Send Them Back?
The Real Estate Consequences of Repatriations*

Gustavo S. Cortes†
University of Florida

Vinicios P. Sant'Anna‡
Cal Poly

August 2026

Abstract

While governments increasingly adopt large-scale immigrant deportations, their consequences for housing markets and urban growth are largely unknown. We study the 1930s mass repatriation of Mexicans and its effects on U.S. housing markets. Developing a novel Census house-linking algorithm, we document that the repatriation negatively impacted U.S. cities. Mexican-occupied properties saw decreased values and rents but were not subsequently occupied by U.S.-born families. Instead, the repatriation lowered neighboring U.S.-born house values and depressed cities' real estate growth. Our findings reveal significantly negative effects on individual and aggregate housing wealth, challenging the notion that deportations benefit U.S.-born households.

Keywords: Immigration, Anti-Immigration Policies, Real Estate, Housing, Great Depression
JEL Classification: J15, R31, N92

*We thank the editor, Joseph Vavra, and two anonymous referees for constructive suggestions. We are grateful to Dan Bernhardt, Kris Mitchener, Stefan Krasa, and Greg Howard for their continued support. We benefited from the insights provided by our discussants Nicholas Carollo, Min Fang, José García-Barrero, Kevin Park, and Joanna Venator. We also thank Torben Andersen, Simon Buchler, Tiago Cavalcanti, Igor Cunha, Donald Davis, Rebecca Diamond, Gordon Hanson, Sehoon Kim, Elena Lutz, Rafael Matta, Joan Monras, Juan Palacios, Pia Orrenius, Ziho Park, Giovanni Peri, Esteban Quiñones, Jay Ritter, Albert Saiz, Dejanir Silva, Patrick Turner, Abigail Wozniak, and seminar participants at the MIT Center for Real Estate, SKEMA Business School, University of Florida, University of Illinois, University of São Paulo, University of South Florida, the Second Annual Workshop on Macroeconomic Implications of Immigration, The Economics of Migration Junior Seminar, University of Michigan's H2D2 seminar series, Young Economist Symposium, Urban Economics Association Meetings, Economic History Society's Ph.D. Thesis Workshop, AREUEA, Canadian Economics Association, Midwest Economics Association, and AERUS for helpful comments. We are also grateful to Gabriel Mathy, Miguel Morin, and Kenneth Snowden for kindly sharing data.

†Warrington College of Business, University of Florida. 306 Stuzin Hall, PO Box 117168, Gainesville, FL 32611–7168. Email: **gustavo.cortes@warrington.ufl.edu**. Website: **sites.google.com/site/cortesgustavos**.

‡Corresponding Author. Orfalea College of Business, California Polytechnic State University. Email: **santanna@calpoly.edu**. Website: **vpsantanna.com**.

"The Mexican immigrant is not good material for citizenship, and in some places Mexican colonies are decidedly objectionable." — The Washington Post, Jan 25, 1930.

I think it's interesting that people focus on, 'well, how do you deport 18 million people?' Let's start with 1 million." — Senator J.D. Vance, U.S. Vice-Presidential Candidate, Aug 10, 2024.

1 Introduction

Immigration has historically been at the center of political and economic controversies. The economic literature has shown that migration affects local economies through numerous channels, including labor markets, consumption, innovation, and economic growth (Card (1990, 2009); Borjas and Monras (2017); Burchardi, Chaney, and Hassan (2018); Terry et al. (2026); Lee, Peri, and Yasenov (2022); Albert and Monras (2022)). A growing part of this literature has focused on the impacts of migration on housing for at least two reasons. First, housing is essential to most households' wealth and expenditure. Second, since migration flows can generate shifts in the local housing demand curve, intense immigration flows can significantly affect local housing availability and prices—potentially impacting the local wealth distribution, the cost of living, and real wages. However, our current empirical understanding of the effects of migration on housing stems from studies focusing on the *inflows* of immigrants (Saiz, 2003; Ottaviano and Peri, 2006; Saiz, 2007; Howard, 2020).

This paper sheds light on an understudied yet increasingly contemplated migration shock to housing markets: an *outflow* of immigrants. Making this distinction is indispensable due to the inherent irreversibility of housing investment. Once a dwelling unit is built, it is prohibitively expensive to convert it back to investable capital—a characteristic also known as the *putty-clay* nature of real estate investment (Johansen, 1959; Cooke and Hamilton, 1984). The literature provides evidence that in-migration shocks to a city are likely to increase housing demand and that its impact on housing prices will depend on the local housing supply elasticity and the relocation decisions of current residents in response to newcomers (Saiz, 2007; Saiz and Wachter, 2011; Accetturo et al., 2014). However, there is no empirical evidence on how a large immigrant outflow shock affects house prices or rents. Because housing is durable, asymmetric effects on prices may arise, inhibiting us from making accurate predictions about out-migration episodes solely based on in-migration elasticities. It is plausible to anticipate a greater short-term impact on prices due to the inelastic downward nature of housing (Glaeser and Gyourko, 2005). One of our key contributions lies in accurately estimating these effects.

The lack of out-migration studies in the housing literature has become more pressing as we witness the invigoration of national populism and immigration backlash across the globe. In recent years, influential leaders have voiced strong views against immigration in election campaigns and electoral mandates.[1] Following this rhetoric, immigration policy has also been shifting towards stricter actions. While most immigration policies are designed to curb the inflow of immigrants (e.g., by limiting the entry of migrants through quotas), more extreme actions involving the mass expulsion of immigrants through raids and deportations have also become frequent in politicians' speeches.

