a financial cushion during periods of economic hardship.

Beyond its wealth-building role, homeownership contributes to stability, autonomy, and well-being. Homeowners are protected from the uncertainties of the rental market, including annual rent fluctuations, and enjoy greater freedom to shape their living environment (Sinai and Souleles, 2005; Zumbro, 2014). Ownership also conveys social status and personal accomplishment, enhancing life satisfaction and self-esteem (Diaz-Serrano, 2006; Dietz and Haurin, 2003; Rohe et al., 2002).

However, most of the literature on the economic effects of immigration during the early twentieth century has ignored its implications for natives' housing market outcomes. Immigrants directly affect local housing markets by settling in and consuming housing in their destination cities (Saiz, 2010). This mechanism has motivated a large literature on the effects of immigration on natives' housing outcomes in more recent periods (Bian et al., 2025; Saiz, 2003, 2007, 2010; Saiz and Wachter, 2011). However, despite the large immigrant presence in the United States during the early twentieth century, with the foreign born accounting for 14.8 percent, 13.3 percent, and 11.7% of the U.S. population in 1910, 1920, and 1930 respectively, relatively little attention has been paid to the effects of immigration on natives' housing market outcomes during this period.

Using United States census data from the early twentieth century, we examine several ways in which immigration affected the housing outcomes of natives. First, we study how changes in the fraction of a city's population made up of recent immigrants influence overall native homeownership rates. Second, we analyze whether changes in immigration between census decades affect natives' transitions into and out of homeownership. We then investigate the possible mechanisms through which immigration may influence these housing decisions,

focusing on the role of housing prices and rents. Finally, we examine whether immigration affects household formation among natives by influencing the likelihood that individuals who were not household heads in one census become household heads in the following census, and whether these newly formed households are more likely to become homeowners or remain renters.

Our estimation sample is drawn from the full-count U.S. censuses of 1900–1930 and the Multigenerational Longitudinal Panel (MLP) from IPUMS, which links individuals across censuses over multiple decades. While the full-count census samples allow us to study changes in homeownership over time through a repeated cross-sectional (RCS) analysis, they do not allow us to study entry into and exit from homeownership, as they do not follow the same households over time. The linked structure of the MLP, however, allows us to directly observe changes in outcomes, including transitions into and out of homeownership as well as transitions into household headship.

A key challenge in studying immigration is the potential endogeneity of immigrant location choices. Immigrants do not choose where to live randomly, and their destination choices may be influenced by housing market conditions or other city characteristics that also affect native homeownership outcomes, such as local taxes, housing policies, or economic opportunities. If these factors are not fully accounted for, the estimated effects of immigration may be biased. To address this concern, we follow the literature ([Price et al., 2020](#); [Tabellini, 2020](#)) and construct a leave-out shift-share instrument that predicts immigrant inflows using historical settlement patterns of earlier migrants from the same birthplace. Our measure of the instrument comes from [Price et al. \(2020\)](#) (henceforth referred to as vLWP) who also use linked data, ensuring that our implementation is directly comparable to theirs.

Our results show that immigration had a large and economically meaningful impact on native homeownership outcomes in early twentieth century American cities. We find that a 5-percentage point increase in the fraction of a city’s population made up of recent immigrants increased native homeownership rates by 2.28 percentage points, corresponding to roughly a 6.0% increase relative to the average native homeownership rate. Examining housing transitions further shows that immigration both increased home buying and reduced

home selling among natives. A 5-percentage point increase in the change in the recent immigrant share of a city’s population raised the probability that natives transitioned into homeownership by approximately 9.82 percentage points, while reducing transitions out of homeownership by about 4.59 percentage points. Relative to the average transition rates, these effects correspond to a 26.1% increase in home buying and a 29.4% decline in home selling among natives. To understand the mechanism behind these results, we examine the response of housing prices and rents using city-level housing price and rent data from the Federal Reserve Bank of Philadelphia. Our IV estimates indicate that immigration reduced home prices at the city level, suggesting that immigration may have encouraged native homeownership by lowering the cost of purchasing homes.

We also find that immigration significantly influenced household formation among natives. A 5-percentage point increase in the fraction of a city’s population made up of recent immigrants increased the probability that an individual formed a new household by approximately 3.83 percentage points, corresponding to an 8.5% increase relative to the average household formation rate. We further show that immigration increased the likelihood that these newly formed households became homeowners rather than remaining renters. Specifically, a 5-percentage point increase in the immigrant share of a city’s population raised the probability that new household heads transitioned into homeownership by approximately 8.67 percentage points. Relative to the average transition rate into homeownership among new household heads, this corresponds to an increase of about 32.1%, indicating a substantial effect of immigration on the housing outcomes of newly formed native households.

The validity of our shift-share instrument relies on two conditions: relevance and exogeneity. Relevance requires that the instrument strongly predicts immigration, which is supported by the strong first stage relationship in our analysis. Exogeneity requires that the instrument affects native housing outcomes only through immigration rather than through persistent city characteristics that independently shape housing market trends. Two concerns may threaten this assumption. First, cities that historically attracted immigrants may have already been on different housing market trajectories before the immigration shocks we study. Second, immigrants from particular birthplaces may continue moving to certain

cities because of persistent economic, social, or cultural characteristics of those cities rather than historical settlement patterns alone. Following vLWP, we address the first concern by controlling for preexisting trends in housing transitions and the second by exploiting variation in immigration generated by WWI and the immigration quota laws of the 1920s, which were driven by international events and federal policy rather than local city conditions.

Controlling for preexisting housing market trends reduces the estimated effect of immigration on transitions into homeownership, and the pre-trend controls themselves are statistically significant. This suggests that part of the baseline relationship was capturing persistent city-level factors that influenced both immigration and native home buying behavior. This raises concerns about the causal interpretation of the estimated effects on transitions into homeownership.

In contrast, the estimated effect of immigration on transitions out of homeownership changes only modestly after including pre-trend controls, and the revised estimate is not statistically different from the baseline specification. Moreover, the pre-trend controls in the home selling regressions are smaller and less precisely estimated than those in the home buying regressions. Taken together, these findings suggest that the estimated effect of immigration on home selling is less influenced by preexisting city-level trends and is therefore more consistent with a causal interpretation.

We also re-estimate our main specifications using an alternative instrument based only on immigration variation generated by WWI and the immigration quota laws of the 1920s. The estimated effects remain nearly identical to our baseline results. This suggests that our findings are not driven by persistent city characteristics that continued to attract immigrants from particular birthplaces over time. Instead, the results remain robust when identification relies only on immigration changes generated by large external geopolitical and policy shocks unrelated to underlying city-level housing market conditions.

We document that immigration increases native homeownership, which suggests that immigration may also shape home formation decisions. We define home formation as the probability that young natives aged 15 to 25 who are initially observed as non-household heads in their first observed census transition into household headship over the subsequent

decade. We estimate this relationship using a 2SLS framework. We also examine whether immigration affects subsequent homeownership transitions among these newly formed households using the same empirical approach.

We find that immigration increases native homeownership. This motivates the possibility that immigration may also influence home formation, defined as the probability that young natives aged 15 to 25 who are initially observed as non-household heads in their first observed census transition into household headship over the subsequent decade. We examine this relationship using a 2SLS framework. We further assess whether immigration affects the likelihood that these newly formed households transition into homeownership, applying the same empirical strategy.

We find a consistently positive and statistically significant relationship between immigration and household formation, as well as between immigration and subsequent homeownership transitions among newly formed households. However, the pre-trend analysis suggests that the estimated effects on household formation partly reflect persistent city characteristics that jointly influence both household formation and immigration, warranting caution in a causal interpretation. In contrast, the pre-trend results for homeownership transitions among newly formed households are more consistent with a causal interpretation.

The essay is organized as follows: Section 2.2 reviews the existing literature that is closely related to the topic, Section 2.3 describes the data and sampling restrictions, Section 2.4 outlines the methodology, Section 2.5 presents and discusses the results, and Section 2.6 concludes.

2.2 Literature Review

The literature on the impact of immigration on native housing outcomes is limited, particularly for the period we study. To our knowledge, no existing study examines how immigration shocks affected the homeownership of natives in the United States during the early twentieth century.

We can divide the available literature on immigration and housing into three strands. The

first strand, which we discuss in the first subsection, includes a small number of studies that compare homeownership rates between immigrants and natives during the late nineteenth and early twentieth centuries, but do not analyze the effects of immigration shocks. In the second subsection, we turn to studies from more recent periods, where increased research attention has allowed for more detailed analysis of the relationship between immigration and homeownership. Apart from homeownership, there is very little literature on how immigration shocks affected housing outcomes in the United States during the early twentieth century which we examine in the third subsection. While the final subsection examines a broader body of work on the effects of immigration on housing values, prices, rents, and native mobility in more recent periods.

2.2.1 Immigration and Homeownership in the Early 20th Century

One common theme in these studies of immigrant homeownership is the use of housing as a marker of economic assimilation and life-cycle success. The first study is by [Sutch \(2012\)](#) which examines urban non-farm homeownership among native- and foreign-born families in the United States between 1890 and 1930. It focuses on how homeownership varied by age and across immigrant cohorts, with the hypothesis that owning a home was a common life-cycle asset and that this motive was particularly strong for immigrants intending to become permanent residents. Using data from full-count census samples and published 1890 reports, the study finds that homeownership rates increased with age for both native- and foreign-born families. Surprisingly, immigrants often had higher homeownership rates than natives after adjusting for city size and other factors. The study also shows that successive immigrant cohorts did not experience declining skills, and that cross-sectional homeownership patterns by age accurately predicted cohort trajectories over time.

The second study is by [Haines and Goodman \(1991\)](#) which examines patterns of homeownership among native- and foreign-born households in the United States during the late nineteenth and early twentieth centuries, with a focus on life-cycle accumulation of wealth. Using microdata from the 1865 New York State census, the 1889/90 U.S. Commissioner of

Labor survey, and the 1900 and 1930 censuses, the study investigates how age, region, rural-urban residence, and nativity influenced the probability of owning a home. The study finds that homeownership historically differed by nativity and race. Foreign-born whites initially had lower ownership rates than natives, particularly in urban areas, but these differences narrowed over time and had largely disappeared by 1930, indicating assimilation within a generation. Age patterns showed a crossover in the 1889/90 labor survey, with immigrants owning less than natives at younger ages but more at older ages, especially among German and Irish immigrants. Regional location, income, occupation, and family composition influenced these patterns, and ethnic differences persisted to some extent even after controlling for these factors.

Third study is by [Haines and Goodman \(1989\)](#) which investigates homeownership and housing demand among urban industrial workers in the United States around 1889–1890, with particular attention to differences between native-born and foreign-born households. The study draws on data from the Sixth and Seventh Annual Reports of the U.S. Commissioner of Labor, covering about 6,800 worker families across nine industries in 24 states. Using probit models for homeownership and renter demand, with expected income rather than current income as a key predictor, the study finds notable differences between native and foreign-born workers. Irish, German, and Slavic immigrants were more likely to own homes than natives, whereas British, Canadian, and other immigrant groups had lower ownership rates. Some of these differences reflected the higher likelihood of returning to their country of origin, especially for British and Canadian migrants. Ethnic differences remained even after accounting for income, age, household composition, industry, occupation, and region, though not all effects were statistically significant. Among renters, most foreign-born households, except Germans, spent less on housing than natives, revealing variation in housing demand across immigrant groups.

Lastly, [Rosenbaum and Friedman \(2007\)](#) address whether social-scientific evidence supports concerns that immigrants are unable or unwilling to assimilate into American society with particular emphasis on the role of race in shaping access to housing opportunities for both native-born and foreign-born households. The study uses a unique dataset com-

binning household- and unit-level observations from the 1999 New York City Housing and Vacancy Survey (HVS) with detailed neighborhood characteristics, including information on the birthplace of the householder. The findings suggest that higher income and education, along with lower levels of economic need, are associated with higher homeownership rates, better housing quality, and residence in neighborhoods with greater socioeconomic resources. However, the results also reveal sharp racial disparities. Households of African heritage and darker-skinned Latino households, whether native-born or foreign-born, occupy substantially lower-quality housing and neighborhoods than white households with similar socioeconomic characteristics. Moreover, among Black households, housing and neighborhood conditions deteriorate across generations, indicating a pattern of downward mobility. These findings suggest that racial barriers significantly constrain the assimilation process and that immigrants of visible Black race face persistent obstacles to full incorporation, leading to their continued concentration in lower tiers of the social and housing hierarchy.

2.2.2 Immigration and Homeownership in Recent Periods

There is a vast body of literature which examines several important aspects of homeownership in relation to immigration. The first strand of the literature examines homeownership assimilation and how it varies across immigrant racial groups. One of the most widely cited contributions is [Borjas \(2002\)](#), which analyze trends in immigrant homeownership and investigates the sources of differences in homeownership between immigrant and native households in the United States over the 1980–2000 period. Using data from the U.S. Censuses and the Current Population Surveys, the study finds that immigrant households had substantially lower homeownership rates than natives and that this gap widened significantly over time. Differences in income and household characteristics accounted for only a small share of the gap. Instead, the national origin composition of the immigrant population and immigrants' residential location choices played a central role. In addition, the formation and expansion of ethnic enclaves in major U.S. cities increased the likelihood of homeownership among immigrant households.

Similarly, [Myers and Lee \(1998\)](#) study homeownership assimilation among immigrants in Southern California. Using census data from 1980 and 1990 and applying a double cohort methodology, they show that immigrants make substantial progress into homeownership, although the patterns differ across groups. Asian immigrants attain high homeownership rates soon after arrival, often matching or exceeding those of native born households of similar age. In contrast, Hispanic immigrants start with very low homeownership rates but experience steady and rapid advancement over time. Although Hispanics do not fully converge to native born homeownership levels, their rate of progress into homeownership is faster than that of native born households moving through the same age range.

In another paper, [Myers et al. \(1998\)](#) estimate homeownership trajectories for native-born non-Hispanic whites, native-born Mexican Americans, and Mexican immigrants by birth cohort and, for immigrants, by arrival cohort using census data from 1980 to 1990. They show that aging, cohort membership, and duration of U.S. residence are strong predictors of homeownership attainment. After adjusting for income, education, marital status, and local housing costs, younger cohorts of native-born whites and Mexican Americans exhibit lower homeownership trajectories than older cohorts, while immigrant homeownership increases with time spent in the United States, reflecting assimilation effects distinct from aging. [Chakrabarty et al. \(2019\)](#) study the evolution of immigrant and minority homeownership in the United States between 2000 and 2015. Using Census and American Community Survey (ACS) data, they estimate linear probability models with socioeconomic controls and Metropolitan Statistical Area (MSA) fixed effects, comparing outcomes to non-Hispanic white natives. They reveal substantial heterogeneity across groups: several Asian and Mexican immigrant groups experienced relative gains in homeownership, while Black natives, Mexican natives, and Cuban immigrants lost ground. Observable characteristics and location explain part, but not all, of these gaps. The analysis further shows that homeownership patterns differ across traditional gateways, emerging gateways, and non-gateway areas, with Asian groups performing relatively better in gateways and Mexican immigrants showing stronger gains in non-gateway locations.

[Chatterjee and Zahirovic-Herbert \(2011\)](#) study differences in homeownership and housing

equity between immigrants and native-born households using the National Longitudinal Survey of Youth (NLSY79). Employing a two-stage model and Oaxaca–Blinder decomposition, they show that immigrants are less likely to own homes, but conditional on ownership, they hold significantly more housing equity than native-born households. Differences in observable characteristics explain roughly two-thirds of the homeownership gap, while the remainder reflects distinct homeownership decision processes across groups. The results suggest that credit constraints and limited access to financial markets may lead immigrant homeowners to accumulate larger down payments and higher initial equity, highlighting divergent paths of housing wealth assimilation between immigrants and natives.

Another body of literature looks at how the great recession affected the homeownership of natives and immigrants. For instance, [Mundra and Uwaifo Oyelere \(2017\)](#) study the determinants of immigrant homeownership in the United States before and after the Great Recession, with a particular focus on how the importance of key factors changed during and after the crisis. Using the March Current Population Survey for the period 2000–2012, the authors restrict the analysis to immigrant households and compare a pre-recession period (2000–2006) with the recession and post-recession period (2007–2012). The paper analyzes how income, savings, length of stay in the United States, citizenship status, family characteristics, and birthplace networks affect the probability of immigrant homeownership. A central contribution of the study is its measurement of immigrant networks at the birth-country level and its use of a proxy for household savings. The results show that citizenship, income, savings, length of stay, and birthplace networks are significant predictors of immigrant homeownership, and that the role of birthplace networks increased substantially after the Great Recession, while the influence of savings declined slightly.

Earlier, [Painter and Yu \(2014\)](#) analyzed the impact of Great Recession on immigrant housing outcomes in the United States, with a focus on Latino and Asian immigrants. Using microdata from the 2006 and 2009 American Community Survey, they examined changes in homeownership and household headship across different metropolitan areas before and after the recession with a particular attention to differences between traditional immigrant gateway cities and newer immigrant destinations. They find that housing outcomes worsened

after the recession, with immigrants experiencing lower homeownership and reduced residential mobility. However, the negative effects were generally less severe for immigrants than for U.S.-born households. Declines in homeownership were largest in established gateway metropolitan areas, while immigrants in newer or smaller destinations experienced smaller reductions and, in some cases, improvements in housing outcomes. They further find that higher metropolitan unemployment rates and mortgage delinquency rates are associated with lower homeownership, highlighting the importance of local labor and housing market conditions. At the same time, stronger immigrant networks at the metropolitan level mitigated some of the negative effects of the recession, increasing the likelihood of homeownership, although they were also associated with lower household headship rates after the recession.

2.2.3 Immigration Shocks and Housing Outcomes in the Early 20th Century

Although there is very little literature on how immigration shocks affected housing outcomes and native mobility in the United States during the early twentieth century, we identify one study by [Cortes and Sant’Anna \(2023\)](#) that is closely related to our analysis in both historical period and focus on housing markets and therefore merits more detailed discussion.

The paper examines the effects of the 1930s mass repatriation of Mexican immigrants on U.S. housing markets, exploiting a large and plausibly exogenous immigrant outflow shock. Methodologically, the authors construct a novel house-level panel by developing an automated address-matching algorithm that links individual housing units across the 1930 and 1940 Censuses. This allows them to directly track changes in house values and rents over time at the property level.

To identify the causal impact of repatriation, the study employs an instrumental variables strategy that combines cities’ historical Mexican population shares with variation in transportation costs to the U.S.–Mexico border, based on 1930s road infrastructure. This approach addresses concerns that repatriation intensity may be correlated with local economic conditions during the Great Depression. The baseline specifications include state

fixed effects and controls for sectoral employment composition to further isolate the effect of immigrant outflows on housing outcomes.