Our paper studies one of the largest ethnically motivated migration shocks in U.S. history to quantify its effects on housing prices and real estate outcomes in U.S. cities. The Mexican repatriation of the 1930s was a negative, large-scale shock to the Mexican workforce in the United States. Between 1930 and 1936, organized labor movements, the press, and local authorities harassed, pressured, and forced Mexicans to leave the U.S. (Hoffman, 1974; Guerin-Gonzales, 1994; Enciso, 2017; Lee, Peri, and Yasenov, 2022). Documenting the local toll of these efforts, Balderrama and Rodríguez (2006) report that "small barrios virtually disappeared" as homes were "abandoned by their owners."[2] We use data from the U.S. Censuses of 1930 and 1940 to assess whether and how housing market conditions in local economies were affected by the intensity of the repatriation. The broad presence of Mexicans in the U.S. labor force and their dispersed geographic distribution allow us to exploit substantial variation in the repatriation of Mexicans across U.S. cities.

Estimating the effects of out-migration on housing is empirically challenging. The nature of the Mexican repatriation as an out-migration shock allows us to address at least two common issues. First, many large-scale out-migration episodes occur in response to wars or natural disasters (Becker and Ferrara, 2019). Naturally, these events destroy the existing housing supply and affect real estate markets through channels beyond out-migration alone. As an ethnically motivated shock to out-migration rather than an armed conflict or natural catastrophe, the Mexican repatriation is less likely to induce a direct destruction of housing units. Second, business cycle conditions can simultaneously affect housing markets and immigrants' decisions to move, potentially biasing the estimates of the economic consequences of immigration. Since the repatriation explicitly targeted individuals based on a specific nationality

[1] For example, Donald Trump (U.S.), Marine Le Pen (France), Giorgia Meloni (Italy), and Viktor Orbán (Hungary).

[2] In 2005, the State of California passed the Apology Act for the 1930s Mexican Repatriation Program, which officially recognized the *"unconstitutional removal and coerced emigration of U.S. citizens and legal residents of Mexican descent"* and apologized to residents of California for violations of civil liberties and constitutional rights. Legal scholars have also studied the unlawful removal of U.S. citizens of Mexican descent in the 1930s (Johnson, 2005).

through harassment, pressure, and coercion, the resulting Mexican outflows plausibly reflected more than a response to economic conditions.

Nonetheless, the intensity of the repatriation efforts could be correlated with other city characteristics that might affect housing markets. To isolate the Mexican repatriation effect on real estate, we employ an instrumental variable (IV) approach. Our instrument exploits patterns in the road infrastructure of 1930, combined with the historical presence of Mexican immigrants in the U.S. at the turn of the century. More specifically, it combines a measure of exposure to the out-migration shock (the share of Mexican workers in 1900) with a measure of the cost to local authorities of repatriating immigrant workers (the inverse of the travel distance to the U.S.-Mexico border, based on the 1930s U.S. road infrastructure). Our baseline specifications also include a series of control variables, including state fixed effects and sector employment shares, to account for various factors influencing house prices—especially the heterogeneous exposure of cities to the Great Depression.

Leveraging the granularity of our house-level data, we begin our study by investigating how the repatriation affected Mexican-occupied houses. We develop a novel automated matching technique to link addresses across the 1930 and 1940 Censuses from the IPUMS Restricted Complete Count Data (Ruggles et al., 2020). The matched address sample allows us to track each housing unit's value or rent evolution across the two censuses. By following *addresses* rather than people (Feigenbaum, 2016; Abramitzky et al., 2021; Price et al., 2021), we sidestep the selection issues that arise when individuals move across regions, neighborhoods, or homes. Using this sample, our two-stage least squares (2SLS) estimates show that the Mexican repatriation had a strikingly large effect on houses where Mexicans lived in 1930. The value of houses inhabited by Mexicans in 1930 had a 14 percentage point lower growth rate for every percentage point drop in a city's Mexican population. Rents on houses occupied by Mexicans in 1930 also fell by more than eight percentage points for every percentage point of Mexican outflow. More importantly, the effect on U.S.-born-occupied houses is heterogeneous by proximity to the shock: the average effect is not statistically significant, but this average masks a significant negative spillover onto the subset of U.S.-born houses with an adjacent Mexican-occupied neighbor.

Our analysis reveals that the 1930s Mexican repatriation had profound effects on housing markets, extending beyond the direct impact on Mexican-occupied homes. We uncover novel evidence that cities with higher repatriation rates experienced a significant shift in the ownership structure of Mexican-inhabited homes, with a higher probability that renter-occupied units were converted to owner-occupied units. Strikingly, we also find that the repatriation did not lead to U.S.-born residents moving into homes previously occupied by Mexican immigrants, even as the Mexican population shrank. This finding challenges the notion that the repatriation created new housing opportunities for U.S.-born households and suggests a persistent pattern of residential segregation. In fact, our findings that the repatriation decreased the property values of U.S.-born residents living near Mexican-occupied homes highlight the negative consequences for communities affected by the policy. These results are consistent with the literature on how neighboring properties experience meaningful valuation declines due to foreclosures and the associated disamenities (e.g., Campbell, Giglio, and Pathak, 2011; Mian, Sufi, and Trebbi, 2015; Gupta, 2019).