The results show large and negative effects of repatriation on housing markets. Houses occupied by Mexican households in 1930 experienced substantial declines in both values and rents following repatriation, with no evidence that U.S.-born households moved into these vacated units. Instead, the decline spilled over to nearby U.S.-born-occupied properties, reducing neighboring house values and rents. At the city level, higher exposure to repatriation led to significantly slower growth in median house values, rents, and building permits, indicating a broader slowdown in real estate market activity rather than improved housing affordability for natives.

Another study also relevant in terms of time period and housing market dynamics, though it focuses on the United Kingdom rather than the United States is by [Meen \(2012\)](#). It examines the relationship between migration and housing markets in the United Kingdom from a long-run historical perspective, with particular attention to the spatial concentration of migrant communities and local housing market adjustment. Using population census data available since the mid-nineteenth century, the analysis documents strong persistence in the geographic distribution of migrant groups, especially in urban areas such as London. The results emphasize the importance of social networks and path dependence in shaping migrant location choices, while housing market factors play a secondary role.

The study also evaluates the impact of migration on domestic housing markets and finds limited evidence that migrant inflows substantially raise house prices or rents. Instead, housing market adjustment occurs through alternative channels, including changes in household formation, overcrowding, and the residential mobility of native households. These mechanisms mitigate price pressures and help explain why large migration waves do not necessarily translate into strong observable effects on housing prices.

2.2.4 Immigration Shocks and Housing Outcomes and Native Mobility in Recent Periods

The debate over whether immigration affects local housing markets is often traced to Saiz's study of the Mariel boatlift in Miami (Saiz, 2003). In 1980, a sudden and largely unanticipated inflow of Cuban immigrants increased the city's renter population by about 9% in a single year. Saiz exploits this exogenous shock to estimate its short run effects on rents and housing prices, comparing Miami with three other metropolitan areas that did not experience a similar inflow.

He finds that rents in Miami increased between 8% and 11% more than in the comparison areas between 1979 and 1981. These increases were concentrated in lower quality rental units, which were most accessible to low income and Hispanic residents. Higher quality units, by contrast, were largely unaffected. Housing prices moved in the opposite direction from rents in the short run, suggesting that the market expected longer run adjustments in property values, rather than permanent immediate effects. The results highlight the segmented nature of the rental market and the inelasticity of the short run supply of low quality housing. He also documents a rapid change in the ethnic composition of the city over the four years following the boatlift, reflecting the concentration of new immigrants in particular neighborhoods. However, the study does not provide direct estimates of native residential mobility or long run effects on housing patterns. Instead, it emphasizes how sudden immigrant inflows can alter the distribution of rents and the short run dynamics of urban housing markets.

Building on his earlier work on the Mariel boatlift, Saiz (2007), in this study examines the broader local economic impact of immigration in U.S. metropolitan areas. Using annual data on legal immigration inflows, decennial Census data on the foreign-born population, rents, and housing values, the study estimates how immigrant arrivals affect local housing markets. To address endogeneity concerns, the author employs a shift-share instrument. The results show that immigration inflows significantly increase rents and housing values. Specifically, an inflow equal to 1% of a city's population is associated with roughly a 1% increase in average

rents and home values. Housing prices tend to increase following the initial rise in rents. The study also shows that immigrants do not displace natives on a one-for-one basis, but higher rents and housing values in areas with inelastic housing supply may influence the residential decisions of some native households.

[Saiz \(2010\)](#) examines how growth in immigrant and Hispanic populations affects housing rents and prices in U.S. cities. Using metropolitan-level data and within-city case studies of Houston, Los Angeles, and Chicago, the paper finds that cities with rapidly growing Hispanic and immigrant populations in the 1980s and 1990s experienced higher overall housing rents and prices. Within metropolitan areas, however, neighborhoods with faster Hispanic population growth tended to have slower relative price appreciation, indicating rising residential segregation. The findings suggest that immigration increases overall housing demand, raising city-level prices, while also interacting with native preferences, as non-Hispanic residents appear willing to pay premiums to avoid neighborhoods with growing Hispanic concentrations.

In a subsequent work [Saiz and Wachter \(2011\)](#), study how growing immigrant populations affect neighborhood housing values and native residential patterns within U.S. metropolitan areas. Using a geographic diffusion model as an instrument for changes in local immigrant density, the analysis accounts for unobservable shocks that may be correlated with proximity to existing immigrant enclaves. The results suggest that neighborhoods experiencing larger inflows of immigrants, particularly those who are low-income or belong to minority groups, become relatively less attractive to natives. In areas where the share of foreign-born residents increases from zero to 30%, housing values are estimated to be about 6% lower. The negative effects on neighborhood values are especially pronounced in high-income areas that were predominantly non-Hispanic white prior to the immigration shock. The study also links these patterns to residential segregation preferences. Natives appear willing to pay a premium for living in predominantly native areas, contributing to the persistence of immigrant enclaves. While immigrants tend to cluster spatially due to social networks and other factors, native preferences amplify the relative negative impact on housing values.

In a related work, [Mussa et al. \(2017\)](#) examine how immigration affects housing markets

in the United States, with a focus on spatial spillovers across metropolitan areas. They consider both positive and negative mechanisms: immigration increases population and housing demand, which can raise rents and house prices, but may also reduce wages, induce native out-migration, and lower local housing demand.

Using a spatial econometric approach, specifically a Spatial Durbin Model, they capture both direct effects of immigration within MSA and spillover effects on neighboring MSAs. Instrumental variables are employed to address potential endogeneity in immigration flows, with data covering 2002 to 2012. They show that immigration inflows raise housing rents and prices locally, with a 1% increase in immigration relative to the MSA population raising rents by about 0.8 percent. Importantly, immigration also affects housing markets in neighboring areas, highlighting the role of spatial dependence and immigrant networks in shaping settlement patterns. They demonstrate that ignoring geographic spillovers would understate the full effect of immigration on housing markets.

Most recently, [Bian et al. \(2025\)](#) examine how immigration affects local housing price dynamics in the United States, focusing on both average housing price appreciation at the county level and the dispersion of price changes within counties. It uses U.S. county-level housing price data combined with zip-code-level information on housing price appreciation and measures of non-European immigration inflows, along with county demographic characteristics such as education, age composition, and racial bias. To address the endogeneity of immigrant settlement, the authors employ a newly developed immigration shock instrument from [Burchardi et al. \(2019\)](#), which exploits exogenous variation in origin-country conditions and avoids concerns associated with traditional shift-share instruments. The results show that an immigration inflow equal to one percent of a county's initial population raises housing price appreciation by about 6.8 percentage points and reduces within-county dispersion in housing price growth by roughly 1.5 percentage points. These effects are highly heterogeneous and are concentrated in counties with younger, more educated, and less racially biased populations, indicating that local attitudes toward immigrants play a central role in shaping how immigration influences housing markets.

[Ottaviano and Peri \(2006\)](#) examine the economic value of cultural diversity in U.S. cities,

focusing on the period from 1970 to 1990. They consider how the presence of foreign-born residents contributes to productivity and local amenities. They measure cultural diversity using a fractionalization index based on country of birth, capturing the probability that two randomly selected residents were born in different countries. They cover 160 major metropolitan areas with varying levels of diversity and use an open-city framework that leverages the high mobility of native-born residents. The empirical strategy accounts for the possibility that immigrants select into already prosperous cities by using shift-share instrumental variables based on national immigration trends. The main findings show that cultural diversity is positively associated with higher rents and higher wages for U.S.-born residents. Higher rents reflect enhanced consumption amenities valued by residents while higher wages suggest productivity gains from the variety of skills and knowledge brought by immigrants.

[Howard \(2020\)](#) Howard (2020) examines the short- and medium-run effects of U.S. domestic migration on local labor markets, emphasizing the role of housing. Traditional economic models assume that labor mobility stabilizes local economies by smoothing shocks, but these models often ignore housing constraints. Using annual IRS migration data and a shift-share instrumental variable approach based on historical migration patterns, the study isolates the causal effects of in-migration while addressing reverse causality. The key finding is that migration raises housing demand, which in turn boosts construction employment, house prices, mortgage activity, and consumption of non-tradable goods, creating positive local labor market externalities. These housing channels are especially strong in cities with inelastic housing supply or rapidly growing populations. For the labor market they find that in-migration both responds to labor demand shocks and amplifies them. A 1% increase in local population through in-migration reduces unemployment cumulatively by about 2.1 percentage-point-years, with effects dissipating after roughly six years.

Beyond the United States, studies from Europe also provide similar evidence that immigrants affect rents and housing prices. For instance, [Gonzalez and Ortega \(2013\)](#) analyze the role of immigration in Spain's housing boom during 1998–2008. Spain experienced a massive increase in housing prices alongside large immigration inflows, with the foreign-born share

of the working-age population rising from 2 to 16 percent. Using regional variation and instruments based on geographic accessibility, the study finds that a 1% increase in population due to immigration raises house prices by 1% – 1.6% and stimulates a 0.8% – 1% increase in housing construction. Similarly, [Accetturo et al. \(2014\)](#) focus on neighborhood-level effects of immigration in 20 large Italian cities between 2003 and 2010. They show that while immigration raises city-level housing prices, districts receiving more immigrants experience slower relative housing price growth. This pattern is driven by native out-migration, particularly in areas with constrained housing supply, reflecting natives’ perceptions of declining local amenities. Using a spatial model and instrumental variables based on historical settlement patterns, the study demonstrates how immigration interacts with local amenities to shape housing markets and urban segregation.

2.3 Data

We use two data sources in our analysis: the full-count U.S. Census data from 1900 to 1930 ([Ruggles et al., 2024](#)) and the Multigenerational Longitudinal Panel (MLP) ([Ruggles et al., 2025](#)) from IPUMS covering 1910 to 1930. The MLP links individuals across census years and is constructed by matching individuals across full-count census samples using characteristics such as name, sex, year of birth, and birthplace ([Helgertz et al., 2022](#)).

To obtain a clean sample for analysis, we exclude individuals in group quarters and retain only private households. Since homeownership status is recorded at the household level in the census, we keep a single observation per household by retaining only the household head. As a result, the birthplace of the household head determines whether the household is classified as native or foreign born. All other characteristics used in the analysis are likewise defined at the household head level. In addition, we restrict the sample to male household heads between the ages¹ of 18 and 74.

To construct our main variable of interest, the fraction of a city’s population made up of

¹In the linked census samples, we restrict the sample to male household heads who are at least 18 years old in their first observed year and no older than 74 years old in their final observed year.

immigrants who arrived in the last decade, as well as the instrumental variables described in the next section, we use the full-count U.S. Census data from 1900 to 1930. Because this variable is defined at the city level, we follow vLWP (2020) and restrict the sample to households residing in one of 471 cities in the initial census year, although they may later move to a city outside this group or to a non-urban area in the next census period. Since approximately 85% of immigrants during this period originated from Europe, we follow [Tabellini \(2020\)](#) and vLWP (2020) and restrict our analysis to European immigrants, classifying these birthplaces into 21 origin groups.

We define five samples for the analyses in this essay. The first sample consists of all native households observed in the full-count censuses of 1910, 1920, and 1930, pooled together after imposing the restrictions described above. This sample is used for the repeated cross-sectional (RCS) analysis and includes all native households, whether or not they can be linked forward or backward to an adjacent census. The second sample is a subset of the first and is used to assess potential selection bias arising from linkage. It includes only those native households that are successfully linked to an adjacent census.

The third sample is used for the analysis of transitions into and out of homeownership. It consists of native households that are successfully linked to the subsequent census. For the transition into homeownership analysis, it includes native households that are renters in the first census in which they are observed and transition into homeownership in the subsequent census. For the transition out of homeownership analysis, it includes native households that are homeowners in the first census in which they are observed and transition into renting in the subsequent census.

The fourth sample is used for the analysis of household formation. It consists of young natives aged 15 to 25 who are successfully linked to the subsequent census. From this group, we retain those who are reported as non-household heads in the first census in which they are observed. The fifth sample is a subset of the fourth sample and includes only those individuals who successfully transition into household headship in the subsequent census.

2.3.1 Inverse Probability Weighting

A central feature of the analysis is the use of linked census data to study household outcomes across decades. Because successful linkage is not random, estimates based on linked samples may be subject to selection bias. A large literature has documented that linkage success is systematically related to observable characteristics such as age, mobility, and socioeconomic status. For example, individuals with common names or inconsistent reporting of age are more difficult to match across records, and individuals who move across locations or change household composition are less likely to be successfully linked. More broadly, more stable households, including those with stronger labor market attachment or more permanent housing arrangements, are more likely to be linked across decades. These patterns imply that linked samples may overrepresent more stable or higher socioeconomic status households, which are also likely to differ in outcomes such as homeownership.

To address this concern, we implement inverse probability weighting (IPW) to adjust for differential probabilities of inclusion in linked samples. We construct three sets of IPW weights corresponding to the three linked samples used in the essay: the RCS sample restricted to linked households, the sample used to study transitions into and out of homeownership, and the sample used to study transitions into headship.

The IPW procedure proceeds by modeling the probability that an observation appears in the relevant linked estimation sample as a function of observed characteristics. Let p_i denote the predicted probability that observation i is included in the linked sample. Under the assumption that selection into the linked sample is conditionally independent of outcomes given observed covariates, weighting observations by the inverse of this probability, $1/p_i$, yields consistent estimates of population parameters. In practice, we estimate these probabilities using logit models and construct weights from the fitted values. To improve stability, we bound predicted probabilities away from zero and one and, in some specifications, normalize weights within year.

The specific implementation of the IPW procedure differs across the three linked samples, reflecting differences in the unit of observation and the underlying selection process. In all

cases, the set of covariates includes flexible controls for demographic and socioeconomic characteristics, including age group indicators, race, literacy, marital status, number of children, state of residence, and a quadratic in occupational income score. These variables are chosen to capture factors that jointly predict both linkage success and the outcomes of interest.

We first consider the RCS sample restricted to households that are successfully linked across adjacent decades. In this setting, the unit of observation is a household-year, and the relevant selection problem is whether a given household-year observation appears in the linked RCS sample. Because inclusion depends on linkage across adjacent census years, the definition of inclusion varies by year. For households observed in 1910, inclusion requires successful forward linkage to 1920. For households observed in 1930, inclusion requires successful backward linkage to 1920. For households observed in 1920, inclusion requires successful linkage either backward to 1910 or forward to 1930. We estimate separate logit models by census year, where the dependent variable equals one if the household-year observation is included in the linked RCS sample under these rules and zero otherwise. The estimation sample in each year is restricted to households that are eligible to appear in the RCS in that year, including those located in cities included in the analysis and satisfying the age restrictions. Predicted probabilities from these year-specific models are used to construct weights, which are then applied to the linked RCS regressions. This procedure reweights the linked RCS sample to resemble the full repeated cross section on observables while accounting for the fact that some households contribute one observation and others contribute multiple observations depending on their linkage status.

Second, we construct IPW weights for the transition-based analysis of homeownership, where the unit of observation is a linked household observed in two adjacent census years. Inclusion in this sample requires successful forward linkage from an initial census year to the subsequent census year. We therefore estimate logit models of the probability that a household observed in the initial period is successfully linked to the next census. The dependent variable equals one if the household is linked across the decade and zero otherwise, and the estimation sample is restricted to households that satisfy the baseline sample criteria in the initial period, including age restrictions and residence in cities included in the analysis.

Covariates are measured in the initial period and include the same demographic and socioeconomic controls described above. Because the transition outcomes differ for households that begin as owners and those that begin as renters, we estimate these linkage models separately for the two groups.

Finally, we construct IPW weights for the analysis of transitions into headship. As in the homeownership transition analysis, inclusion in the estimation sample requires successful linkage across adjacent census years. We estimate logit models of the probability that an individual observed in the initial period is successfully linked to the subsequent census. The dependent variable equals one if the individual is observed in both periods and zero otherwise, and the estimation sample is restricted to individuals at risk of transitioning into headship. The set of covariates mirrors that used in the homeownership transition analysis.

Across all three applications, the IPW procedure adjusts for observable differences between linked and unlinked observations. By comparing unweighted and weighted estimates, we assess the extent to which selection into linked samples affects the results. In practice, the weighted estimates are similar to, and in some cases larger than, the unweighted estimates, suggesting that selection on observables does not drive the main findings.

2.4 Methodology

We begin our analysis by examining whether immigration at the city level is associated with natives' housing tenure decisions. For this purpose, we follow [Tabellini \(2020\)](#) and estimate the following regression model:

$$H_{i,ct} = \alpha + \beta \text{FracImmig}_{ct} + \sigma_a + \gamma_c + \gamma_{st} + \varepsilon_{i,ct} \quad (1)$$

where $H_{i,ct}$ is an indicator equal to 1 if native household i owns a home in city c in census year t . FracImmig_{ct} represents the fraction of the population in city c that consists of recent immigrants in census year t .

The model also includes age fixed effects, σ_a , city fixed effects, γ_c , and state-by-year

fixed effects, γ_{st} . Therefore, the coefficient β measures whether an increase in the fraction of immigrants in a city is associated with a higher level of homeownership among the native population, after controlling for differences across age groups, permanent differences across cities, and state-specific changes over time. Identification comes from variation within cities over time in the share of immigrants in the population.

A change in native homeownership in response to immigration can arise either from changes in home buying or changes in home selling among natives in response to immigration. To examine whether changes in immigration encourage or discourage natives from buying and selling homes, we closely follow vLWP and estimate the following regression model:

$$O_{i,ct} = \alpha + \delta \Delta \text{FracImmig}_{ct} + \sigma_a + \gamma_{st} + \varepsilon_{i,ct} \quad (2)$$

where $O_{i,ct}$ is the homeownership transition for householder i , of age a , in city c , in state s , between periods t and $t + 10$. In the home-buying model, $O_{i,ct}$ is an indicator equal to one if a household transitions from renting to owning in the subsequent decade. In the home-selling model, the dependent variable is an indicator equal to one if a household transitions from owning to renting in the subsequent decade. $\Delta \text{FracImmig}_{ct}$ is the change in the fraction of city c 's population that consists of recent immigrants between t and $t + 10$; σ_a denotes age fixed effects that control for life-cycle effects; and γ_{st} denotes state-by-year fixed effects. Because equation (2) is the first-difference transformation of equation (1), the city fixed effects are removed by construction. The coefficient of interest is δ , which measures the relationship between changes in the population of recent immigrants in a city across decades and the outcome being studied.

2.4.1 Instrumental Variable Construction

A key challenge in the study of immigration is that immigrants do not choose their destinations randomly, and their location choices may be correlated with the outcomes of interest. If these location decisions are influenced by housing market conditions or other characteristics of cities, such as local government policies that make home buying or selling easier or

more difficult through higher taxes or tax incentives, then the estimated impact of immigration may be biased. Therefore, treating these choices as exogenous may produce misleading estimates.

Consistent with the literature (Price et al., 2020; Tabellini, 2020), we exploit the tendency of immigrants to settle in cities where earlier migrants from the same birthplace are already present to construct a shift-share instrument. Specifically, to instrument for $FracImmig_{ct}$ in equation (1), we construct the following measure:

$$PredFracImmig_{ct} = \frac{1}{\hat{P}_{ct}} \sum_i \omega_{i,c1900} Immig_{i,-ct} \quad (3)$$

where $\hat{P}_{ct}$ is the predicted population of city c in period t , based on its 1900 population and overall urban population growth, excluding the city's own census region; $\omega_{i,c1900}$ is the fraction of immigrants from birthplace i living in city c in 1900; and $Immig_{i,-ct}$ is the total number of new immigrants from birthplace i who arrived between t and $t + 10$, excluding those who eventually settled in city c . Thus, $PredFracImmig_{ct}$ represents the predicted share of recent immigrants in city c between t and $t + 10$.

This instrument exploits two sources of variation. First, it uses cross-sectional variation in the share of immigrants from each birthplace living across U.S. cities in 1900, captured by $\omega_{i,c1900}$. Second, it uses time variation arising from changes in the total number of immigrants from different birthplaces in a given census year, captured by $Immig_{i,-ct}$.

We modify the instrument in equation (3) by taking decade-to-decade changes in the predicted share of recent immigrants. We use this transformed measure to instrument for the home-buying and home-selling specifications described in equation (2). The resulting instrument is defined as follows:

$$\Delta PredFracImmig_{ct} = PredFracImmig_{ct} - PredFracImmig_{c,t-10} \quad (4)$$

2.4.2 Identifying Assumptions and Threats to Identification

The validity of our instrument depends on two key conditions: relevance and exogeneity. Relevance means that the instrument is strongly related to the fraction of a city’s population made up of recent immigrants. Our analysis in the next section shows a strong correlation between the instrument and immigration, thereby satisfying the relevance condition.

Exogeneity means that the instrument affects native homeownership only through its effect on immigration and not through other factors directly related to housing outcomes. Our key identifying assumption is that cities with higher predicted immigration were not already on different underlying housing market trajectories because of persistent city characteristics.

There are two threats to our identifying assumption. First, if the characteristics of cities that attracted immigrants prior to 1900 continue to influence both migration patterns and housing market outcomes in the post-1900 period, then our identification fails. In this case, we may incorrectly conclude that immigration causally affects native housing outcomes when, in reality, both immigration and native housing market outcomes are driven by persistent city characteristics that act as omitted variables. Second, immigrants from particular birthplaces may continue to move to certain cities not only because earlier migrants from the same birthplace had already settled there, but also because those cities possess persistent characteristics that specifically attract immigrants from those birthplaces over time. For example, Irish immigrants may continue moving to Boston or Italian immigrants to New York not only because earlier migrants from those groups had already settled there, but also because certain economic, social, or cultural characteristics of these cities continue to attract immigrants from those same birthplaces over time.

Following vLWP (2020), we address the first concern by testing whether the immigration flows predicted by our instrument are related to housing market trends that already existed before the immigration shocks we study. For the 1910–1920 transitions, we examine transition trends that existed before 1910, and for the 1920–1930 transitions, we examine trends that existed before 1920.

We address the second concern by focusing on changes in immigration that were driven

by major historical events rather than by characteristics of particular cities. Specifically, following vLWP (2020), we use the large changes in immigration patterns caused by

WWI and the immigration quota laws introduced in the 1920s. WWI sharply disrupted migration from many European countries, while the quota laws of the 1920s placed strict limits on immigration from specific birthplaces. These changes were driven by international events and federal policy rather than by conditions in individual cities. As a result, the variation in immigration generated by these shocks is less likely to reflect persistent city characteristics that continued to attract immigrants from particular birthplaces over time. This allows us to better isolate changes in immigration that are plausibly unrelated to underlying city specific housing market trends.

2.5 Results

Table 2.1 presents the results from estimating equation (1) using the Repeated Cross-sections data under different sample specifications.

The first two columns report results for the full sample, which includes all households that are either successfully linked across adjacent decades or could potentially be linked based on the characteristics described in Section 3.1. The next two columns present results for the linked sample, which consists only of households that are linked to at least one adjacent decade, without applying inverse probability weights. The final two columns report results for the same linked sample after adjusting for inverse probability weights. For each specification, we estimate equation (1) using both OLS and 2SLS (IV) methods.

We find that an increase in the fraction of city’s population who are recent immigrants is associated with higher homeownership rates among natives after controlling for age fixed effects, city fixed effects and state by year fixed effects, as shown by the positive and statistically significant OLS coefficient in the full sample specification. The IV coefficient for this specification also remains positive but is smaller in magnitude than the corresponding OLS estimate. This suggests that endogeneity is an important concern and that the OLS coefficient should not be interpreted as a causal effect. The large value of the first stage

F-statistic is noteworthy, as it indicates a strong relationship between the instrument and fraction of city’s population who are recent immigrants in the first stage regression.

Notice that when we restrict the sample to households linked across at least one adjacent decade, the OLS coefficient declines, suggesting that the association between immigration and native homeownership becomes weaker. In contrast, the IV coefficient increases in magnitude, indicating a larger estimated effect of immigration on native homeownership within the linked sample. However, since this specification does not adjust for inverse probability weights, these results should be interpreted with caution as they may still be affected by selection bias.

Table 2.1: *Homeownership, Repeated Cross-Section*

	Full Sample		Linked, Unweighted		Linked, Weighted	
	OLS	IV	OLS	IV	OLS	IV
Frac Recent Imm	0.432*** (0.123)	0.331* (0.153)	0.392*** (0.114)	0.397** (0.145)	0.451*** (0.109)	0.456** (0.146)
Mean Dep. Var.	0.340	0.340	0.415	0.415	0.380	0.380
F-Statistic		222.6		223.1		191.7
Observations	14,984,581	14,984,581	7,128,676	7,128,676	7,110,758	7,110,758

Notes: Columns (1) and (2) present OLS and 2SLS estimates of equation (1) for the full sample. This sample includes all native male-headed households (both linked and unlinked to an adjacent census) with heads aged 18–74 residing in one of the 471 analyzed cities across the 1910, 1920, and 1930 full-count censuses. Columns (3) and (4) repeat these estimations restricting the sample to native households linked to at least one adjacent census; these regressions are unweighted. Columns (5) and (6) use the same linked sample but apply inverse probability weights to adjust for selection as discussed in Section 2.3.1. The dependent variable in all specifications is a homeownership indicator (1 if the household owns the home and 0 otherwise). *Frac Recent Imm* is the fraction of immigrants who arrived in the previous decade relative to the predicted city population; in the 2SLS specifications, this variable is instrumented using the baseline shift-share instrument defined in equation (3) and described in Section 2.4.1. All models include age fixed effects (by individual year), city fixed effects, and state-by-year fixed effects. For IV specifications, the Kleibergen–Paap (K–P) F-statistic for weak instruments is reported. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The last two columns present our preferred specification, which applies inverse probability weights to account for the likelihood that a household is linked across at least one adjacent census. We find that both the OLS and IV coefficients are larger than in the full sample, and the IV coefficient is also larger than the OLS estimate. This pattern suggests that failing to account for selection into the linked sample attenuates the estimated relationship between immigration and native homeownership, leading to an understatement of the causal effect.

Our interest is in establishing a causal relationship, so we focus on the IV specification.

The estimated coefficient is 0.456. Given that the average change in the fraction of recent immigrants in the data is around 5 percentage points per decade, we use this as a benchmark and scale the estimated coefficient by 0.05 (i.e., divide by 20; (Price et al., 2020; Tabellini, 2020)) when interpreting its magnitude. This implies that a 5-percentage point increase in the fraction of a city’s population that is made up of recent immigrants increases native homeownership by 2.28 percentage points, controlling for age, city and state by year fixed effects. Given the average native homeownership rate of 38.0%, this corresponds to an increase of about 6.0% relative to the mean. This represents a sizable effect on native homeownership.

Table 2.2: *Home Purchase, MLP*

	Linked, Unweighted		Linked, Weighted	
	OLS	IV	OLS	IV
Change Frac Imm	0.965*** (0.108)	1.761*** (0.270)	1.044*** (0.107)	1.964*** (0.246)
Mean Dep. Var.	0.391	0.391	0.376	0.376
F-Statistic		395.8		421.2
Observations	2,633,298	2,633,298	2,633,298	2,633,298

Notes: Columns (1) and (2) report OLS and 2SLS estimates of equation (2), respectively. The sample consists of native male-headed households residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or between 1920–1930, whose heads are at least 18 years old when first observed and no older than 74 years old when last observed. The sample is restricted to households that were renters in the first census in which they are observed. Columns (3) and (4) report OLS and 2SLS estimates, respectively, using the same sample as Columns (1) and (2), but weighted by inverse probability weights to account for selection into linkage as discussed in Section 2.3.1. The dependent variable in all specifications is an indicator equal to one if a household transitions from renting in its initial observed census to homeownership in the subsequent census, and zero if it remains a renter. *Change Frac Imm* denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. In the 2SLS specifications, *Change Frac Imm* is instrumented using the decadal change in the baseline shift-share instrument defined in equation (4) and described in Section 2.4.1. All specifications include age fixed effects and state-by-year fixed effects. Reported F-statistics are Kleibergen–Paap weak-instrument statistics. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Next, we examine whether natives adjust their home buying and selling behavior in response to recent immigration. Table 2.2 reports results for the home buying specification of equation (2), while Table 2.3 presents results for the home selling specification of equation (2). For both outcomes, we estimate the models with and without probability weights to account for selection into being linked across censuses. Comparing weighted and unweighted

specifications shows that the coefficients are larger in absolute value in the weighted models, suggesting that selection into the linked sample attenuates the estimated effects. Similarly, in both tables, the IV coefficients are larger in absolute value than the OLS estimates, indicating downward bias in OLS due to endogeneity.

Table 2.3: *House Sale, MLP*

	Linked, Unweighted		Linked, Weighted	
	OLS	IV	OLS	IV
Change Frac Imm	-0.443*** (0.063)	-0.704*** (0.166)	-0.523*** (0.073)	-0.917*** (0.146)
Mean Dep. Var.	0.163	0.163	0.156	0.156
F-Statistic		256.1		271.6
Observations	1,338,526	1,338,526	1,338,526	1,338,526

Notes: Columns (1) and (2) report OLS and 2SLS estimates of equation (2), respectively. The sample consists of native male-headed households residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or between 1920–1930, whose heads are at least 18 years old when first observed and no older than 74 years old when last observed. The sample is restricted to households that were homeowners in the first census in which they are observed. Columns (3) and (4) report OLS and 2SLS estimates, respectively, using the same sample as Columns (1) and (2), but weighted by inverse probability weights to account for selection into linkage as discussed in Section 2.3.1. The dependent variable in all specifications is an indicator equal to one if a household transitions from homeownership in its initial observed census to renting in the subsequent census, and zero if it remains a homeowner. *Change Frac Imm* denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. In the 2SLS specifications, *Change Frac Imm* is instrumented using the decadal change in the baseline shift-share instrument defined in equation (4) and described in Section 2.4.1. All specifications include age fixed effects and state-by-year fixed effects. Reported F-statistics are Kleibergen–Paap weak-instrument statistics. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The IV coefficient in the probability weighted specification reported in Table 2.2 is 1.964 and is positive and statistically significant, indicating that increases in the decadal change in the fraction of a city’s population made up of recent immigrants raise the likelihood that natives transition into homeownership. Since a 5- percentage point increase in the fraction of recent immigrants represents a typical change in the data, we use this as the benchmark for scaling the coefficient by 0.05. Specifically, a 5-percentage point increase in the change in the recent immigrant share of a city’s population increases the probability of transitioning into homeownership by approximately 9.82 percentage points. Relative to the average transition rate into homeownership of 37.6%, this represents an increase of about 26.1%, indicating a sizable effect on native home buying behavior.

The IV coefficient in the probability weighted specification reported in Table 2.3 is negative and statistically significant, indicating that increases in the change in the fraction of recent immigrants at the city level reduce home selling among natives. The estimated coefficient is -0.917, implying that a 5-percentage point increase in the change in the fraction of a city’s population made up of recent immigrants decreases the probability that natives transition out of homeownership by approximately 4.59 percentage points. Relative to the average transition rate out of homeownership among natives of 15.6%, this corresponds to a decline of about 29.4%, suggesting a substantial reduction in native home selling behavior.

The increase in native homeownership in response to a rise in the fraction of recent immigrants in a city’s population, established in Table 2.1, is therefore consistent with both a significant increase in home buying among natives and a substantial decline in home selling among natives, as shown by the results reported in Tables 2.2 and 2.3.

2.5.1 Mechanisms Linking Immigration to Native Homeownership

What explains the increase in native homeownership in response to immigration at the city level? To better understand the mechanism through which immigration may increase native homeownership, we examine the response of housing prices and rents. Immigration may increase the demand for housing in a city, which can raise home prices and rents and potentially reduce homeownership. At the same time, immigration may also encourage the construction of new housing units if investors expect future housing demand to rise. This increase in housing supply may put downward pressure on home prices and make homeownership more affordable.

To examine how immigration affects housing prices and rents, we use city-level housing data from the [Federal Reserve Bank of Philadelphia Historical Housing Prices Project](#), which provides historical data on housing sale prices and rents over the 20th century using information collected from the real estate sections of historical newspapers. Although the published dataset covers 30 major U.S. cities, we find consistent data for 27 cities across the census decades of 1910, 1920, and 1930. As a result, our final panel consists of 81 city-decade

observations over the 1910–1930 period.

For this analysis, we estimate the following equation:

$$\log(P_{c,t}) = \alpha + \beta \text{FracImmig}_{c,t} + \gamma_c + \delta_t + \varepsilon_{c,t} \quad (5)$$

where $P_{c,t}$ denotes housing prices in city c in census year t , and $\text{FracImmig}_{c,t}$ is the fraction of the population in city c consisting of recent immigrants in census year t . The specification includes city fixed effects, γ_c , and census year fixed effects, δ_t .

Table 2.4: House Prices, City

	Unweighted		Weighted		Weighted (1900 Pop)	
	OLS	IV	OLS	IV	OLS	IV
Frac Recent Imm	-2.967* (1.234)	-2.698 (1.468)	-2.607* (1.169)	-2.633 (1.362)	-2.891** (1.008)	-2.247* (1.076)
F-Statistic		123.5		26.8		27.4
Observations	81	81	81	81	81	81

Notes: The sample is restricted to 27 cities with consistent housing price data in 1910, 1920, and 1930, yielding 81 city-decade observations. The dependent variable is the log of city-level housing prices. *Frac Recent Imm* is the fraction of the city population consisting of immigrants who arrived in the previous decade, relative to predicted city population. In IV specifications, this variable is instrumented using the shift-share instrument defined in equation (3) and described in 2.4.1. Columns (1)–(2) report unweighted OLS and IV estimates of equation (5). Columns (3)–(4) weight observations by contemporaneous city population. Columns (5)–(6) use 1900 city population weights, which are predetermined and avoid endogeneity from population responses to immigration. All regressions include city and census-year fixed effects. Standard errors are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We estimate three versions of equation (5). In the first specification, each city receives equal weight. In the second specification, observations are weighted by contemporaneous city population so that larger cities have a proportionally greater influence on the estimates. However, because city population may itself respond to immigration, such weights may introduce endogeneity into the weighting scheme. In the third specification, we use 1900 city population weights, which are predetermined and fixed prior to immigration shocks, ensuring that weights are not affected by subsequent changes in immigration.

We estimate both OLS and IV specifications for each of the three weighting schemes. Across all specifications, we find a consistent negative relationship between immigration and housing prices. Our preferred estimate is the IV specification using 1900 population weights, which is statistically significant. The coefficient from this specification is 2.247, implying

that a 5 percentage point increase in the fraction of a city’s population consisting of recent immigrants is associated with approximately an 11.2% decrease in city-level housing prices, controlling for city and census-year fixed effects. Taken together, these findings are consistent with a mechanism in which immigration encourages home buying and reduces home selling among natives by lowering housing prices at the city level.

Table 2.5: *Rent, City*

	Unweighted		Weighted		Weighted (1900 Pop)	
	OLS	IV	OLS	IV	OLS	IV
Frac Recent Imm	-0.176 (1.317)	0.325 (1.567)	-1.946* (0.872)	-0.683 (0.967)	-2.109* (0.848)	-0.795 (0.997)
F-Statistic		123.5		26.8		27.4
Observations	81	81	81	81	81	81

Notes: The sample is restricted to 27 cities with consistent housing rent data in 1910, 1920, and 1930, yielding 81 city-decade observations. The dependent variable is the log of city-level housing rents. *Frac Recent Imm* is the fraction of the city population consisting of immigrants who arrived in the previous decade, relative to predicted city population. In IV specifications, this variable is instrumented using the shift-share instrument defined in equation (3) and described in 2.4.1. Columns (1)–(2) report unweighted OLS and IV estimates of equation (5). Columns (3)–(4) weight observations by contemporaneous city population. Columns (5)–(6) use 1900 city population weights, which are predetermined and avoid endogeneity from population responses to immigration. All regressions include city and census-year fixed effects. Standard errors are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In Table 2.5, we re-estimate equation (5) using the logarithm of city-level housing rents as the dependent variable. We again find a negative relationship between immigration and rents, but the estimates are not statistically significantly different from zero. A decline in rents can make the transition from renting to owning more feasible for marginal renters by improving housing affordability and allowing them to save more.