Finally, we examine the aggregate effects of exposure to Mexican repatriation on the growth rates of city-level real estate outcomes. Our baseline estimates indicate that cities with greater exposure to Mexican repatriation experienced lower growth rates across all our housing outcome variables. Precisely, we find that one standard deviation (1 p.p.) larger repatriation depressed the median house value growth rate (by 1.79 p.p.) and the median rent growth rate (by 1.25 p.p.). Comparing these results with the observed growth rates of median house values and rents in the decade helps to put these numbers in perspective. On average, median house values (rents) decreased 20.5% (4.6%) in the decade. Consequently, a one-standard-deviation increase in exposure to Mexican repatriation accounts for approximately 8.7% and 27% of the average decrease in house values and rents, respectively.

While lower median rents and median house prices might initially suggest increased affordability for U.S.-born families, this interpretation is inconsistent with our house-level evidence that the repatriation did not cause U.S.-born residents to occupy the houses previously inhabited by Mexicans. Instead, these aggregate city-level results align with the hypothesis that cities experienced a slowdown in real estate market dynamism due to a significant population loss and persistent residential segregation patterns. This hypothesis is further corroborated by our analysis of cities' building permits. We find that the repatriation depressed the growth rates of building permits: a city with a one standard deviation (1 p.p.) larger repatriation saw its growth rates for the value of building permits decrease by 5 p.p. between 1930 and 1940. Because building permits capture current and future expectations about local construction activity (Cortes and Weidenmier, 2019; Cortes and LaPoint, 2024), our results suggest that the loss of demand from Mexican families slowed down residential and commercial real estate markets. In sum, our findings suggest that not only did the repatriation fail to enhance housing affordability for incumbent U.S.-born residents, but it also deteriorated the expectations about future housing market conditions and local real estate activity.

We conduct a series of robustness and validation tests to mitigate concerns about our empirical strategy. First, we show that our instrumental variable is not associated with adverse economic conditions or with various measures of the intensity of the Great Depression, supporting the validity of the exclusion restrictions. Second, we find no evidence of a linear association between our instrument and the outflow of other immigrant groups, thereby alleviating concerns that our instrument is linked to common factors affecting the mobility of immigrants in general. Another concern is the potential existence of pre-trends in housing markets related to the presence of Mexican immigrants in a city. We address this issue in two ways. First, given the absence of Census data on house values and rents before 1930, we examine pre-trends using building permit data as a proxy for housing market conditions. This provides us with a valuable alternative approach, as changes in permit activity tend to closely reflect shifts in housing demand and prices. Reassuringly, we find no evidence of pre-trends in the growth rates of permit values or quantities associated with the pre-existing Mexican population. Second, we demonstrate that our main results are robust to controlling for pre-repatriation trends in the non-Mexican population. The stability of our estimates after accounting for the previous decade's trends further supports the interpretation that the observed housing market effects are plausibly attributable to the repatriation event rather than a continuation of existing population dynamics.

Our paper contributes to several branches of the extensive literature on the economic consequences of immigration. Within this broad literature, this paper is related to the studies focused on the effects of forced migration (for a review, see Becker and Ferrara, 2019). Most studies have focused on assessing the economic effects on receiving regions (Hornung, 2014; Schumann, 2014). In contrast, we provide evidence of the economic consequences of forced migration for origin regions. This is closely related to Chaney and Hornbeck (2016), Testa (2021), and Ferrara and Fishback (2024), who study politically or ethnically motivated expulsion events in history. While these papers show evidence of depressed regional growth from sending regions, our paper is the first to directly quantify the effects of an ethnically motivated out-migration shock on housing markets, one of the main channels through which migration can affect economic growth.

Our paper also relates to the recent literature that focuses on the economic effects of restrictions on immigration based on race or country of origin (Clemens, Lewis, and Postel, 2018; Feigenberg, 2020; Tabellini, 2020; Abramitzky et al., 2023). In studying the economic consequences of the 1930s Mexican repatriation, our analysis is closely related to Lee, Peri, and Yasenov (2022), who studied the labor market consequences of the Mexican repatriation. The authors find that in regions more exposed to the repatriation, U.S.-born workers had a lower probability of having a job in 1940, and this effect was larger on low-skilled workers. The authors also find that the repatriation did not cause internal migration of U.S.-born workers to replace the repatriated immigrants. Our analysis contributes to their findings by examining the effects of the Mexican repatriation on the U.S. housing markets. We use real estate conditions to gauge the costs to local economic growth because housing is a crucial channel through which immigration affects economic activity.

A growing strand of the literature focuses on the effects of immigration shocks on general prices (Lach, 2007; Cortes, 2008), and specifically on housing prices (Saiz, 2003; Greulich et al., 2004; Ottaviano and Peri, 2006; Saiz, 2007; Sharpe, 2019; Howard, 2020; Ang, Kwon, and Zheng, 2026). To study the causal effects of migration on house prices and availability, the existing literature typically leverages exogenous shocks to migrant inflows into a region. Most studies find that immigration increases rents and house prices, suggesting that immigrants do not entirely displace natives. In some settings, immigration is shown to have a local negative effect on house prices due to native flight (Saiz and Wachter, 2011; Sá, 2015). Baerlocher, Cortes, and Sant'Anna (2025) document that the Dust Bowl migration reshaped the housing market of Los Angeles—the leading destination city for environmentally displaced families—highlighting how forced migration can reshape prices and the composition of urban housing stocks. Few studies, however, focus on the effects of forced migration episodes on housing markets at the origin. A recent study by Howard, Wang, and Zhang (2024) estimates the impact of the 2008 Secure Communities program on construction costs and house prices during the Great Recession, focusing on how labor market disruptions affect housing supply. Our paper examines a fundamentally different, proportionately larger shock that reduced the Mexican immigrant population by over 30%. Unlike the supply-focused analysis of Howard, Wang, and Zhang (2024), our study captures a predominantly demand shock on housing. This distinction is crucial because understanding both supply- and demand-side channels is essential for a comprehensive assessment of how mass deportations affect housing markets—particularly given the durable and irreversible nature of housing investment.

This paper also relates to the literature studying populism and nationalism, primarily focusing on 1930s America (Bennett, 1969). Recent work has focused on the direct consequences of exposure to populist radio hosts on political preferences (Wang, 2021) or the effects on uncertainty caused by populist leaders' actions (Mathy and Ziebarth, 2017). The political economy of immigration restrictions has been extensively studied. Benhabib (1996) and Ortega (2005) theoretically formalize how anti-immigration attitudes and immigration quotas arise from voting equilibria, while Tabellini (2020) and Alesina and Tabellini (2024) empirically document that immigration triggers political backlash and more conservative policies. Our paper thus contributes to understanding the economic consequences of immigration backlash rooted in the ideals of national populism. Our findings are particularly relevant to today's ongoing debate on the economic effects of immigration backlash, showing that repatriations have large impacts on house values and rents in more exposed homes and that these effects matter to city-level housing market indicators.

2 Mexican Immigration in the 1920s and the 1930s Repatriation

The U.S. experienced a massive influx of immigrants in the late 19th century and the early 20th century, especially from Europeans escaping adverse conditions in their home countries. Mexican immigration grew steadily throughout the early decades of the 20th century, but especially during the 1920s. This robust inflow was driven mainly by U.S. employers recruiting Mexican workers in the agriculture, railroad, meatpacking, and steel mill sectors. In the 1920s, the number of Mexican immigrants increased dramatically. The increased demand for cheap labor coincided with the 1924 Immigration Act, which imposed quotas on European immigration (Abramitzky et al., 2023), prompting many employers to turn to Mexican labor to fill the job vacancies. The pressing economic conditions and armed conflicts in Mexico, such as the Mexican Revolution (1910–1920) and the Cristero War (1926–1929), also contributed to the inflow.

As the U.S. economy entered the Great Depression, starting with the 1929 Crash, organized groups, including labor unions, local authorities, and local media, pressed for immigration quotas and repatriation of Mexicans (for an example of such pressure, see Panel B of Figure A.1 in the Internet Appendix). As the Depression deepened, many Americans began to view Mexicans as unwelcome aliens who were a burden on their local community.[3] The historiography argues that the Mexican community was an easier target for two main reasons. First, the number of Mexican immigrants had increased dramatically in the previous years, making them the

[3] Balderrama and Rodríguez (2006) presents historical evidence that local media, labor unions, and officials often overestimated the relief expenditures towards Mexican immigrants. Alesina, Miano, and Stantcheva (2023) show that these perceptions that immigrants are a burden to the welfare system are still common in many countries.

largest group of newcomers in many cities in the United States. As Cikara, Fouka, and Tabellini (2022) shows, when a minority group increases, they are more likely to be targeted with hate crimes and more negative attitudes. The existing historical evidence supports the idea that Mexican immigrants were targeted mostly because of the great inflow observed in the previous decades. Second, because of ethnic and cultural differences, some Americans often saw them as unassimilable, that is, unable to become a part of American culture and society (Simon, 1974; Balderrama and Rodríguez, 2006; Enciso, 2017).

The local and decentralized nature of the Mexican repatriation makes it challenging to determine the exact number of repatriated individuals. This uncertainty explains the variation among historians' estimations of the total Mexican outflow in the 1930s.[4] The more conservative estimates by Hoffman (1974) and Gratton and Merchant (2013) suggest that nearly 400,000 Mexicans left the U.S. between 1929 and 1937, while Balderrama and Rodríguez (2006) cites a much larger number—from 1 million to 2 million, based on estimates that attempt to include those undocumented immigrants omitted from official calculations.

Using the Census, we calculate that there were 263,900 fewer Mexican immigrants in the U.S. in 1940 than in 1930, which constituted 33.2% of the overall Mexican population present in the U.S. as of 1930.[5] Figure 1 depicts the surge of Mexican-born immigrants between 1900 and 1920, followed by a sharp decline between 1930 and 1940. Another interesting fact from Figure 1 is that the number of immigrants from other (non-Mexican) Latin American countries did not experience such a decline in the same period.

While the existing historiography varies in its accounts of the period, there is a consensus that coercion, forced deportation, and various other fear-spreading tactics were extensively used against the Mexican population of the time. The historical accounts of this period document that local authorities encouraged and enforced repatriation, even though it was officially categorized as voluntary migration (Valdés, 1988; Balderrama and Rodríguez, 2006). Immigration officers and local police sometimes assisted welfare agencies and even staged raids to convince Mexicans to depart. They often harassed Mexicans, provided free transportation, and, on many occasions, coerced them to leave their U.S. homes. The lack of granular data on individual U.S.-Mexico border

[4] The most intense period of Mexican deportations and repatriations was 1929–1934. Historical evidence shows that deportations and repatriations continued until 1937 (Hoffman, 1972).

[5] The observed decrease in the number of Mexican immigrants living in the U.S. between 1930–1940 is likely to be an underestimate of the actual number of people affected by the repatriation for a couple of reasons. First, Balderrama and Rodríguez (2006) highlights that repatriation efforts also targeted a large number of U.S. citizens of Mexican descent. Second, many Mexican immigrants were undocumented, making them more likely to avoid government surveys and census takers.