2.5.2 Empirical Validity of the Shift-Share Instrument

The causal estimates in Tables 2.2 and 2.3 are based on the shift-share instruments described in equations (3) and (4). A key concern, as discussed in section 2.4.2, with these estimates is that cities that later receive more immigrants may have already been on different underlying housing market trajectories before the immigration shocks we study. In particular, persistent city characteristics that originally attracted immigrants may also independently shape trends in native homeownership transitions over time. If this is the case, the instru-

ment would partly capture these preexisting differences across cities rather than exogenous variation in immigration. To examine this possibility, we test whether the immigration flows predicted by our instrument are correlated with preexisting trends in native homeownership transitions. Specifically, we modify equation (2) to include measures of pre-trends in native homeownership transitions into and out of homeownership and estimate the following equation:

$$O_{i,ct} = \alpha + \delta \Delta \text{FracImmig}_{ct} + \pi_1 \left(\overline{\Delta(O_{a,c,1900-10})} \times D_{1910} \right) + \pi_2 \left(\overline{\Delta(O_{a,c,1910-20})} \times D_{1920} \right) + \gamma_{st} + \sigma_a + \varepsilon_{i,ct} \quad (6)$$

where, as before, $O_{i,ct}$ is the homeownership transition for householder i , of age a , in city c , in state s , between periods t and $t + 10$. In the home buying model, $O_{i,ct}$ is an indicator equal to one if a household transitions from renting to owning in the subsequent decade. In the home selling model, the dependent variable is an indicator equal to one if a household transitions from owning to renting in the subsequent decade. $\Delta \text{FracImmig}_{ct}$ is the change in the fraction of city c 's population that are recent immigrants between t and $t + 10$; γ_{st} are state-by-year fixed effects; and σ_a are age fixed effects to control for lifecycle effects. The coefficient of interest is δ , which measures the relationship between a change in the population of recent immigrants in a city across decades and the outcome being studied.

The main difference between equations (6) and (2) is the inclusion of two pre-trend controls: $\overline{\Delta(O_{a,c,1900-10})} \times D_{1910}$ and $\overline{\Delta(O_{a,c,1910-20})} \times D_{1920}$. The first term captures average changes in city-level homeownership transitions by age in the decade prior to 1910, while the second captures average changes in city-level homeownership transitions by age in the decade prior to 1920, each interacted with the relevant census period indicator. These controls allow us to account for preexisting differences in housing market trends across cities that could be correlated with later immigration shocks.

For the instrument to be robust to pre-trends, the coefficient on the main variable of interest, $\Delta \text{FracImmig}_{ct}$, should remain stable in magnitude and statistically comparable to the baseline estimates after including the pre-trend controls. A substantial change in this coefficient after accounting for preexisting homeownership transition trends would indicate that baseline estimates may be driven in part by differential trends across cities rather than

Table 2.6: Pre-Trend Analysis, MLP

	Bought		Sold	
	Main	Pre-Trend	Main	Pre-Trend
Change Frac Imm	1.964*** (0.246)	1.338*** (0.158)	-0.917*** (0.146)	-0.795*** (0.125)
$\overline{\Delta(O_{a,c,1900-10})} \times D_{1910}$		0.418*** (0.051)		0.070* (0.030)
$\overline{\Delta(O_{a,c,1910-20})} \times D_{1920}$		0.349*** (0.073)		0.074** (0.024)
Mean Dep. Var.	0.376	0.376	0.156	0.156
F-Statistic	421.2	399.6	271.6	293.9
Observations	2,633,298	2,632,918	1,338,526	1,338,230

Notes: The sample consists of native male-headed households residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or between 1920–1930. Household heads are required to be at least 18 years old when first observed and no older than 74 years old when last observed. Columns (1)–(2) retain households that are renters in the first census in which they are observed and report 2SLS estimates of equation (2) without and with pre-trend controls for home buying. Columns (3)–(4) retain households that are homeowners in the first census in which they are observed and report 2SLS estimates for home selling, with Column (4) including analogous pre-trend controls. *Change Frac Imm* denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. It is instrumented using the shift-share instrument defined in equation (4) and described in Section 2.4.1. All specifications use inverse probability weights to account for selection into linkage described in 2.3.1. All regressions include age fixed effects and state-by-year fixed effects. Standard errors are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

immigration effects alone. Table 2.6 reports results for the home buying and home selling specifications in equation (6), alongside the corresponding baseline estimates from equation (2).

The results indicate that the coefficient on the change in the fraction of a city’s population made up of recent immigrants declines from 1.964 in the baseline specification to 1.338 once the pre-trend controls are included in the transition into homeownership regression. The estimated coefficient is also statistically different from the baseline estimate. In addition, both pre-trend controls are large in magnitude and statistically significant for home buying, suggesting that earlier city-level trends in homeownership transitions are correlated with subsequent immigration flows. Taken together, these findings imply that part of the baseline effect was capturing persistent city-level factors that influenced both immigration and native housing transitions. The combined evidence from the decline in the immigration coefficient and the strong significance of the pre-trend controls therefore raises concerns about the causal interpretation of the estimated effects for transitions into homeownership.

The results for the corresponding specification of transitions out of homeownership show that the coefficient on the fraction of a city’s population made up of recent immigrants also declines in absolute value, from -0.917 in the baseline specification to -0.795 once the pre-trend controls are included. However, unlike the home buying specification, the estimated coefficient in this case is not statistically different from the baseline estimate. This suggests that the estimated effect of immigration on transitions out of homeownership is less sensitive to the inclusion of preexisting trends and is therefore more consistent with a causal interpretation. In addition, the pre-trend controls in the home selling regressions are smaller in magnitude and estimated less precisely than those in the home buying regressions, indicating that earlier city-level trends in transitions out of homeownership are less strongly associated with subsequent immigration flows. These findings increase our confidence that the estimated effects for transitions out of homeownership reflect a causal relationship.

We re-estimate our main specifications using the alternative instrument based only on the variation in immigration generated by WWI and the immigration quota laws of the 1920s. The results are nearly identical to our baseline estimates in both magnitude and statistical significance. This suggests that our findings are not driven by persistent city birthplace specific factors that continued to attract immigrants from particular countries over time. Instead, the results indicate that the estimated effects are robust to using immigration variation generated by large external geopolitical and policy shocks that were unrelated to underlying city-level housing market conditions.

2.5.3 Impact on Household Formation and Homeownership of New Households

While our previous results suggest that immigration is likely to increase native homeownership, it may also affect the formation of new households. We define household formation as the likelihood that young natives who are initially observed as non-household heads in their first observed census transition into household headship in the subsequent decade. We therefore examine whether immigration affects the probability of this transition. To do so,

we retain a sample of individuals between the ages of 15 and 25 who were not household heads in their first observed census year and re-estimate equation (2) with the following modification:

$$F_{i,ct} = \alpha + \tau \Delta \text{FracImmig}_{ct} + \sigma_a + \gamma_{st} + \varepsilon_{i,ct} \quad (7)$$

where $F_{i,ct}$ is an indicator variable equal to one if the young native i , of age a , in city c , in state s , is not a household head in period t but transitions into household headship by period $t + 10$, and equal to zero if the individual remains a non-household head over the same period. We retain the change in the fraction of a city's population made up of recent immigrants ($\Delta \text{FracImmig}_{ct}$) as the main independent variable and include age fixed effects (σ_a) and state-by-year fixed effects (γ_{st}). The coefficient of interest, τ , measures the relationship between changes in the share of recent immigrants in a city across decades and the probability of transitioning into household headship.

We estimate four specifications of this equation: models with and without inverse probability weights, each estimated using both OLS and 2SLS. The results are presented in Table 2.7.

Across all four specifications, we find a consistent positive and statistically significant relationship between immigration and household formation, indicating that immigration increases the likelihood of household formation among natives. Our preferred specification uses inverse probability weights to account for selection into household formation. The IV coefficient in this specification is 0.766, implying that a 5-percentage point increase in the fraction of a city's population made up of recent immigrants raises the probability of new household formation by approximately 3.83 percentage points. Relative to the average household formation rate of 45.2%, this corresponds to an increase of about 8.5%, indicating a substantial effect on household formation among natives.

These newly formed households may subsequently choose either renting or homeownership. Since immigration affects the likelihood of transitioning into homeownership, it may also influence homeownership decisions among these newly formed households. Because

these young natives were not household heads when first observed, they were excluded from the estimation sample used for equation (2). Therefore, they do not contribute to the corresponding results in Table 2.2 on the impact of immigration on home buying. Consequently, the estimates in Table 2.2 may not fully capture the overall effect of immigration on native homeownership.

Table 2.7: *Transition into Head of Household, MLP*

	Linked, Unweighted		Linked, Weighted	
	OLS	IV	OLS	IV
Change Frac Imm	0.445*** (0.102)	0.832*** (0.174)	0.422*** (0.094)	0.766*** (0.159)
Mean Dep. Var.	0.431	0.431	0.452	0.452
F-Statistic		451.3		521.5
Observations	3,466,875	3,466,875	3,466,875	3,466,875

Notes: Columns (1) and (2) report OLS and 2SLS estimates of equation (7), respectively. The sample consists of native male individuals residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or between 1920–1930. Individuals are restricted to those aged 15–25 who are not household heads in their first observed census year. Columns (3) and (4) report OLS and 2SLS estimates using the same sample as Columns (1) and (2), but weighted by inverse probability weights to account for selection into linkage as discussed in Section 2.3.1. The dependent variable is an indicator equal to one if an individual transitions from non-household head status in the initial observed census to household headship in the subsequent census, and zero otherwise. *Change Frac Imm* denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. In the 2SLS specifications, *Change Frac Imm* is instrumented using the decadal change in the baseline shift-share instrument defined in equation (4) and described in 2.4.1. All specifications include age fixed effects and state-by-year fixed effects. Reported F-statistics are Kleibergen–Paap weak-instrument statistics. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

To analyze whether immigration affects the likelihood of transitioning into homeownership among newly formed households, we restrict the sample to young natives who were non-household heads in their first observed census and who transitioned into household headship in the subsequent census (i.e., $F_{i,ct} = 1$). We then estimate equation (8) using this restricted sample.

$$T_{i,ct} = \alpha + \phi \Delta \text{FracImmig}_{ct} + \sigma_a + \gamma_{st} + \varepsilon_{i,ct} \quad (8)$$

The dependent variable, $T_{i,ct}$, is an indicator for transitioning into homeownership among these newly formed households. We retain the change in the fraction of a city’s population made up of recent immigrants ($\Delta \text{FracImmig}_{ct}$) as the main independent variable and include age fixed effects (σ_a) and state-by-year fixed effects (γ_{st}). We estimate four specifications of this regression: models with and without inverse probability weights, each estimated using

both OLS and 2SLS.

Table 2.8 presents the results. Across all four specifications, we find a consistent positive and statistically significant effect of immigration on transitions into homeownership among newly formed households. The IV coefficient in the probability-weighted specification is 1.733, implying that a 5-percentage point increase in the change in the fraction of a city’s population made up of recent immigrants increases the likelihood that new household heads transition into homeownership by approximately 8.67 percentage points. Relative to the average transition rate into homeownership of 27.0% among new household heads, this corresponds to an increase of about 32.1%, indicating a large effect.

Table 2.8: *Owner among New Heads of Households, MLP*

	Linked, Unweighted		Linked, Weighted	
	OLS	IV	OLS	IV
Change Frac Imm	0.916*** (0.124)	1.727*** (0.232)	0.971*** (0.115)	1.733*** (0.221)
Mean Dep. Var.	0.266	0.266	0.270	0.270
F-Statistic		388.6		465.8
Observations	1,491,946	1,491,946	1,491,946	1,491,946

Notes: The sample consists of native male individuals residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or between 1920–1930. Individuals are restricted to those aged 15–25 who are not household heads in their first observed census year and who transition to household headship in the subsequent decade. The sample is further restricted to individuals who are renters in their first observed census year. Columns (1) and (2) report OLS and 2SLS estimates of equation (8), respectively. Columns (3) and (4) report OLS and 2SLS estimates using the same sample as Columns (1) and (2), but weighted by inverse probability weights to account for selection into linkage as discussed in Section 2.3.1. The dependent variable is an indicator equal to one if an individual transitions from renting in the initial observed census to homeownership in the subsequent census, and zero otherwise. *Change Frac Imm* denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. In the 2SLS specifications, *Change Frac Imm* is instrumented using the decadal change in the baseline shift-share instrument defined in equation (4) and described in 2.4.1. All specifications include age fixed effects and state-by-year fixed effects. Reported F-statistics are Kleibergen–Paap weak-instrument statistics. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Overall, we find that immigration increases native homeownership both by raising the likelihood that young non-homeowners become homeowners and by increasing homeownership among newly formed households.

If there exist persistent city characteristics that influence household formation ($F_{i,ct}$) among natives and also attract immigrants, then the IV estimates in Table 2.7 may partly capture the effects of those characteristics rather than the causal effect of immigration. Similarly, if these city characteristics affect both immigration and transitions into homeownership ($T_{i,ct}$), then the estimates in Table 2.8 may reflect the influence of these characteristics on

homeownership among newly formed households rather than the effect of immigration itself.

Table 2.9: *Transition into Head of Household and Ownership with Pre-trend Analysis, MLP*

	Home Formation		Transition to Homeownership	
	Main	Pre-Trend	Main	Pre-Trend
Change Frac Imm	0.766*** (0.159)	0.594*** (0.129)	1.733*** (0.221)	1.599*** (0.219)
$\overline{\Delta(F_{a,c,1900-10})} \times D_{1910}$		0.204*** (0.028)		
$\overline{\Delta(F_{a,c,1910-20})} \times D_{1920}$		0.292*** (0.031)		
$\overline{\Delta(T_{a,c,1900-10})} \times D_{1910}$				0.113 (0.060)
$\overline{\Delta(T_{a,c,1910-20})} \times D_{1920}$				0.238** (0.060)
Mean Dep. Var.	0.452	0.452	0.270	0.270
F-Statistic	521.5	511.7	465.8	468.6
Observations	3,466,875	3,464,008	1,491,946	1,491,195

Notes: The sample consists of native male individuals residing in one of the 471 cities included in the analysis and linked either between 1910–1920 or 1920–1930. Individuals are restricted to those aged 15–25 who are not household heads in their first observed census year. Columns (1) and (2) report 2SLS estimates of equation (7) without and with controls for pre-trends in transitions into household headship, respectively. Columns (3) and (4) report 2SLS estimates of equation (8) without and with controls for pre-trends in transitions into homeownership among newly formed households, respectively. The dependent variable in Columns (1) and (2) is an indicator equal to one if an individual transitions from non-household head status in the initial observed census to household headship in the subsequent census. The dependent variable in Columns (3) and (4) is an indicator equal to one if an individual transitions from renting in the initial observed census to homeownership in the subsequent census, conditional on transitioning to household headship. Change Frac Imm denotes the decadal change in the fraction of a city’s population consisting of immigrants who arrived during the previous decade, divided by predicted city population. In the Main specifications, Change Frac Imm is instrumented using the decadal change in the baseline shift-share instrument defined in equation (4) and described in Section 2.4.1. All specifications are estimated using inverse probability weights to account for selection into linkage, as discussed in Section 2.3.1. All specifications include age fixed effects and state-by-year fixed effects. Reported F-statistics are Kleibergen–Paap weak-instrument statistics. Standard errors (in parentheses) are clustered at the metropolitan statistical area (MSA) level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We therefore modify equations (7) and (8) to control for pre-trends in household formation and homeownership transitions among newly formed households, respectively. In the modified equation (7), we interact the average transition into household headship for each age group between 1900 and 1910 with an indicator for census year 1910, $\overline{\Delta(F_{a,c,1900-10})} \times D_{1910}$, and the average transition into household headship for each age group between 1910 and 1920 with an indicator for census year 1920, $\overline{\Delta(F_{a,c,1910-20})} \times D_{1920}$.

Similarly, in the modified equation (8), we interact the average transition into homeownership among newly formed households for each age group between 1900 and 1910 with an indicator for census year 1910, $\overline{\Delta(T_{a,c,1900-10})} \times D_{1910}$, and the corresponding average transition between 1910 and 1920 with an indicator for census year 1920, $\overline{\Delta(T_{a,c,1910-20})} \times D_{1920}$. The results from these regressions are reported in Table 2.9.

We observe that in the household formation specifications, the coefficient on immigration declines once we account for pre-trends and becomes statistically different from the baseline estimate of 0.766. In addition, the coefficients on the two interaction terms are large in magnitude and statistically significant. Taken together, these results suggest that the baseline estimates are partly capturing persistent city-level factors correlated with both household formation and immigration. This evidence raises concerns about the causal interpretation of the estimated effects on household formation. Therefore, the results from this specification should be interpreted as reflecting an association rather than a causal effect.

We also observe a decline in the coefficient on immigration in the homeownership transition specifications, although the estimate that accounts for pre-trends (1.599) is not statistically different from the baseline estimate (1.733). In addition, the pre-trend in homeownership transitions prior to 1910 is not statistically significant, whereas the corresponding pre-trend for the period prior to 1920 is statistically significant. Overall, this suggests that the estimated effect of immigration on transitions into homeownership is less sensitive to the inclusion of preexisting trends and is therefore more consistent with a causal interpretation.

2.6 Conclusion

This essay examines how immigration during the early 20th century affected native homeownership outcomes in the United States. Using linked census data from the IPUMS MLP and a shift-share instrument for city-level immigrant inflows, we study how changes in immigrant populations influenced native homeownership rates and their transitions into and out of homeownership. We then investigate the mechanisms through which immigration may have affected these housing outcomes. Finally, we examine how immigration influenced household formation among natives and whether newly formed native households became more likely to enter homeownership following increases in immigration.

Overall, our findings suggest that immigration during this period increased native homeownership in the United States. We find that cities experiencing larger increases in the share of recent immigrants also experienced higher native homeownership rates, greater transitions

of natives into homeownership, and lower transitions out of homeownership, even after controlling for age and state-by-year fixed effects. We further show that immigration reduced city-level home prices, suggesting that changes in housing affordability is an important channel through which immigration affected native housing outcomes. In addition, immigration increased the likelihood that individuals who were not household heads in one census became household heads in the following census, indicating that immigration encouraged native household formation. Finally, we find that newly formed native households became more likely to enter homeownership in response to increases in immigration. Taken together, these results suggest that immigration played an important role in shaping housing transitions and household formation among natives during the study period.

Our identifying assumption for the shift-share IV analysis is that cities receiving higher predicted immigration were not already on different underlying housing market trajectories because of persistent city-specific factors. This assumption may be violated for two reasons. First, persistent characteristics of cities may have both influenced later immigration flows and independently affected native housing outcomes, causing immigration and housing market trends to be jointly driven by underlying city-specific factors. Second, immigrants from particular birthplaces may have continued moving to certain cities because of long-standing economic, social, or cultural ties unrelated to historical settlement patterns alone. To address the first concern, we control for preexisting trends in homeownership transitions across cities. To address the second concern, we construct an alternative instrument using immigration variation generated by WWI and the immigration quota laws of the 1920s.