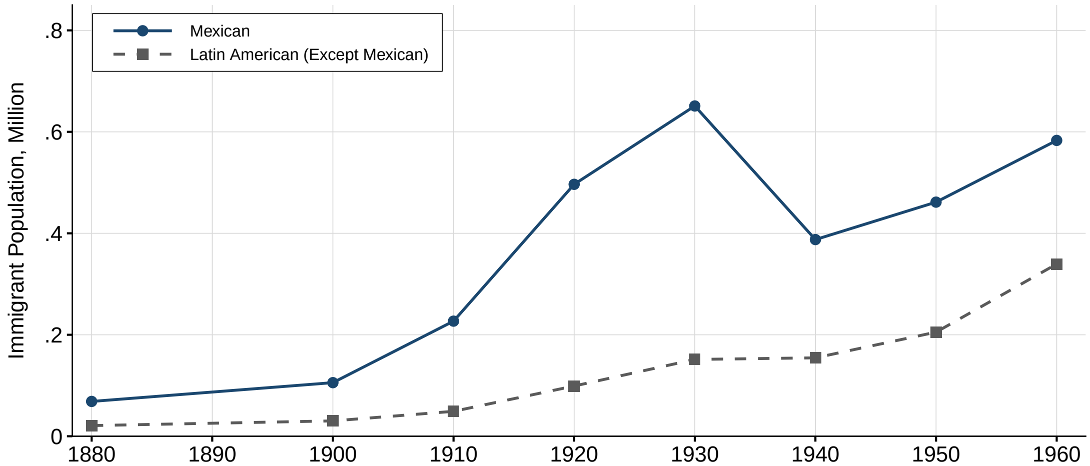


Figure 1. Total Latin American immigrant population in the United States by origin (1880–1960). This figure shows the total number of Mexican and other Latin American immigrants in the United States from 1880 to 1960. Immigrants are defined according to the country of birth reported on each census. Latin American countries include Central America (Belize/British Honduras, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama), the Caribbean (Cuba, Dominican Republic, Haiti, Jamaica, Antigua-Barbuda, Bahamas, Barbados, Dominica, Grenada, Montserrat, St. Kitts-Nevis, St. Lucia, St. Vincent, Trinidad and Tobago, Turks and Caicos, British Virgin Islands, Netherlands Antilles, Curacao, Guadeloupe, and St. Barthelemy), and South America (Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Guyana/British Guiana, Paraguay, Peru, Suriname, Uruguay, and Venezuela).

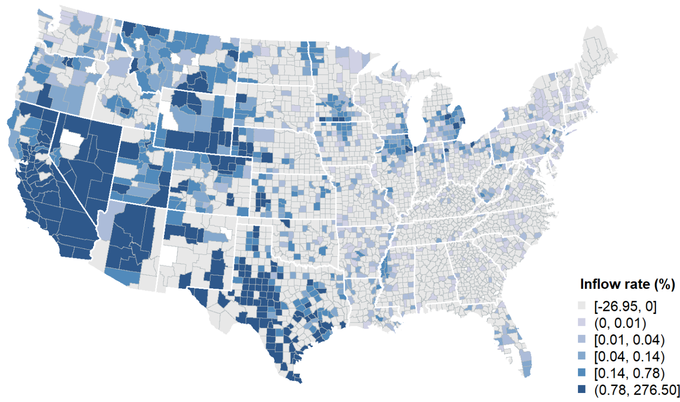
Alt text: Line chart, 1880 to 1960, of U.S. immigrant population in millions: a solid line for Mexican immigrants and a dashed line for other Latin Americans.

crossings during that time makes it difficult to determine the exact impact of these repatriation efforts on the overall Mexican outflow. The available estimates in the literature suggest that the repatriation efforts involving harassment, coercion, pressure, and forced deportations were the primary determinant of the Mexican outflow of the 1930s. For instance, using the outflow of French-Canadian immigrants as a comparison group, Gratton and Merchant (2013) estimate that over 70% of the observed net Mexican outflow was involuntary, due to excess repatriation.

In the Internet Appendix Figure A.1, we present historical newspaper evidence of anti-Mexican sentiment and hostile acts targeted at Mexican immigrants. These acts involved the government (e.g., immigration officers), organized labor (e.g., unionized workers), and the press (e.g., local and national newspapers). As shown in Panel A, *The New York Times* reported in 1931 that 35,000 Mexican immigrants in California were "*pressed by economic diversity, fearful over recently renewed activities of immigration authorities, and perplexed by what they regard as anti-Mexican sentiment.*" This illustrates that economic distress ("*idleness*") and tighter enforcement from government immigration officials played an essential role in Mexicans' decision to leave the country.[6]

[6]One example is the intensity of the Immigration Service's efforts in deporting Mexicans. According to Balderrama and Rodríguez (2006), 46.3% of those deported from the U.S. between 1930–1939 were Mexican. Yet, according to Census data, Mexicans comprised less than 5% of the total immigrant population in the U.S. in 1930.

(A) Mexican Inflow (1920–1930)



(B) Mexican Outflow (1930–1940)

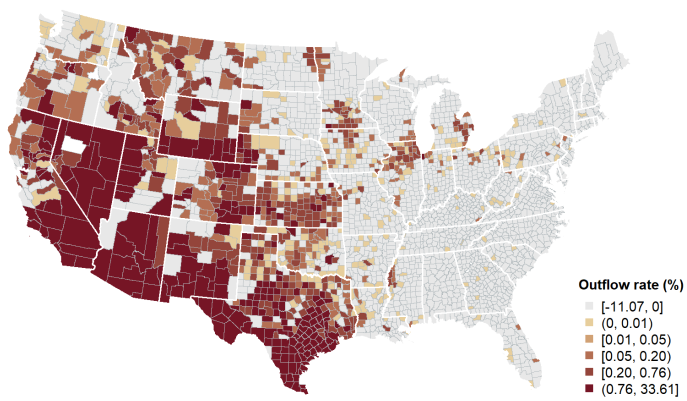


Figure 2. Geographical distribution of the inflow and outflow of Mexicans. This figure shows, for each county, the 1920-30 inflow (Panel A) and the 1930–40 outflow (Panel B) of Mexicans as calculated from the full-count U.S. Censuses of 1920, 1930, and 1940. Nationality is defined by place of birth. The flows are measured as the county-level change in the Mexican working-age population relative to the local working-age population. For instance, a Mexican outflow of 10% means that the drop in the Mexican population of that county is equivalent to a 10% drop in the total working-age population. We define counties according to 1930 limits by IPUMS–NHGIS (Manson et al., 2020).

Alt text: Two U.S. county maps: Mexican inflow 1920–1930 (Panel A) and outflow 1930–1940 (Panel B), both darkest across the Southwest border states.

In Panel B, the *Los Angeles Times* reports a near-riot in 1930 in which some 50 unemployed American laborers "*tried to prevent Mexican workmen (...) to continue their work by guarding their toolboxes and demanding that the contractors employ white labor.*" The national and local press also contributed to the hostile climate by publishing articles and opinions demeaning to Mexican immigrants. For example, as shown in Panel C, an article published by *The Washington Post* argued that "*the Mexican immigrant is not good material for citizenship, and in some places, Mexican colonies are decidedly objectionable*," even though it conceded that, given the economic importance of Mexican immigrants, "*a sudden reversal of* [immigration] *policy would work great hardship to employers in the Southwest*."

We next document the geographic distribution of Mexican immigration across U.S. counties before and after repatriation. Panel A of Figure 2 presents the inflow as measured by the change in the total number of Mexicans between 1920 and 1930 as a share of each county's total working-age population. Darker blue shades represent higher inflows. Conversely, Panel B of Figure 2 portrays counties with a greater outflow of Mexicans between 1930 and 1940 with darker shades of red. The maps show that while the outflow of Mexican immigrants was more pronounced in the counties closer to the U.S.-Mexico border, we still observe large outflow rates in cities across the country.

Another helpful way to put the 1930s Mexican repatriation in perspective is to compare the outflow rates of Mexicans with those of immigrants from other countries. Figure 3 plots, for each ethnic group, the U.S. working-age population growth rate in 1920–1930 (hollow circle) and 1930–1940 (solid circle), with an arrow tracing the change from the pre-repatriation to the post-repatriation decade. Groups are ordered by the magnitude of their decade-over-decade decline, so Mexico appears at the top. The figure shows that the massive Mexican outflow of about 33% during the 1930–1940 period is unmatched by other groups: it exceeds the European outflow by roughly 1.6 times, while Canadians—who also share a border with the U.S. and could return more easily than other nationals—had less than half that rate. A final takeaway from Figure 3 is that non-Mexican Latin American immigrants experienced a substantially smaller outflow—even though the inflows of both Mexican and non-Mexican Latin immigrants had grown at similar percentage rates in the previous decade.

3 Immigrant Repatriation in a Spatial Framework

In this section, we introduce a simple spatial framework to rationalize our empirical strategy for isolating the effects of repatriation and to illustrate potential channels through which repatriation can affect housing prices. The model suggests a set of relationships to be tested in the data. We generalize previous spatial frameworks in which a group of residents has a distaste for a minority and takes action in response to the minority's presence.

In the context of racial segregation, Boustan (2010) and Akbar et al. (2025) show that white residents can respond by moving out of neighborhoods with a high concentration of black residents, a phenomenon also known as "white flight." In the context of immigration, studies show evidence that growing immigrant presence can also induce "native flight" if immigrant enclaves are perceived as less desirable places to live by incumbent natives (Saiz and Wachter, 2011; Moraga, Ferrer-i-Carbonell, and Saiz, 2019; Andersson, Berg, and Dahlberg, 2021; Boje-Kovacs et al.,