Including pre-trends reduces the coefficient on transitions into homeownership, suggesting that the IV may have partly captured persistent city-specific factors that jointly influence immigration and native transitions into homeownership. This evidence raises concerns about the causal interpretation of the estimated effects on transitions into homeownership. For transitions out of homeownership, the coefficient declines slightly in magnitude but is not statistically different from the baseline, making the results more consistent with a causal interpretation. Finally, the baseline results remain essentially unchanged when re-estimating the model using an alternative instrument based on immigration variation generated by

WWI and the immigration quota laws. This suggests that the findings are not driven by persistent city characteristics that attract immigrants from particular birthplaces and may simultaneously shape native housing market outcomes, but are instead robust to identifying variation coming from large exogenous geopolitical and policy shocks.

The main results suggest that immigration increases native homeownership. This raises the possibility that immigration may also affect household formation, defined as the likelihood that young natives aged 15 to 25 who are initially observed as non-household heads in their first observed census transition into household headship in the subsequent decade. We examine this relationship using a 2SLS approach. In addition, we analyze whether immigration affects the likelihood that these newly formed households transition into homeownership, using the same empirical framework.

We find a consistent positive and statistically significant relationship between immigration and household formation, as well as between immigration and subsequent transitions into homeownership among newly formed households. However, the pre-trend analysis indicates that the estimated effects on household formation partly reflect persistent city characteristics that influence both household formation and immigration, suggesting caution in interpreting these results as causal. In contrast, the pre-trend analysis for homeownership transitions among newly formed households is more supportive of a causal interpretation.

Chapter 3

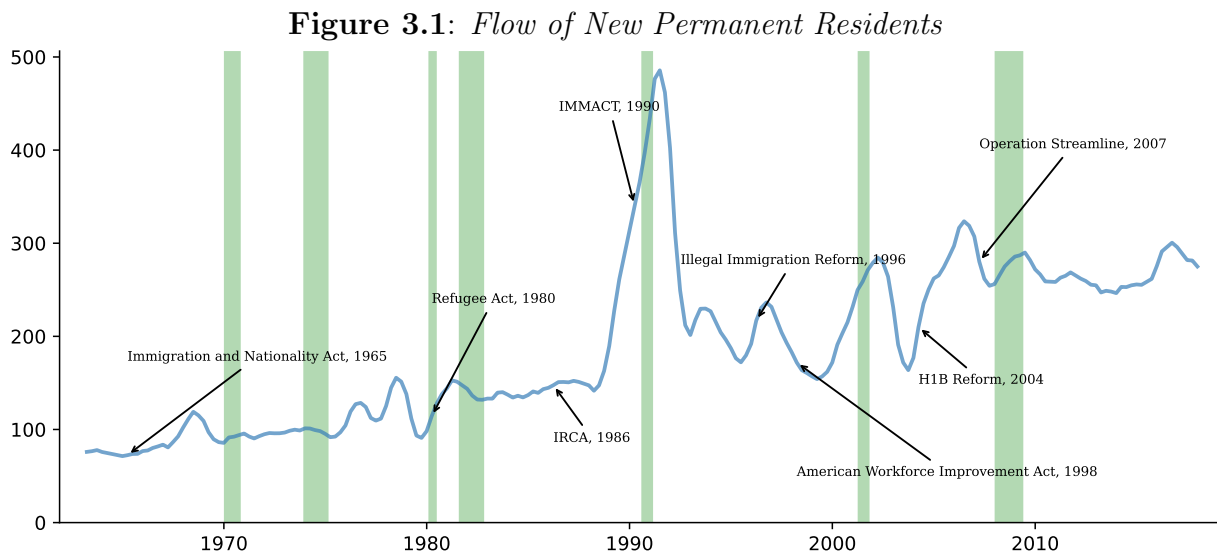
Immigration and U.S. Housing Markets: The 1986 Legal Status Shock

3.1 Introduction

Immigration inflows to the United States rose substantially after the Immigration and Nationality Act of 1965, with a sharp increase occurring after the enactment of the Immigration Reform and Control Act (IRCA) in 1986 (Figure [3.1](#)). This major policy intervention led to a sharp increase in immigration, as measured by the flow of new permanent residents. IRCA established a legalization pathway granting permanent residency, and eventual eligibility for naturalization, to undocumented immigrants who had entered the United States before 1982. Under this program, approximately 1.7 million unauthorized immigrants obtained legal status through the “general” legalization program, with an additional 1.3 million gaining status through the “Special Agricultural Workers” program ([Rytina and Simanski, 2009](#)).

Legal status is an important determinant of the economic opportunities available to immigrants. Immigrants lacking legal status face severe economic constraints, including exposure to exploitation in the labor market, where they often receive wages below their

skill levels and are concentrated in low-wage occupations to reduce the risk of deportation (Kossoudji and Cobb-Clark, 2002). In addition to labor market frictions, legalization may also ease constraints in credit markets and mobility, which can influence housing demand (Wang et al., 2022). Consequently, legalization may facilitate upward socioeconomic mobility among immigrants, with potentially important implications for housing demand and housing market outcomes.



Notes: The quarterly immigration series used in this figure is obtained from Li (2023), who constructs it by interpolating annual data on inflow of new permanent residents into the United States from the Yearbook of Immigration Statistics. The sample covers 1963Q1–2017Q4. Immigration policy reform dates are indicated by arrows, and shaded areas denote NBER recession periods. The y-axis reports immigration inflows measured in thousands of persons.

Housing is central to economic welfare, as both natives and immigrants spend a large share of income on housing, and housing wealth constitutes a major component of household wealth (Saiz, 2010). There are several reasons to expect legalization to affect housing markets. First, legal status relaxes constraints in rental markets by enabling access to formal housing arrangements that are often unavailable to undocumented immigrants due to difficulties verifying identity or entering into lease contracts. By expanding access to formal rental housing, legalization may increase demand for rental units and place upward pressure on rents. These effects are likely to materialize in the short run, as obtaining access to formal rental housing is often more immediate than transitioning into homeownership. Second, legal

status relaxes constraints in credit markets by enabling identification and legal recognition that allow households to access mortgages and other financial services. Improved access to credit can facilitate home purchases among immigrants with sufficient savings and, more broadly, increase borrowing capacity, thereby raising housing demand and house prices. These effects are likely to materialize more gradually, as accumulating savings, establishing credit histories, and qualifying for mortgage financing typically take time. Given the large scale of the IRCA, which legalized approximately three million undocumented immigrants, these mechanisms imply potentially meaningful aggregate effects on housing demand and prices, making IRCA a relevant setting for studying housing market responses to immigration policy.

To examine the effects of legal status, a large literature has treated the IRCA legalization program as a natural experiment. Decennial Census data, which record immigrants' year of arrival in the United States, allow researchers to compare immigrants eligible for legalization with otherwise similar immigrants who were ineligible. This literature has primarily focused on labor market outcomes, with mixed evidence on the effects of legalization: some studies find improvements in wages and employment for eligible individuals ([Amuedo-Dorantes et al., 2007](#); [Kossoudji and Cobb-Clark, 2000](#); [Rivera-Batiz, 1999](#)), while others find neutral or even adverse effects for undocumented immigrants ([Davila et al., 1998](#); [Donato et al., 1992](#); [Phillips and Massey, 1999](#)).

Compared to extensive literature on labor market outcomes, relatively little is known about how IRCA affected housing market outcomes. To our knowledge, only one recent study, by ([Sharpe, 2020](#)), examines the impact of IRCA on homeownership among beneficiaries relative to similar individuals, using decennial Census data and exploiting the sharp eligibility cutoff. However, this evidence is limited to homeownership outcomes for a specific group and does not capture broader housing market effects. No other study examines how IRCA affected rental housing demand, rents, home purchase demand, house prices, or supply-side responses.

In this essay, we fill this gap by analyzing the effects of IRCA on multiple dimensions of the housing market. We study rental housing demand using rental vacancy rates and assess

whether changes in demand translate into higher rents using CPI rent index (rent of primary residence). We also examine effects on the owner-occupied market using homeowner vacancy rates and the number of single-family houses sold, and we analyze house prices using both housing price indices and median prices of houses sold. Finally, we assess potential supply-side responses using building permit applications and housing starts. For the empirical analysis, we construct a quarterly dataset of twelve time series covering immigration flows and these housing market outcomes. The quarterly immigration series is obtained from (Li, 2023), while all housing-related series are drawn from the FRED database.

From the twelve series, we construct two alternative eight-variable specifications for our analysis. The baseline specification includes immigration, rental vacancy rates, real rent (CPI rent index), homeowner vacancy rates, the real housing price index, new privately-owned housing units authorized by building permits, new housing starts, and mortgage rates. All variables, except mortgage rates, are measured in quarter-on-quarter growth rates. Mortgage rates are included to control for broader macroeconomic conditions that may affect both housing demand and construction decisions. As a robustness check, we estimate an alternative specification in which homeowner vacancy rates and the real housing price index are replaced with the number of new single-family houses sold and the real median sale price of houses sold, respectively. This allows us to assess whether the estimated responses are robust to alternative indicators of the owner-occupied housing market.

To identify the causal effects of legalization-induced immigration shocks, we employ a Bayesian Structural Vector Autoregression (BSVAR) framework. Identification is achieved by combining traditional sign restrictions with narrative sign restrictions that incorporate historical information on the timing and implementation of IRCA. This approach allows us to isolate exogenous variation in immigration flows associated with the policy shock. The analysis focuses on tracing the dynamic responses of rental and owner-occupied housing markets, as well as housing supply, at the national level.

For identification, we first impose sign restrictions on the impact responses to the legalization shock. In the baseline specification, immigration, rents, and housing prices are restricted to respond positively on impact, while rental and homeowner vacancy rates are

restricted to respond negatively. All remaining variables are left unrestricted. In the alternative specification, we impose positive impact responses on the number of new single-family houses sold and the median sale price of houses sold. These restrictions are motivated by standard housing market mechanisms: given that the housing stock is effectively fixed in the short run, an increase in immigration raises housing demand on impact, leading to higher rents and house prices, while reducing vacancy rates as existing housing units become more fully utilized.

It is well known in the SVAR literature that sign restrictions alone are not sufficient to uniquely identify structural shocks. This is because they leave multiple admissible structural models consistent with the imposed restrictions. To further narrow the identified set, we augment the sign restrictions with narrative restrictions.

We use information on the implementation timeline of IRCA. Under the legislation, eligible individuals were required to apply for legalization within a one-year window between May 1987 and May 1988 ([Library of Congress](#)). As a result, immigration inflows associated with legalization began to rise substantially from the third quarter of 1988 onward. Our narrative restrictions are motivated by this timing. Specifically, we impose two restrictions: first, the legalization shock must have a positive effect on immigration in 1988Q3; and second, the contribution of the legalization shock to immigration in that quarter must be larger in absolute value than that of any other structural shock.

Our results show that the legalization shock associated with IRCA generated a persistent increase in immigration and substantial demand pressures across both rental and owner-occupied housing markets. Immigration growth rises by approximately 0.94 percentage points on impact and continues to increase over the following year before stabilizing at elevated levels. In rental markets, the adjustment occurs primarily through quantities rather than prices. Rental vacancy growth declines by approximately 0.92 percentage points on impact and remains persistently lower over the horizon, while real rent growth increases by about 0.17 percentage points initially and peaks at approximately 0.29 percentage points after six quarters. Relative to immigration, rental vacancies respond almost one-for-one on impact, whereas rents respond much more modestly, suggesting that increased housing de-

mand is absorbed primarily through more intensive use of the existing rental housing stock rather than through large immediate increases in rental prices.

The owner-occupied housing market exhibits even stronger responses to the legalization shock. In the baseline specification, home vacancy growth declines by approximately 1.58 percentage points on impact, while the quarterly growth rate of the housing price index rises by about 0.31 percentage points on impact and continues to increase over time. In the alternative specification, the quarterly growth rate of new single-family home sales increases by about 2.26 percentage points on impact and by 3.09 percentage points one year after the shock, while the growth rate of median house prices increases by approximately 0.78 percentage points on impact and by 1.13 percentage points after one year.

Overall, these results indicate that legalization-induced immigration generates strong and persistent demand pressures in owner-occupied housing markets along both quantity and price margins. In contrast, we find little evidence of a meaningful supply-side response, as neither housing permits nor housing starts exhibit statistically robust increases following the shock.

One possible explanation for the stronger response in the owner-occupied market relative to the rental market is that IRCA granted legal status to individuals who had already been residing in the United States prior to 1982 and, by the time of implementation in 1988, had spent at least six years in the country. This extended period of residence likely allowed them to develop stronger labor market attachment and accumulate savings. Moreover, the passage of IRCA in 1986 may have generated expectations of eventual legalization, potentially encouraging precautionary saving and facilitating entry into owner-occupied housing.

Compared to existing studies, our results suggest a weaker transmission from immigration to housing prices than is typically found in the literature. Cross-sectional metropolitan-level studies, such as (Saiz, 2007) and (Mussa et al., 2017), generally report elasticities of rents and housing prices with respect to immigration close to or just below unity. In our setting, the relative response of rents is substantially smaller, with rents increasing by about 0.17 percentage points on impact compared to a 0.94 percentage point increase in immigration growth, while housing prices rise by between 0.31 and 0.78 percentage points depending on

the measure. Even at their peak, the relative responses of housing prices remain below one, suggesting that immigration pressures are transmitted more weakly into prices in this setting than in many cross-sectional studies.

A plausible explanation for our weaker effect of immigration on housing rents and home prices compared to the existing literature is that housing suppliers may have formed expectations about future demand before legalization took effect. If news reports provided information on the likely size and geographic distribution of the population eligible for legalization, developers and homebuilders could have anticipated higher future housing demand and adjusted construction plans in advance. In this case, part of the supply response would have occurred prior to the legalization shock identified in our analysis, so that when legalization took effect, much of the adjustment was already reflected in the housing stock. This timing would mechanically reduce measured contemporaneous supply responses and help explain why adjustments appear primarily along the quantity margin in both rental and owner-occupied markets and price adjustment remained slow. While this is only one possible explanation, it is consistent with the relatively modest supply responses observed in the data.

The essay is organized as follows. Section 3.2 reviews the literature on the effects of legalization on labor and housing markets. Section 3.3 describes the data employed in the analysis, while Section 3.4 presents the model. Section 3.5 explains the identification restrictions, Section 3.6 presents the results, and Section 3.7 concludes.

3.2 Literature Review

Several studies examine the relationship between immigrants' legal status and housing market outcomes and provide consistent evidence that legal status shapes housing attainment. For instance, [Hall and Greenman \(2013\)](#) show that undocumented immigrants are significantly less likely to be homeowners and experience poorer housing quality and neighborhood conditions than documented immigrants, using imputed legal status from Survey of Income and Program Participation data. [McConnell \(2015\)](#) similarly finds that unauthorized immi-

grants are the least likely to own homes, based on evidence from the Los Angeles Family and Neighborhood Survey, and highlights the importance of legal status in structuring housing inequality among Latinos. Using the New Immigrant Survey, [McConnell and Akresh \(2008\)](#) document substantial differences in housing tenure among recently legalized immigrants, emphasizing the role of prior unauthorized experience and financial integration. In contrast, [McConnell and Marcelli \(2007\)](#) find no statistically significant relationship between legal status and homeownership among Mexican immigrants in Los Angeles after accounting for economic and assimilation factors, suggesting that local housing and credit market conditions may mediate the effects of legal status. Together, these studies indicate that legal status is an important, though context dependent, determinant of immigrants' housing outcomes.

Apart from homeownership, [Allen \(2022\)](#) studied the relationship between immigrant legal status and housing affordability in the United States, focusing on whether unauthorized status is associated with a higher likelihood of housing cost burden. Employing logit regression models that control for a wide range of household and socioeconomic characteristics, he finds that unauthorized immigrant and mixed status renter households are significantly more likely to experience housing cost burden than lawfully present immigrant and native-born households. Among homeowners, unauthorized and mixed status households also face a higher probability of housing cost burden relative to native born households.

These findings motivate a closer examination of the literature on the impact of IRCA on immigrants' housing market outcomes. In the following subsection, we review key studies that examine how IRCA and two other legalization policies influenced probability of homeownership for immigrants who obtained legal status under these programs versus those who did not.

3.2.1 Existing Evidence on the Effects of U.S. Immigration Policies on Housing Markets

The literature on the effects of changes in U.S. immigration policy on housing outcomes is limited. To our knowledge, only a few studies explicitly examine how changes in legalization

status resulting from immigration policies influence housing outcomes. Importantly, these studies consider only one outcome: homeownership.

One study is closely related to our analysis in terms of the policy considered. While our essay evaluates the impact of the IRCA on housing prices and rents, [Sharpe \(2020\)](#) examines how IRCA affected the probability of homeownership among eligible and ineligible immigrants. From the two other studies [Wang et al. \(2022\)](#) study the effect of the Deferred Action for Childhood Arrivals (DACA) program on homeownership among eligible versus ineligible individuals, while [Antonoudi et al. \(2025\)](#) examine the impact of the 287(g) program and the Secure Communities program on homeownership probabilities. Each of these studies is discussed in greater detail in this sub-section.

[Sharpe \(2020\)](#) analyzes whether gaining legal permanent resident (LPR) status causally affects immigrant homeownership by exploiting the IRCA (1986) as a natural experiment. IRCA granted amnesty to undocumented immigrants who entered the United States before January 1, 1982, creating a sharp discontinuity in the probability of legal status by year of arrival. Using Census data and a local randomization regression discontinuity design, he estimates the causal impact of amnesty eligibility on homeownership among Mexican immigrants. The results show that amnesty eligibility increased homeownership rates by approximately 4 percentage points, an effect that is robust across alternative samples and falsification tests. Importantly, the estimated effect is homogeneous with respect to citizenship status, indicating that legal status itself, rather than subsequent naturalization, drives the increase in homeownership. Additionally he finds that these effects reflect changes in housing tenure choice rather than shifts in household formation, as co-residency increases are observed primarily among amnesty-ineligible immigrants.

[Wang et al. \(2022\)](#) examine the causal effect of legalization on housing tenure decisions in the United States using the 2012 DACA program. Although temporary, DACA provided deportation relief and work authorization to eligible undocumented immigrants, which may reduce labor market, credit market, and mobility frictions that influence housing demand. Using American Community Survey (ACS) microdata, they analyze housing tenure outcomes among likely undocumented immigrants, proxied primarily by Mexican non-citizens,

with robustness checks based on the residual method of [Borjas and Cassidy \(2019\)](#). Their identification strategy relies on a difference in differences design that exploits the age based eligibility cutoff for DACA, specifically being younger than 31 in 2012, and compares otherwise similar individuals just above and below the cutoff before and after the program was implemented. They find that DACA eligibility increased homeownership by approximately 7 percentage points, representing a 19% increase relative to the sample mean. Improved access to credit and a longer expected duration in the United States appear to be important contributing factors.