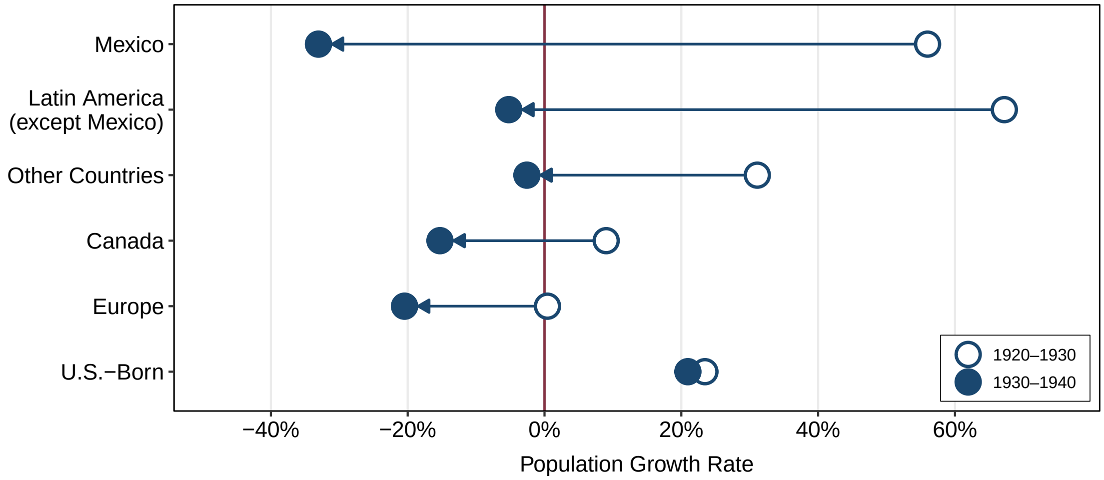


Figure 3. Percent changes in working-age population by origin: 1920–30 versus 1930–40. For each ethnic group, this figure plots the 1920–30 (hollow circle) and 1930–40 (solid circle) growth rate of the working-age population of each nationality in the U.S. labor force; an arrow traces the change from the pre-repatriation to the post-repatriation decade. Groups are ordered by the magnitude of their decade-over-decade decline. Nationalities are defined based on the person's place of birth from the U.S. Census. For example, between 1920 and 1930, the Mexican immigrant population in the U.S. grew by roughly 56%, then contracted by about 33% in the following decade (1930–40). See notes on Figure 1 for a list of countries included under *Latin America*.

Alt text: Arrow plot of working-age population growth rates by origin group, comparing 1920–1930 (hollow circle) with 1930–1940 (solid circle).

2024). We complement this literature by assessing another way in which natives can respond to immigrant presence: by engaging in repatriation efforts that forcibly push immigrants to leave the country. While flight responses are essentially a household's choice, pursuing repatriation or other policies to bar immigrants requires a collective effort and some degree of social and political coordination. We argue that such drastic actions can find support among large segments of the native population during economic crises. Recent studies show that populist nationalism can arise from worsening economic conditions (Pastor and Veronesi, 2021; Wang, 2021).

Suppose every resident in a U.S. city is a worker, and every city has two types of residents: U.S.-born Natives (N) and a new group of immigrants that we will refer to as Mexican immigrants (M). Denoting by L_N and L_M, respectively, the total working-age population of natives and Mexican immigrants, we define the share of Mexican immigrants in a city's total population as $\lambda = {}^{L_M}/_{(L_M+L_N)}$. The economy has two states of the world, defined by $S \in \{G, B\}$: a good economic state ("non-crisis") and a bad economic state ("crisis").

Native Worker Preferences. Following the Rosen–Roback intuition that workers value local housing costs and city fundamentals (Rosen, 1979; Roback, 1982), let the utility of a native worker in state of the world S, $U_N^{(S)}(\cdot)$, be written as:

$$U_N^{(S)}(p, z, \lambda) = h(p, z) + g^{(S)}(\lambda). \tag{1}$$

$h(p, z)$ is decreasing in the price of housing (p) and increasing in the city-level demand shifter (z) that represents local amenities or productivity. The component $g^{(S)}(\lambda)$ captures how the share of Mexican immigrants affects a native's utility in ways that depend exclusively on the state of the economy S.

Assuming that the country-level economic state S is common to all cities, it does not directly affect a worker's decision of what city to live in within the country in a given period. What changes with S is how natives perceive the presence of immigrants. We capture this reduced-form change through the state-dependent component $g^{(S)}(\lambda)$:

$$g^{(S)}(\lambda) = \begin{cases} 0, & \text{if } S = G \\ f(\lambda), & \text{if } S = B \end{cases} \tag{2}$$

When the U.S. economy is not in crisis ($S = G$), natives are indifferent to the share of Mexican immigrants in a city. However, in times of economic crisis ($S = B$), natives may develop a distaste for Mexican immigrants depending on λ, i.e., the relative importance of Mexican immigrants in a city's total population. Distaste for Mexican immigrants may reflect common, stereotypical feelings towards Mexicans during the 1930s, such as the perception that Mexicans were not fit for U.S. society or that they were "stealing" jobs from natives or were a burden to local government spending. This structure also follows recent findings showing that natives' attitudes toward immigrants or other minorities tend to become more pessimistic during economic crises (Isaksen, 2019; Bursztyn et al., 2022). We normalize $f(0) = 0$ and assume that, in the crisis state, distaste for immigrant workers is captured by a continuous and decreasing function of the share of immigrants, $f(\lambda)$, with $f'(\lambda) < 0$, which together imply $f(\lambda) < 0$ for $\lambda > 0$. Thus, the larger the share of immigrants in the city, the larger is the utility loss natives associate with immigrant presence. This block is meant to capture the political desirability of immigrant presence; it does not by itself determine housing prices.

Repatriation Efforts. Engaging in repatriation efforts requires collective action, political mobilization, and administrative resources. Suppose that supporting such efforts entails a fixed