Similarly, [Antonoudi et al. \(2025\)](#) examine whether participation in the 287(g) program and the Secure Communities program affects the probability of homeownership among Hispanics, non-citizens, and United States-born residents. While prior research has extensively studied immigrant homeownership and the labor market effects of immigration policy, limited evidence exists on how immigration enforcement, rather than legalization or individual characteristics, influences long-term housing investments. Using nationally representative ACS microdata, they exploit spatial and temporal variation in the adoption of 287(g) agreements and the implementation of Secure Communities across local areas. The empirical strategy estimates the causal effect of exposure to enforcement on homeownership outcomes while accounting for demographic characteristics, economic conditions, length of residence in the United States, and behavioral factors such as English proficiency. The results show that 287(g) agreements are associated with large and statistically significant declines in homeownership, particularly among Hispanic populations. Homeownership probabilities decline by approximately 7.5 percentage points in high within county concentration Hispanic communities and by 3.6 to 5.6 percentage points among Hispanics more broadly. These effects are robust across alternative model specifications and definitions of exposure and suggest meaningful spillover effects beyond the intended targets of the policy. In contrast, Secure Communities shows smaller and less robust effects, which is consistent with its less aggressive enforcement design.

Compared to the limited evidence on how legal status affects housing outcomes, there is a substantial literature on the effects of legalization on immigrants' labor market outcomes.

For instance, [Borjas and Tienda \(1993\)](#), [Kaushal \(2006\)](#), and ([Orrenius et al., 2012](#)) study three different legalization episodes and find that obtaining legal status raises immigrants' wages, earnings, and labor force participation. In the following sub-section, we briefly review key contributions in this area, particularly focusing on the impact of IRCA on immigrants' labor market outcomes.

3.2.2 Impact of IRCA on Labor Market Outcomes of Immigrants

A substantial body of research also finds that legalization under the IRCA led to significant improvements in labor market outcomes for its beneficiaries. Surveys conducted by Westat, Inc. for the United States Department of Labor show that the real hourly wages of immigrants who obtained legal status through IRCA increased substantially in the years following legalization, such that by 1992, four to five years after legalization, real hourly wages had risen by an average of 15.1% ([Hinojosa-Ojeda, 2012](#)). Wage gains were larger for women than for men, with women experiencing an average increase of 20.5% and men a 13.2% increase ([Massey et al., 2002](#)).

Several studies attribute these wage gains directly to the acquisition of legal status. [Rivera-Batiz \(1999\)](#) estimates that by 1992 legal status alone increased wages by 8.4% for male IRCA beneficiaries and by 13% for female beneficiaries, independent of changes in education, English proficiency, or other productivity related factors. Using the same survey data, [Kossoudji and Cobb-Clark \(2000\)](#) find that legalization facilitated occupational mobility, with 38.8% of Mexican men moving into higher paying occupations by 1992. [Lozano and Sorensen \(2011\)](#) also show that obtaining legal status under IRCA increased earnings by approximately 20-log points and suggest that most of the earnings gains operated through access to higher paying occupations rather than wage increases within occupations. Other studies confirm these positive wage effects while accounting for broader economic conditions. [Amuedo-Dorantes et al. \(2007\)](#) estimate that real hourly wages increased by 9.3% for male IRCA beneficiaries and by 2.1% for female beneficiaries by 1992, independent of economy wide wage trends. Similarly, [Kossoudji and Cobb-Clark \(2002\)](#) find that legalization raised

the wages of male IRCA beneficiaries by approximately 6%, even after controlling for changes in the United States and California labor markets.

[Mukhopadhyay \(2018\)](#) compares wage gains among immigrants who obtained legal status through IRCA with those who legalized through other channels. He uses data from the New Immigrant Survey for immigrants legalized through family ties or smaller legalization programs and data from the Legalized Population Survey for immigrants legalized under IRCA. He shows that wage increases following legalization are about 22% larger for male immigrants who legalized through family based or smaller programs than for IRCA beneficiaries. He demonstrates that the estimates from difference in difference in difference regressions using comparison groups from the National Longitudinal Survey of Youth, including the 1979 and 1997 cohorts, yield similar results. A substantial portion of this difference is attributed to a larger wage penalty associated with undocumented status following the employer sanctions introduced by IRCA. The remaining difference may be explained by changes in the relative supply of documented and undocumented workers resulting from the large scale amnesty program.

Some studies also document unfavorable impact of IRCA on labor market outcomes of undocumented workers. For instance, [Davila et al. \(1998\)](#) find that IRCA reduced the job alternatives of undocumented appearing workers, leading to weaker labor market outcomes even in the absence of clear evidence of increased wage discrimination. Similarly, [Donato et al. \(1992\)](#) find that while wage rates did not show a clear structural break after IRCA, undocumented migrants experienced declines in hours worked and monthly income. The likelihood of working in agriculture and having taxes withheld also declined, indicating a shift in employment conditions and a deterioration in labor market outcomes for undocumented migrants, even as migration flows remained high. [Phillips and Massey \(1999\)](#) focusing specifically on wages find that IRCA did not increase discrimination against Mexican workers in general, but did appear to increase discrimination against undocumented migrants, with employers passing the costs and risks of unauthorized hiring onto the workers.

Finally, to the best of our knowledge, [Li \(2023\)](#) is the only study that uses time series econometric methods to examine the causal effects of IRCA and other immigration policy

changes on the U.S. macroeconomy. Using quarterly data from 1953 to 2017, he estimates a structural vector autoregression (SVAR) which includes immigration, labor productivity, hours worked, consumption, and the term spread, and identifies structural shocks using a combination of short run and long run restrictions. He shows that positive immigration supply shocks raise labor productivity and hours worked at business cycle horizons and in the long run, while macroeconomic shocks such as productivity and news shocks also influence immigration flows. Importantly, the estimated impulse responses vary across immigration policy regimes, with IRCA and the Immigration Act of 1990 playing a key role in shaping these dynamics.

Overall, the existing literature reveals a clear gap. To our knowledge, no study examines how IRCA affected housing market prices and rents at the national level. Research that considers housing market outcomes in response to IRCA focuses exclusively on homeownership, while studies that use time series methods to analyze IRCA concentrate on its macroeconomic effects rather than housing markets. This study addresses this gap by analyzing the impact of IRCA on housing rents and housing prices using a structural VAR framework and narrative sign restrictions to identify the effects of IRCA on the housing market.

3.3 Data

The dataset consists of eleven variables. One variable measures immigration, capturing the inflow of people into the country. Of the remaining ten variables, nine describe the housing sector. In addition, a general CPI series is included and used to adjust all price variables for inflation.

For immigration, the only available time series is the annual inflow of new permanent residents into the United States, published by the Department of Homeland Security (DHS) in its [Yearbook of Immigration Statistics](#). Because the data are reported annually, interpolation is required to obtain a higher frequency. [Li \(2023\)](#), using a regression-based interpolation method described by [Silva and Cardoso \(2001\)](#), derives a quarterly series for immigration. Upon request, he provides this constructed series. The resulting dataset spans the period

from 1948Q1 to 2017Q4.

All housing series are obtained from the FRED database maintained by the Federal Reserve Bank of Saint Louis. Among these, three represent housing demand, three reflect price levels in the rental and owner-occupied markets, two capture construction activity and supply and one variable captures economy-wide credit conditions.

The three series capturing housing demand include rental vacancy rates, homeowner vacancy rates, and the number of one-family houses sold. The Rental Vacancy Rate ([RRVRUSQ156N](#)) captures the share of rental units that are vacant and available for rent, while the Homeowner Vacancy Rate ([RHVRUSQ156N](#)) measures the share of homeowner units that are vacant and available for sale. Both series begin in 1956Q1. We also use the New One Family Houses Sold: United States ([HSN1F](#)) series, which records the number of newly constructed single-family homes sold. This is a monthly series beginning in January 1963, which we convert to quarterly frequency by taking quarterly averages. We include these measures because sudden changes in the population eligible to rent or own housing may affect the utilization of both existing vacant units and the absorption of newly constructed homes in the rental and owner-occupied markets.

Because housing construction is fixed in the short run, an increase in immigration is expected to shift demand for both rental and owner-occupied housing to the right, thereby putting upward pressure on housing prices. To measure price responses in rental markets, we use the monthly series Consumer Price Index for All Urban Consumers: Rent of Primary Residence in U.S. City Average ([CUUR0000SEHA](#)), available from December 1914. This series is converted to quarterly frequency by taking quarterly averages. For owner-occupied housing, we use two series: the quarterly All Transactions House Price Index for the United States ([USSTHPI](#)), which begins in 1975Q1, and the quarterly Median Sales Price of Houses Sold for the United States ([MSPUS](#)), which begins in 1963Q1. All nominal rent and house price series are converted to real terms using the Consumer Price Index for All Urban Consumers: All Items in U.S. City Average ([CPIAUCNS](#)), which is available monthly from January 1913. This series is also converted to quarterly frequency by taking quarterly averages.

Higher housing prices may signal developers to invest in the housing sector. To assess whether this price increase is sufficient to stimulate supply over time, we use two measures: New Privately Owned Housing Units Authorized in Permit Issuing Places ([PERMIT](#)) and New Privately Owned Housing Units Started ([HOUST](#)). The permits series begins in January 1960 and reflects the number of new housing units for which building permits have been issued. The housing starts series begins in January 1959. Both series are available at a monthly frequency and are converted to quarterly frequency by taking quarterly averages. The permits series provides an early indication of developers' response to changes in housing demand, capturing the time required to recognize new demand and initiate investment, while the housing starts series measures the number of units on which construction has actually begun.

To account for financing conditions, we utilize mortgage rates using the weekly series 30 Year Fixed Rate Mortgage Average in the United States ([MORTGAGE30US](#)), which begins in April 1971. We convert this series to quarterly frequency by taking quarterly averages. We treat mortgage rates as exogenous in this context because they are mainly influenced by broader financial conditions, including international capital markets and monetary policy. Although they are not expected to respond directly to immigration, they are included to capture the effects of monetary policy and overall economic conditions that may influence housing supply and coincide with immigration related changes.

To ensure consistency across all series, the data was restricted to the period from 1975 Q1 to 2017 Q4. All nominal price indices were deflated to constant 2017 Q4 dollars. Finally, we express all series, except mortgage rates, in quarter-on-quarter percentage changes to make the results easier to interpret. This yields a final sample of 171 quarters, spanning 1975 Q2 to 2017 Q4.

3.4 The Model

We borrow the methodology from [Antolín-Díaz and Rubio-Ramírez \(2018\)](#). We employ a Structural Vector Autoregression (VAR) model for our analysis, defined by the following

mathematical expression:

$$x_t' D_0 = \sum_{i=1}^p x_{t-i}' D_i + c + \varepsilon_t' \quad (1)$$

The vector x_t is an 8×1 vector. The matrix D_0 is an 8×8 coefficient matrix capturing contemporaneous relationships, where each element represents the contemporaneous effect of one variable on another. The coefficient matrices D_i contain the lagged effects of the five lags¹ of each of the eight variables across the eight equations. The vector c is an 8×1 vector of constants, and ε_t denotes an 8×1 vector of structural error terms.

For convenience, this can be rewritten in compact form as:

$$x_t' D_0 = z_t' \Pi + \varepsilon_t' \quad (2)$$

where Π is an $m \times n$ matrix containing the lagged coefficients and constants of the model, defined as

$$\Pi = [D_1', \dots, D_p', c'] ,$$

with n denoting the number of equations in the system. The vector z_t' is an $m \times 1$ vector containing p lags of the endogenous variables and a constant term, given by

$$z_t' = [x_{t-1}', \dots, x_{t-p}', 1] ,$$

where $m = np + 1$. In our specification, the number of equations is $n = 8$, and the lag length is set to $p = 5$. Thus, in our study,

$$m = (8 \times 5) + 1 = 41.$$

¹The lag length is selected using the Akaike Information Criterion (AIC). We estimate the model under alternative maximum lag lengths and find that the selected lag order is sensitive to the admissible lag space, sometimes choosing interior values and in other cases selecting the upper bound. Given this lack of stability, we adopt a parsimonious specification that balances fit and degrees of freedom in a high-dimensional VAR setting.

The reduced-form representation of equation (2) is given by:

$$x'_t = z'_t B + u'_t \quad (3)$$

In this representation,

$$B = \Pi D_0^{-1},$$

$$u'_t = \varepsilon'_t D_0^{-1},$$

and

$$E[u_t u'_t] = E[D_0^{-1} \varepsilon_t \varepsilon'_t D_0^{-1}] = (D_0 D'_0)^{-1} = \Sigma,$$

since

$$E[\varepsilon_t \varepsilon'_t] = I_n.$$

The matrices B and Σ are reduced-form parameters, while D_0 and Π are structural parameters. In a similar manner, u'_t and ε'_t are reduced-form innovations and structural shocks, respectively.

We construct a baseline specification of the vector x_t to capture the main dimensions of the housing market while mitigating potential multicollinearity arising from highly correlated housing indicators. The specification includes the flow of new permanent residents (i_t), rental vacancy rates ($v_{r,t}$), real housing rent (r_t), home vacancy rates ($v_{h,t}$), the real housing price index ($p_{h,t}$), new privately-owned housing units authorized in permit-issuing places ($n_{2,t}$), new privately-owned housing units started ($n_{3,t}$), and mortgage rates (m_t). Together, these variables capture conditions in the rental market, the owner-occupied market, and the housing supply pipeline, while mortgage rates control for broader financing conditions that may affect housing demand and construction activity. All variables except mortgage rates are measured in quarter-on-quarter growth rates. The baseline specification of the vector x_t is

$$x'_{B,t} = \begin{bmatrix} i_t & v_{r,t} & r_t & v_{h,t} & p_{h,t} & n_{2,t} & n_{3,t} & m_t \end{bmatrix}$$

To assess whether the estimated housing market effects are consistent across alternative indicators, we estimate an alternative specification that replaces selected owner-occupied housing variables with different but related measures of housing market activity. In particular, we substitute home vacancy rates ($v_{h,t}$) and the real housing price index ($p_{h,t}$) with the number of new one-family houses sold ($n_{1,t}$) and the real median sale price of houses sold ($p_{m,t}$), respectively. This allows us to examine whether the responses obtained in the baseline specification reflect broader dynamics in the owner-occupied housing market. Consistent responses across these alternative measures would provide stronger evidence that legalization-induced immigration shocks affect housing markets more generally, rather than affecting only particular indicators. The alternative specification of x_t is defined as follows:

$$x'_{A,t} = \begin{bmatrix} i_t & v_{r,t} & r_t & n_{1,t} & p_{m,t} & n_{2,t} & n_{3,t} & m_t \end{bmatrix}$$

3.4.1 Impulse Response Functions

Given the estimates of the structural parameters $\Phi = (D_0, \Pi)$, the response of the i -th variable to the j -th structural shock at horizon k corresponds to the i -th row and j -th column of the matrix $\mathcal{I}_k(\Phi)$, which is defined recursively as

$$\mathcal{I}_0(\Phi) = (D_0^{-1})', \quad \text{for } k = 0$$

$$\mathcal{I}_k(\Phi) = \sum_{l=1}^k (D_l D_0^{-1})' \mathcal{I}_{k-l}(\Phi), \quad \text{for } 1 \leq k \leq p$$

$$\mathcal{I}_k(\Phi) = \sum_{l=1}^p (D_l D_0^{-1})' \mathcal{I}_{k-l}(\Phi), \quad \text{for } p < k < \infty$$

3.4.2 Historical Decomposition

The historical decomposition of structural shocks reveals the cumulative contribution of each shock to the observed unexpected change in a variable between two periods. Given the knowledge of structural parameters $\Phi = (D_0, \Pi)$, equation (2) can be rearranged to express the structural shocks as:

$$\varepsilon'_t = x'_t D_0 - z'_t \Pi \quad (4)$$

The contribution of the j -th shock to the observed unexpected change in the i -th variable between time periods t and $t + h$ is given by:

$$H_{i,j,t,t+h}(\Phi, \varepsilon_t, \dots, \varepsilon_{t+h}) = \sum_{l=0}^h e'_{i,n} \mathcal{I}_l(\Phi) e_{j,n} e'_{j,n} \varepsilon_{t+h-l}$$

where $e_{j,n}$ is the j -th column of I_n for $1 \leq i, j \leq n$ and $h \geq 0$.

3.4.3 How the Model Works Intuitively

In a time-series framework, economic variables typically exhibit persistence, meaning that their current values are partly influenced by their past values. This occurs because economic behavior and market conditions generally adjust gradually over time rather than changing instantaneously from one period to the next. As long as the underlying economic incentives and conditions remain relatively stable, individuals and firms tend to behave in similar ways across adjacent periods.

Consequently, the evolution of an economic variable over time depends both on its historical values and on unexpected changes that occur in the current period. These unexpected changes are commonly referred to as shocks or error terms. For example, the rental vacancy rate in 1980:Q4 is likely to depend partly on the vacancy rate observed in 1980:Q3 due to persistence in housing market conditions, while also reflecting any unexpected events or shocks that occurred during 1980:Q4.

Economic variables are also interconnected because the decisions made by households and

firms respond to changing economic conditions. As a result, aggregate time-series variables tend to move together over time. For example, changes in rental vacancy rates are closely related to changes in rents. A decline in available rental units may place upward pressure on rents, while higher rents may in turn affect housing demand and vacancy conditions. Consequently, the value of an economic variable in a given period depends not only on its own past values, but also on the past values of other related variables. A VAR model captures these dynamic interrelationships by allowing each variable in the system to depend on the lagged values of all other variables in the model.

More importantly, many economic variables are jointly determined within the same period. In housing markets, rents and vacancy rates are determined simultaneously because both respond contemporaneously to changes in housing demand and housing supply conditions. This simultaneity means that it is often difficult to separate cause and effect within a given quarter because the variables influence each other at the same time.

For example, suppose a large number of new immigrants enter a city during a particular quarter. The increased demand for housing may quickly reduce the number of vacant rental units available in the market. As vacant units become scarcer, landlords may respond by increasing rents. At the same time, higher rents may discourage some potential renters, induce households to share housing, or encourage some individuals to postpone moving. These responses can themselves influence vacancy rates within the same quarter. Thus, rents affect vacancy rates while vacancy rates also affect rents contemporaneously.

A structural VAR model (equation 1) is designed to capture these contemporaneous relationships. Instead of assuming that variables react only to past information, the model allows current values of economic variables to interact with one another within the same period. In our context, this means that variables such as rents, vacancy rates, housing prices, construction activity, and immigration flows are modeled as part of an interconnected system in which shocks to one part of the housing market can immediately influence other parts of the system.

The structural VAR framework allows us to disentangle these contemporaneous relationships in two steps. In the first step, we estimate what is known as the reduced-form VAR

(given in equation 3). This involves regressing each variable in the system on its own past values as well as the past values of all other variables included in the model. For example, current rents are estimated as a function of past rents, past vacancy rates, past immigration flows, past housing prices, and the past values of the remaining variables in the system. The same procedure is repeated for every variable in the VAR.

After accounting for all predictable movements based on past information, the remaining unexplained component in each equation is called the reduced-form residual. Intuitively, these residuals represent the unexpected movements in the variables during a particular period. However, these residuals do not yet correspond to economically meaningful shocks. This is because the variables in the system are simultaneously related to one another within the same period. As a result, a reduced-form residual generally reflects a combination of several underlying structural shocks occurring contemporaneously.

For example, an unexpected increase in rents during a quarter may partly reflect an immigration-driven demand shock, partly a housing supply shock, and partly changes in financing conditions such as mortgage rates. Since all of these forces can influence the housing market simultaneously, the reduced-form residuals alone cannot tell us which underlying economic shock is responsible for the observed movement in the data.

The second step of the structural VAR therefore focuses on identification. In this step, we impose economically motivated assumptions that allow us to separate, or identify, the underlying structural shocks from the combined reduced-form residuals. These assumptions help disentangle a “pure” immigration shock from other contemporaneous shocks affecting the housing market. In our study, this identification is achieved using sign restrictions and narrative sign restrictions, which impose economically and historically plausible patterns on the responses of the variables and on the timing of the legalization shock associated with IRCA.

Sign restrictions are imposed on what are known as impulse response functions (IRFs). An IRF measures how the economy is predicted to evolve after experiencing a one-time unexpected shock relative to a hypothetical scenario in which the shock never occurred. In this sense, an IRF is fundamentally a counterfactual exercise. The model first predicts

how the variables would have evolved in the absence of the shock and then compares that path to the predicted evolution after the shock occurs. The difference between these two predicted paths represents the effect of the shock itself. The IRF therefore traces how each variable responds over time because of the shock, holding constant the normal evolution of the economy implied by past data.

The idea behind sign restrictions is straightforward. The model generates many possible ways of separating the reduced-form residuals into underlying structural shocks. Some of these possible separations imply economically reasonable responses, while others imply responses that are inconsistent with basic economic intuition. The sign restrictions therefore act as a screening mechanism. We instruct the model to keep only those solutions in which the variables respond to the immigration shock in ways that are economically plausible. For instance, if a particular solution implied that a positive immigration shock immediately reduced immigration flows or lowered housing demand, that solution would be rejected because it contradicts the economic mechanism underlying the shock. In contrast, solutions that generate responses consistent with housing market theory are retained.

In addition to the sign restrictions on IRFs, we also use historical information related to IRCA to further sharpen the identification. To understand this idea, suppose immigration increased unexpectedly during a particular historical period. Many different factors could potentially explain that increase, such as changes in the economy, housing market conditions, labor demand, or immigration policy. The model therefore attempts to separate the observed movement in immigration into the portions explained by each underlying shock. This part of the analysis is called the historical decomposition because it breaks down an observed historical movement into the contributions coming from different shocks. Intuitively, it asks: when immigration moved unexpectedly during a certain period, which shock was mainly responsible for that movement? We use historical information about the timing of IRCA to guide the model in answering this question.

These restrictions help connect the statistical model to the actual historical event being studied. Rather than allowing the model to attribute the rise in immigration during the IRCA period to unrelated economic disturbances, the restrictions guide the model toward

interpretations that are consistent with the historical timing and nature of the policy.

3.5 Sign Restrictions and Identification of the Model

The structural VAR model in equation (2) is not identified and therefore requires some restrictions on the structural parameters for identification. Following [Antolín-Díaz and Rubio-Ramírez \(2018\)](#) we impose both conventional sign restrictions on impulse response functions and narrative sign restrictions on realized structural shocks and their implied historical contributions. The narrative restrictions further discipline the set of admissible structural parameterizations by requiring that, during key historical episodes, the inferred shocks and their contributions to observed variables are consistent with the established narrative.

3.5.1 Traditional Sign Restrictions

The traditional sign restrictions can be characterized by the following function:

$$\Gamma(\Phi) = (e'_{1,n}\mathcal{F}(\Phi)'S'_1, \dots, e'_{n,n}\mathcal{F}(\Phi)'S'_n) > 0 \quad (5)$$

Choosing S_j and $\mathcal{F}(\Phi)$ lead to the sign restrictions on IRFs or the structural parameters. To place sign restrictions on IRFs one must define $\mathcal{F}(\Phi)$ as vertically stacking IRFs at different horizons over which one intends to impose restrictions and the elements of S_j (i.e., an $s_j \times r_j$ matrix of zeros, ones and negative ones) will pick the horizons and the variables over which one wants to impose the r_j sign restrictions to identify the structural shock s_j . To impose restrictions on the structural parameters one must define $\mathcal{F}(\Phi) = \Phi$ and S_j (i.e., an $s_j \times r_j$ matrix of zeros, ones and negative ones) will pick the elements of Φ over which one wants to impose the sign restrictions.

Formally, letting $\mathcal{I}_k(\Phi)$ denote the 8×8 impulse response matrix at horizon k , we define

$$\mathcal{F}(\Phi) = \begin{bmatrix} \mathcal{I}_0(\Phi)' \\ \mathcal{I}_1(\Phi)' \\ \vdots \\ \mathcal{I}_K(\Phi)' \end{bmatrix}$$

where K is the maximum horizon over which restrictions are imposed. Each transpose is included to ensure that the columns of $\mathcal{F}(\Phi)$ correspond to variables and the rows correspond to restrictions, so that multiplication with S_j is dimensionally consistent.

In this essay, identifying restrictions are motivated by demand and supply dynamics in rental and owner-occupied housing markets. An increase in the inflow of permanent residents raises demand for rental housing units and owner-occupied housing, shifting demand curves outward and exerting upward pressure on prices in both markets in the short run. Accordingly, we impose sign restrictions on the impulse responses to the IRCA-induced immigration (legalization) shock.

We expect that, following an immigration shock, the flow of new permanent residents increases. Given that housing supply is inelastic in the short run, the adjustment is expected to occur primarily through demand. In the short run, vacant rental units are likely to be absorbed and vacant homes to be filled. Accordingly, rental and homeowner vacancies are expected to decline on impact, while the number of new one-family houses sold is expected to increase. As the existing stock of rental and owner-occupied housing becomes more fully utilized, upward pressure builds on housing prices, leading to increases in both real rents and real house prices.

Therefore, we restrict the contemporaneous response of the flow of new permanent residents, the number of new one-family houses sold, real housing rents, and the two housing price indicators in their respective specifications to be positive, while imposing negative impact responses for rental and homeowner vacancy rates. We leave the contemporaneous responses of the supply-side measures and mortgage rates unrestricted.

To allow the data to determine the effects with minimal restrictions, we impose sign

restrictions on the contemporaneous response (i.e., $k = 0$). This leads to the following sign pattern for the impulse response matrix in the baseline specification:

$$\mathcal{I}_0(\Phi) = \begin{bmatrix} +1 & * & * & * & * & * & * & * \\ -1 & * & * & * & * & * & * & * \\ +1 & * & * & * & * & * & * & * \\ -1 & * & * & * & * & * & * & * \\ +1 & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \end{bmatrix}$$

where the first column corresponds to the legalization shock. Rows one through five represent the quarter-on-quarter growth in the flow of new permanent immigrants, rental vacancy rates, real housing rents, home vacancy rates, and housing price index. The remaining elements are left unrestricted, as indicated by asterisks.

Imposing sign restrictions only on the contemporaneous response (i.e., $k = 0$) yields the following sign pattern for the impulse response matrix in the alternative specification, which replaces homeowner vacancy rates and the housing price index with the number of new one-family houses sold and the real median sales price of houses sold respectively.

$$\mathcal{I}_0(\Phi) = \begin{bmatrix} +1 & * & * & * & * & * & * & * \\ -1 & * & * & * & * & * & * & * \\ +1 & * & * & * & * & * & * & * \\ +1 & * & * & * & * & * & * & * \\ +1 & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \\ * & * & * & * & * & * & * & * \end{bmatrix}$$

Importantly, these restrictions are imposed only on the immediate, or contemporaneous, responses of the variables rather than on their entire future paths. This allows the data substantial freedom in determining how the effects evolve over time after the initial impact. In other words, the restrictions help identify the shock itself without tightly constraining the longer-run dynamics produced by the model.

Traditional sign restrictions alone are not sufficient to uniquely identify the effects of the IRCA-induced immigration shock. As is well known in the SVAR literature, sign restrictions typically deliver set rather than point identification. That is, multiple structural decompositions of the reduced-form VAR can satisfy the same set of sign restrictions, implying a range of admissible impulse responses rather than a unique response. Accordingly, in addition to the standard sign restrictions on the impulse responses, we impose narrative sign restrictions directly on the legalization-induced immigration shock. These narrative restrictions incorporate external information about historically identified episodes for which there is broad agreement regarding the direction of the shock, thereby sharpening identification and disciplining the set of admissible structural impulse responses.

3.5.2 Narrative Sign Restrictions - Restrictions on the Signs of Structural Shocks

We now consider the first class of narrative sign restrictions, which impose restrictions directly on the structural shocks during specific historical episodes. Suppose we want to require that the j -th structural shock is positive during s_j episodes occurring at dates $t_1, \dots, t_{s_j}$. Then the narrative sign restrictions can be expressed as:

$$e'_{j,n} \epsilon_{t_v}(\Phi) > 0, \quad 1 \leq v \leq s_j, \quad (6)$$

where $\epsilon_{t_v}(\Phi)$ denotes the vector of structural shocks at date t_v , and $e'_{j,n}$ is a selection vector that picks the j -th shock.

To impose this restriction, we rely on the timing of the realization of the impact of IRCA

(1986), which we define based on historical evidence. According to policy details from the Library of Congress, the legislation required eligible applicants, those who had continuously resided in the United States since 1982, to apply within a one-year window from May 1987 to May 1988. Applicants were also required to pay a fee and submit extensive documentation, including fingerprints, employment history, and proof of continuous residency. Accordingly, the effects of IRCA are expected to materialize only after the expiration of the application period in May 1988. We therefore define the start of the policy period as the third quarter of 1988.

Thus, we formally define our first narrative sign restriction as:

Narrative – Sign Restriction 1: The legalization shock must take a positive value in the third quarter of 1988.

3.5.3 Narrative Sign Restrictions - Restrictions on the Historical Decomposition

We now turn to the second class of narrative sign restrictions, which are imposed on the historical decomposition of the model. Historical narratives provide information not only about the sign of a shock, but also about its relative importance in driving unexpected movements in particular variables over specific periods.

[Antolín-Díaz and Rubio-Ramírez \(2018\)](#) introduce two types of restrictions. Type A restrictions require a given structural shock to be the largest (or smallest) contributor, in absolute value, to the unexpected change in a variable over a specified period relative to all other shocks. Type B restrictions require the contribution of a given shock to be larger (or smaller) in absolute value than the combined contributions of all remaining shocks. Since Type B restrictions are stronger than Type A restrictions, we employ only Type A restrictions in this study. Therefore, we formally define our second restriction as:

Narrative – Sign Restriction 2: For the period specified by Restriction 1, the legalization shock is the most important contributor to the observed unexpected movements in immigration. In other words, the absolute value of the contribution of the legalization

shock is larger than the absolute value of the contribution of any other structural shock to immigration.

We estimate the two specifications using the `bsvarSIGNs` package for R. We generate a total of 30,000 posterior draws, discard the first 20,000 as burn-in to reduce the impact of initial values, and use the remaining 10,000 draws for inference. We compute impulse responses over 12 horizons. For each horizon, we compute the median impulse response together with the 16th and 84th percentiles of the posterior distribution, which are used to plot the impulse response functions and construct 68% pointwise credible intervals.

3.6 Model Results

This section presents the dynamic responses of immigration and housing market outcomes to the legalization shock associated with IRCA (1986). The analysis groups variables into demand, price, and supply categories to better organize the response patterns across the housing market.

It is important to note that these findings should be interpreted in the specific context of IRCA and may not generalize to other immigrant groups. Unlike many other immigrant cohorts, individuals affected by IRCA had already spent several years living and working in the United States before becoming legally eligible to participate fully in formal rental and homeownership markets. As a result, the estimated responses reflect adjustment dynamics for a relatively established immigrant population rather than newly arrived migrants.

3.6.1 Immigration Responses to the Legalization Shock

Under the baseline and alternative specifications, the legalization shock generates a moderate and persistent increase in immigration. In both cases, immigration rises on impact, continues to increase over the following four quarters, and then stabilizes. The effect remains statistically significant for approximately six quarters after the shock.

At impact ($h = 0$), immigration growth rises by approximately 0.0094 under the baseline

specification. Since the immigration variable is defined as a growth rate, this corresponds to an increase of about 0.94 percentage points in the quarterly growth rate of immigration. Relative to the pre-shock standard deviation of quarterly immigration growth (0.0664), this implies an effect of roughly 0.14 standard deviations, indicating a relatively small initial response.

The response accumulates to approximately 3.42 percentage points one year after the shock ($h = 5$), corresponding to about 0.52 standard deviations in the medium run. This suggests a moderate and economically meaningful increase in immigration dynamics following the legalization shock.

In the long run, two years after the shock, the cumulative response stabilizes at approximately 2.45 percentage points, equivalent to about 0.37 standard deviations. This persistence suggests that legalization induced a lasting shift in immigration flows rather than a temporary surge.

3.6.2 Housing Demand Responses to Immigration Shocks

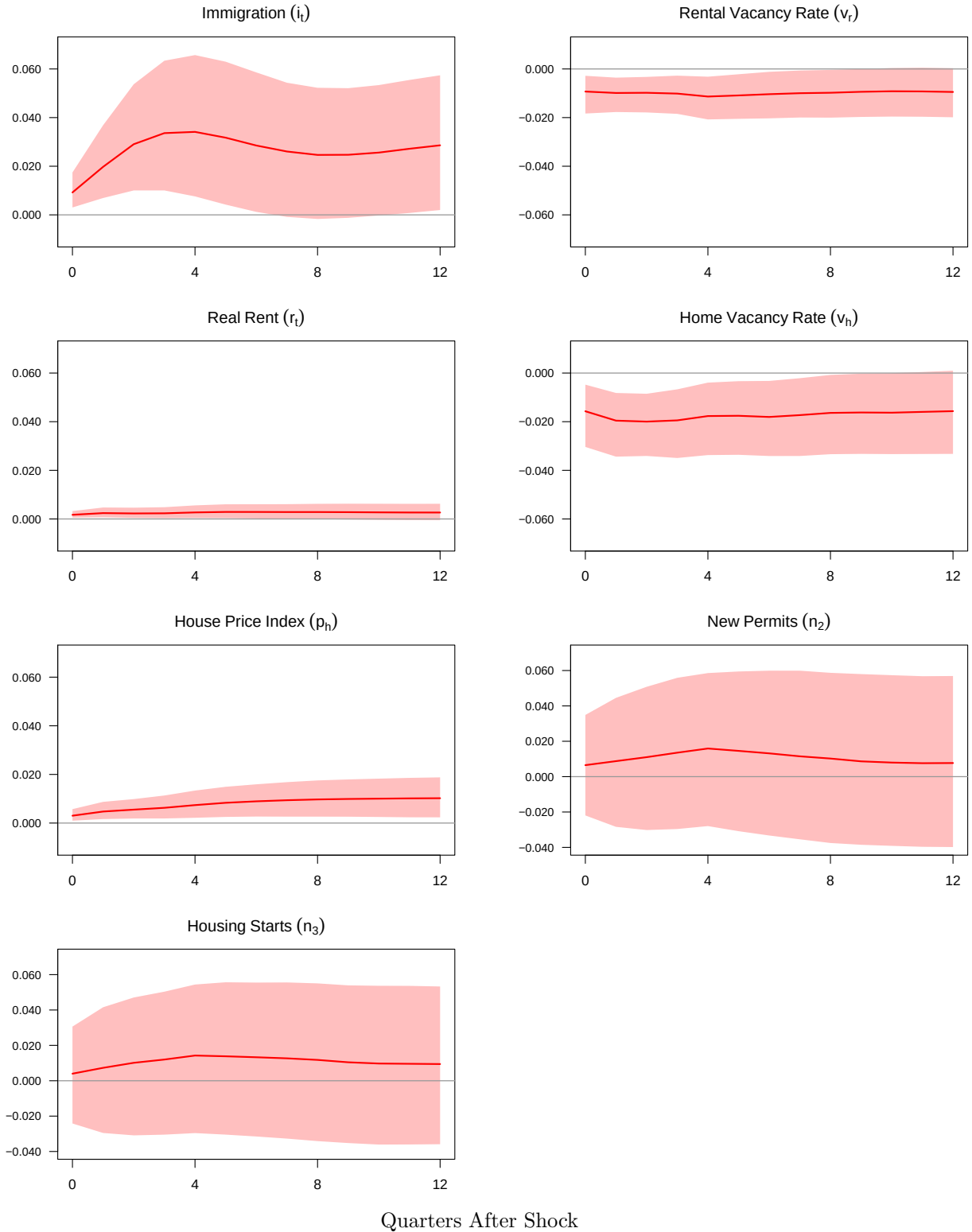
Figure 3.2 reports the cumulative responses of the flow of new permanent residents (i_t), rental vacancy rates ($v_{r,t}$), and homeowner vacancy rates ($v_{h,t}$). Figure 3.3 complements this analysis by replacing homeowner vacancy rates with the number of new single-family houses sold ($n_{1,t}$), allowing us to examine the response of an additional housing market indicator. We analyze the response of each series in turn.

Figure 3.2 shows that the legalization shock leads to an immediate decline in rental vacancy rates, which then remain relatively stable over the rest of the horizon. The response is statistically significant for approximately nine quarters following the shock.

In terms of magnitude, rental vacancy rates fall by approximately 0.0092 at impact ($h = 0$), corresponding to a 0.92 percentage point decline. Relative to the pre-IRCA standard deviation of quarterly changes in rental vacancy rates (0.0487), this implies an effect of about 0.19 standard deviations.

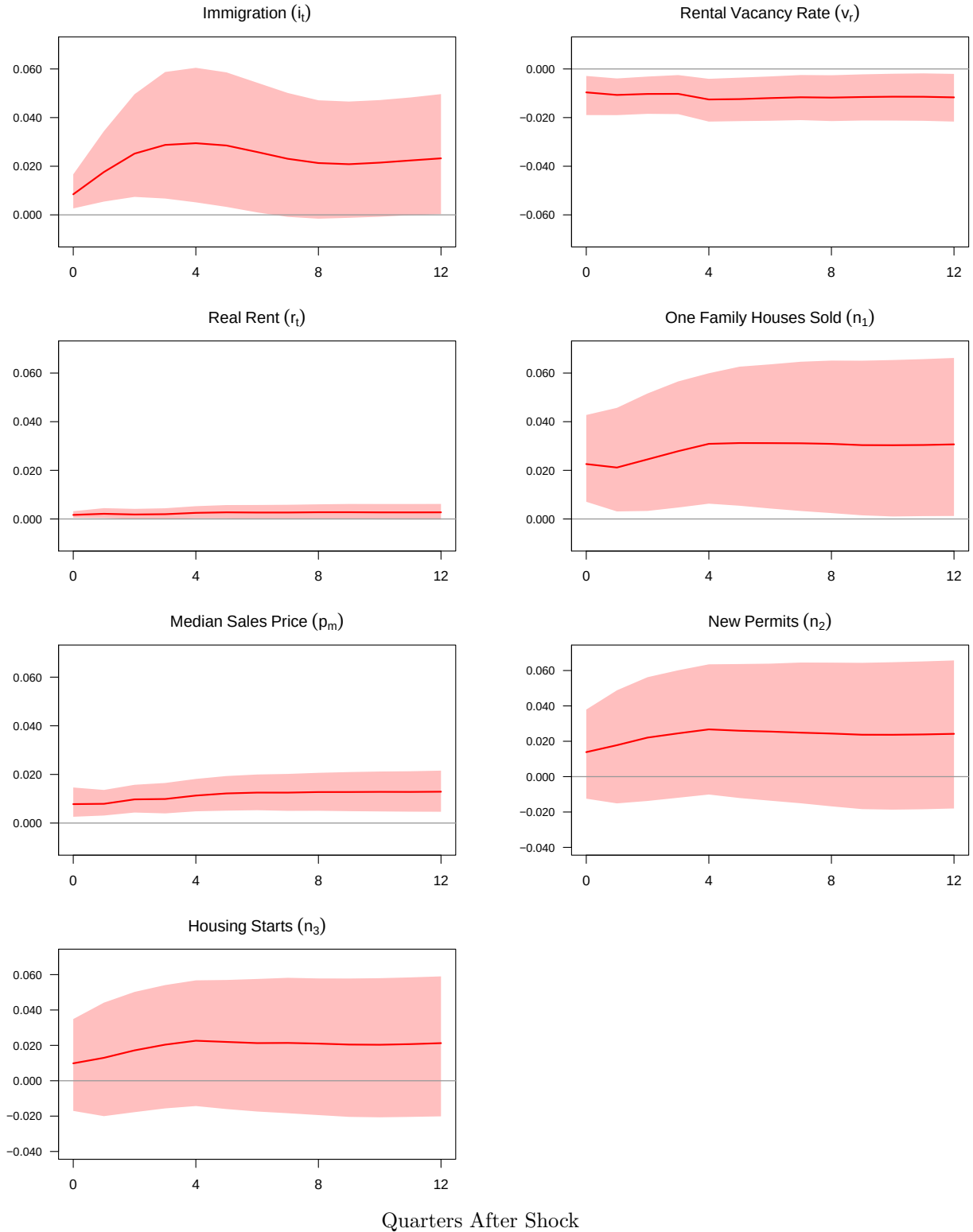
The decline in rental vacancy rates reaches its peak after approximately one year ($h = 5$),

Figure 3.2: Housing Market Responses (Baseline Specification)



Notes: The figure reports median cumulative impulse responses to an immigration shock identified using sign and narrative sign restrictions. Shaded regions represent 68% posterior credible intervals. The underlying variables are quarterly growth rates in levels. Cumulative responses can be interpreted as approximate percent deviations in the level of each variable relative to baseline. The horizontal axis measures quarters after the shock.

Figure 3.3: *Housing Market Responses (Alternative Specification)*



Notes: The figure reports median cumulative impulse responses to an immigration shock identified using sign and narrative sign restrictions. Shaded regions represent 68% posterior credible intervals. The underlying variables are quarterly growth rates in levels. Cumulative responses can be interpreted as approximate percent deviations in the level of each variable relative to baseline. The horizontal axis measures quarters after the shock.

with a reduction of about 1.14 percentage points. Relative to the pre-shock variation in vacancy rates, this corresponds to roughly 0.23 standard deviations, suggesting that the overall effect of the legalization shock is economically modest.

A more intuitive way to describe the relationship between immigration and rental vacancy rates is to focus directly on how strongly the two variables move relative to each other in response to the legalization shock.

At impact, the shock increases immigration by 0.94 percentage points in quarterly growth and reduces rental vacancy growth by 0.92 percentage points. This implies an almost one-for-one response (0.98) in growth rates, meaning that immigration and rental vacancy rates move in opposite directions with very similar magnitude in the short run. In other words, the immediate adjustment in the housing market closely mirrors the increase in immigration flows.

Over time, however, this relationship weakens. One year after the shock (medium run), the implied relative response is approximately 0.33, indicating that rental vacancy rates still decline in response to higher immigration, but the magnitude of the effect is substantially smaller than in the short run. Two years after the shock (long run), this attenuated relationship persists, with the relative response at approximately 0.41, suggesting continued but moderate adjustment in rental vacancy rates as the housing market gradually absorbs part of the initial pressure generated by increased immigration.

One possible explanation for this decline in responsiveness is adjustment through prices. As rents increase over time, remaining rental units become more expensive, which dampens further adjustments in vacancy rates.

Home vacancies exhibit a sharper response than rental vacancies, declining by 1.58 percentage points on impact and remaining broadly persistent at this lower level over the entire horizon. The response reaches its peak three quarters after the shock, with a decline of approximately 1.95 percentage points, and remains statistically significant for most of the horizon. Relative to the pre-shock standard deviation of quarterly home vacancy growth (7.53), the peak effect corresponds to roughly 0.26 standard deviations, suggesting a persistent but modest decline following the shock.

Relative to the impact of the shock on immigration (0.94 percentage points), home vacancy rates decline by a larger amount at impact, falling by 1.58 percentage points. This implies a ratio of 1.69, indicating that home vacancies respond more than one-for-one in the short run. Over time, however, this relative response attenuates, with the ratio declining to between 0.52 and 0.68 one to two years after the shock, exhibiting a pattern similar to rental vacancy rates.

A stronger impact of immigration on housing demand is reflected in the sharp increase in the quarterly growth rate of one-family home sales. On impact, it rises by 2.26 percentage points and continues to increase modestly over the next four quarters before stabilizing at around 3.09 percentage points one year after the shock. Relative to the pre-shock standard deviation of the series (10.93), this corresponds to an effect of about 0.21 standard deviations on impact, increasing and stabilizing at approximately 0.28 standard deviations in the medium run. Overall, these results indicate a persistent rise in housing demand following the shock.

This series shows a much stronger response to the shock relative to immigration compared to the vacancy rate measures. At impact, the ratio of the response in quarterly growth of one-family houses sold to the response in immigration growth is 2.40, indicating a more than two-for-one relationship and suggesting a strong immediate absorption of housing demand following the shock.

In the medium to long run, this ratio declines but remains close to unity, indicating that the demand for one-family housing remains closely tied to immigration even two years after the shock.

Again, one possible explanation for the declining relative responsiveness of both home vacancy rates and new single-family home sales is the gradual adjustment in housing prices, which may dampen the effect of continued immigration on housing market dynamics over time.

A plausible explanation for the stronger response in one-family home sales is that immigrants who benefited from IRCA had already been living and working in the United States for at least six years before receiving legal status. As a result, they were more attached to

the labor market and had more time to accumulate savings compared to recent arrivals in 1988.

Another important factor is news reporting around the proposed amnesty program. Media coverage of the legalization debate may have informed eligible immigrants that a general amnesty was likely, which could have encouraged precautionary saving in anticipation of improved access to formal housing and credit markets. For example, an immigrant who arrived in December 1981, just before the January 1982 cutoff, would have had several years between the passage of IRCA in 1986 and the end of the application period in 1988 to adjust their behavior and accumulate savings.

Once legal status was granted, these accumulated savings, together with improved access to mortgage markets, may have made entry into homeownership more feasible for some households. This would help explain why one-family home sales respond more strongly than rental outcomes. Overall, this pattern is consistent with the idea that legalization generated housing demand pressures that extended beyond the rental market into owner-occupied housing, although this is only one possible interpretation.

3.6.3 Housing Price Responses to Immigration Shocks

After establishing that the legalization shock increased immigration and strengthened demand for both rental and owner-occupied housing units, we now turn to quantifying its impact on rental and housing prices. Figure 3.2 presents the responses of quarterly growth in real housing rent (r_t) and the real housing price index ($p_{h,t}$), while Figure 3.3 reports the responses of quarterly growth in the real median sale price of houses sold ($p_{m,t}$).

Quarterly growth in real rents increases by 0.17 percentage points on impact and reaches a peak of 0.29 percentage points six quarters after the shock. Relative to the pre-shock standard deviation of quarterly rent growth (0.80 percentage points), this corresponds to about 0.22 standard deviations on impact. As the response builds over time, it rises to approximately 0.36 standard deviations at its peak, indicating a gradual and persistent upward pressure on rental prices following the legalization shock.

The response of real rent growth to immigration appears relatively modest when compared to the response of immigration itself following the legalization shock. At impact, immigration growth increases by 0.94 percentage points, while real rent growth rises by only 0.17 percentage points, implying a ratio of about 0.19. In other words, rents increase by roughly one-fifth as much as immigration in the short run. By contrast, rental vacancy rates respond much more strongly, moving almost one-for-one with immigration on impact.

As the effects of the shock accumulate over time, this relative response of rents declines further, falling to roughly 0.09 at the peak response of rents. These results suggest that the initial immigration-driven pressure in rental markets is absorbed more through adjustments in quantities, particularly lower vacancy rates, rather than through immediate increases in rental prices.

Next, we discuss the responses of prices in the owner-occupied housing market. Quarterly growth in real housing prices exhibits a modest increase of 0.31 percentage points on impact. However, the response strengthens considerably over the horizon, more than doubling to approximately 0.74 percentage points one year after the shock and continuing to increase gradually thereafter. Relative to the pre-IRCA standard deviation of quarterly growth in real housing prices (1.49 percentage points), these responses correspond to approximately 0.21 standard deviations on impact, rising to 0.49 standard deviations after one year and to about 0.65 standard deviations two years after the shock. Overall, these results indicate a persistent and economically meaningful increase in housing prices following the legalization shock.

Housing prices respond more strongly to immigration than housing rents, as reflected in the relative magnitudes of their responses to the legalization shock. At impact, the ratio of the response in housing price growth to the response in immigration growth is approximately 0.33, compared to only 0.19 for rents. This suggests that immigration-induced demand pressures are transmitted more rapidly into owner-occupied housing prices than into rental prices. In the medium run, the relative response of housing prices declines to around 0.22, but it increases again to approximately 0.40 in the long run, indicating that price pressures in the owner-occupied housing market become more persistent over time.

The response of quarterly growth in the real median sale price of houses sold is broadly similar to that of the housing price index, although it continues to accumulate for roughly six quarters following the shock before stabilizing. On impact, quarterly growth in the median sale price increases by 0.78 percentage points and rises further to approximately 1.13 percentage points over the following year, after which the response stabilizes. Relative to the pre-IRCA standard deviation of the series (2.57 percentage points), these responses correspond to approximately 0.30 standard deviations on impact and about 0.44 standard deviations one year after the shock, indicating a persistent increase in transaction-level housing prices following the legalization shock.

Relative to the increase in quarterly immigration growth, the response of quarterly growth in the median sale price of houses sold is substantial. At impact, the ratio of the response in median sale price growth to the response in immigration growth is approximately 0.83, indicating a strong immediate increase in transaction-level housing prices following the legalization shock. This large response is consistent with the sharp rise in demand for one-family housing units observed earlier. Although the relative response moderates over time, it remains elevated between approximately 0.33 and 0.52 during the one- to two-year period following the shock, suggesting persistent upward pressure on housing transaction prices as housing demand continues to adjust.

Overall, these results suggest that the rental market absorbs much of the immigration shock primarily through tighter vacancy conditions rather than large increases in rents, whereas the owner-occupied housing market exhibits broader and more persistent adjustments along both quantity and price dimensions. In particular, the legalization shock is associated with sustained declines in home vacancy rates, strong increases in one-family house sales, and persistent appreciation in housing prices, indicating substantial and long-lasting demand pressures in the owner-occupied segment of the housing market.

Existing studies using metropolitan level variation typically report housing market elasticities with respect to immigration that are close to one. Using variation across metropolitan areas, [Saiz \(2007\)](#) finds that an immigration inflow equal to one percent of a city's population increases average rents and housing prices by approximately one percent, implying an

elasticity of about one. Similarly, [Mussa et al. \(2017\)](#) estimate that a one percent increase in immigration relative to metropolitan area population raises rents by roughly 0.8%, corresponding to an elasticity of 0.8. However, these estimates are best interpreted as long run effects, as they are based on changes that unfold over several years rather than short run dynamics.

In comparison, although we do not formally estimate elasticities, the relative responses of rents and housing prices to immigration are substantially smaller than those implied by much of the existing literature. In the long run, the relative response of rents is about 0.09, while the corresponding response for housing prices ranges between 0.40 and 0.52 depending on the measure used. Our results therefore suggest a weaker relationship in this setting, with housing prices responding at most at about one-half the magnitude of immigration, and rents responding even less.

3.6.4 Housing Supply Responses to Immigration Shocks

A natural question is whether the observed increase in housing prices generates sufficient incentives for real estate investment and supply expansion. To address this, we examine the response of new privately owned housing units authorized in permit-issuing places ($n_{2,t}$) as well as new privately owned housing units started following the shock ($n_{3,t}$).

Both measures of housing supply show a very small increase at impact, followed by a slight rise over the next year, after which they stabilize. However, these responses are not statistically significant. Although the supply responses were left unrestricted, the estimated paths are consistent with expectations in the sense that they are non-negative and do not suggest a decline in supply following the shock.

Overall, the results provide little evidence of a meaningful supply response to the increase in prices. This is consistent with the idea that the price incentives generated by the shock are not large enough to trigger a measurable expansion in housing supply.

One important caveat is that anticipation may have affected not only immigrants but also housing suppliers. Just as eligible immigrants may have increased savings in anticipation

of legalization, developers and homebuilders may have anticipated higher future housing demand. For example, if news reports provided estimates of the number of immigrants likely to qualify for legalization, as well as information about where these populations were concentrated, builders may have used this information to anticipate future housing demand in local housing markets with larger eligible immigrant populations and adjust construction plans before legalization took effect.

If such anticipatory behavior occurred, part of the housing market adjustment would have taken place before the legalization shock identified in our analysis. By the time immigrants obtained legal status, some additional housing supply may already have been available in both rental and owner-occupied markets. This could help explain why the rental market adjusted primarily through quantities rather than prices and why quantity adjustments also remain important in the owner-occupied market. More generally, anticipation would spread the effects of legalization over time, causing the estimated impulse responses to understate the full impact of the policy on housing demand and supply. Thus, anticipation by housing suppliers may have contributed to the relatively modest responses observed in the housing supply measures, although other explanations are also possible.

3.7 Conclusion

By using quarterly data and a Bayesian Structural VAR framework, we capture the dynamic effects of legalization-induced immigration shocks on housing markets. Unlike prior work that focuses on medium- to long-run averages, our approach allows for a detailed view of how housing outcomes respond in the short run, the medium run, and the long run. In doing so, we provide new evidence on the broader economic consequences of immigration policy, highlighting how changes in legal status affect housing demand, housing supply, and prices in the economy.

The results of this study show that the legalization provisions of IRCA had a lasting effect on immigration flows in the United States. Immigration increased gradually after the policy was implemented, reaching elevated levels over several quarters before stabilizing.

This pattern indicates that legalization induced a shift in the inflow of immigrants rather than a temporary surge, reflecting the broad and persistent impact of the policy.

These shifts in immigration were transmitted to housing markets in a differentiated manner. Rental markets responded relatively quickly, with rental vacancies declining and real rents rising on impact, and with both effects persisting over the sample period. The results suggest that much of the adjustment in the rental market occurs through quantities rather than prices, as the response of rents to the shock is relatively modest compared to the stronger adjustment in vacancies.

The ownership market exhibits stronger responses to the legalization shock compared to the rental market. Home vacancies decline significantly and remain persistently lower over the sample period, while house prices, measured by the quarterly growth in the house price index, increase modestly over the same horizon. Measures of market activity, including the quarterly growth in the number of new single-family houses sold and the median sale price of houses sold, show even more pronounced responses. Overall, these results suggest that adjustment in the ownership market occurs along both quantity and price margins.

One caveat of this analysis is that the results for the ownership market may not be generalizable to all immigration episodes, particularly because the immigrant population studied here differs in important ways from those typically examined in other contexts. A key distinction is that IRCA granted legal status to immigrants who had already been residing in the United States prior to 1982. Given the policy's implementation in May 1988, these individuals had typically been in the country for at least six years before receiving legal status. Over this period, they were likely to have developed stronger labor market attachment and accumulated savings. Moreover, if eligible immigrants anticipated a high probability of obtaining legal status, they may have increased precautionary savings and adjusted their housing plans in advance of legalization. These factors may have enabled some households to prepare for entry into the owner-occupied housing market before becoming legally eligible to purchase homes.

Lastly, we examine the supply-side dynamics of the housing market by assessing whether the legalization shock is associated with increased construction activity. In particular, we

consider whether there is an increase in demand for new building permits and whether this translates into actual increases in housing starts. The results suggest some upward movement in both permit approvals and the number of new constructions started following the legalization shock. However, these estimates are imprecisely estimated and not statistically robust. Taken together, we therefore find no strong evidence of a systematic supply-side response to the legalization shock.

Another important caveat is that some of the housing market adjustment may have occurred before legalization took effect. Just as eligible immigrants may have increased savings in anticipation of obtaining legal status, developers and homebuilders may have anticipated higher future housing demand and adjusted construction plans in advance. If such anticipatory behavior occurred, part of the increase in housing demand and supply would have been realized before the legalization shock identified in our analysis, causing the estimated impulse responses to understate the full effect of legalization on housing markets. This mechanism may help explain the relatively modest responses observed in the housing supply measures and the prominent role of quantity adjustments in both rental and owner-occupied housing markets, although other explanations are also possible.

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