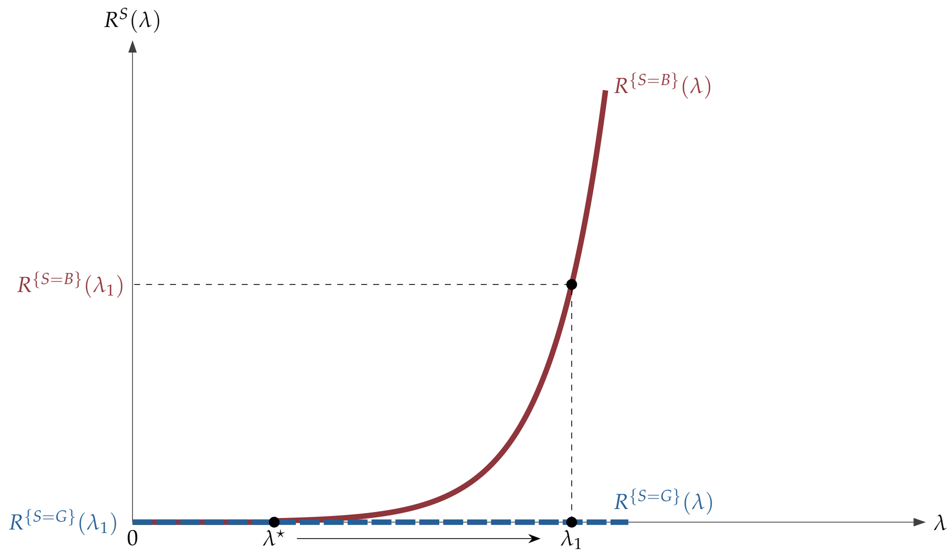


Figure 4. Repatriation Function. This figure depicts the repatriation efforts or intensity as an increasing function of the share of Mexicans in the city. The dashed blue line depicts the city in a good state ($S = G$) equilibrium, such that the native population is indifferent to the share of Mexicans λ_1. In the good state, there are no repatriation efforts for any share of Mexicans, λ. The continuous red line represents the repatriation function when the economy changes its state to $S = B$. In the bad state, there will be an increase in the anti-Mexican sentiment, i.e., natives will have a distaste for a high share of Mexican workers ($\lambda > \lambda^\star$). In cities where the initial share of Mexican workers exceeds the threshold level $\lambda^\star$, there will be repatriation efforts carried out by natives.

Alt text: A line chart showing the repatriation effort on the y-axis and the Mexican population share on the x-axis, with the curve rising past the threshold $\lambda^\star$.

coordination cost $\kappa > 0$ for natives in a city. In the crisis state, natives support repatriation when the utility loss from immigrant presence, $-f(\lambda)$, exceeds this cost. Because $f(\lambda)$ is decreasing in λ, there exists a threshold $\lambda^\star$ such that cities with $\lambda > \lambda^\star$ support repatriation, whereas cities with $\lambda \leq \lambda^\star$ do not. Denote repatriation efforts by $R^{(S)}(\lambda)$. In the non-crisis state, natives are indifferent to immigrant share and thus $R^{(S=G)}(\lambda) = 0$ for all λ. In the crisis state, repatriation efforts follow:

$$R^{(S=B)}(\lambda) = \begin{cases} 0, & \text{if } \lambda \leq \lambda^\star \\ \tilde{R}(\lambda), & \text{if } \lambda > \lambda^\star \end{cases} \tag{3}$$

where $\tilde{R}(\lambda) > 0$ and $\tilde{R}'(\lambda) \geq 0$. When the regime switches to a crisis state, repatriation efforts arise only in cities where immigrants exceed the threshold share $\lambda^\star$. Figure 4 illustrates repatriation efforts as a function of the immigrant share λ. The role of Eq. (2) and (3) is to explain where repatriation pressure emerges: cities with larger pre-existing Mexican communities are more exposed to repatriation efforts during a crisis. Recent literature shows that the attitudes of natives towards immigrants can be positively or negatively associated with their local presence. Earlier theoretical work by Benhabib (1996) and Ortega (2005) formalizes how immigration attitudes and quotas can emerge from a voting equilibrium, a mechanism consistent with our threshold structure. Empirically, Tabellini (2020) and Alesina and Tabellini (2024) show that increased immigration tends to increase support for anti-immigration policies. Conversely, a more positive perception among natives towards immigrants can emerge over extended periods of coexistence (Bursztyn et al., 2024) or from witnessing the struggles of immigrant groups (Andries et al., 2023). Considering that the major influx of Mexican immigrants occurred in the few decades preceding 1930 and the existing residential segregation between Mexican immigrants and natives, it is more plausible that the presence of Mexican immigrants was associated with the anti-Mexican sentiment observed in the 1930s and, consequently, repatriation efforts.

Mexican Immigrant Preferences. We do not need to fully specify Mexican immigrants' utility for the comparative statics of the model. Let $U_M^{(S)}(p, z, \lambda)$ denote immigrant utility, assumed to be decreasing in housing prices p and increasing in local conditions z and the city's share of Mexican immigrants λ, the latter capturing the value of co-ethnic networks. Under the good economic state $(S = G)$, immigrants choose to stay in the U.S. whenever their utility is at least as high as some reservation utility in Mexico, $\underline{u}_M$:

$$U_M^{(S)}(p, z, \lambda) \geq \underline{u}_M \tag{4}$$

Under the bad economic state $(S = B)$ and the existence of local repatriation efforts $R^{(S)}(\lambda)$, Mexican immigrants may be deported regardless of their preferences—even if their utility from staying exceeds their reservation level, $\underline{u}_M$. In other words, the imposed repatriation efforts can override individual utility considerations, forcing Mexican immigrants to leave even when their utility from staying surpasses $\underline{u}_M$. This model reflects the historical reality of the 1930s Mexican repatriation, which involved widespread efforts to expel Mexican immigrants from the country rather than simply displacing them within the U.S.[7]

Consider the period of the Great Depression as a switch to the crisis state $(S = B)$. This shift raises anti-Mexican sentiment country-wide and makes repatriation politically feasible in cities with sufficiently high Mexican shares. Since the threshold is more likely to be crossed when λ is large, the model predicts that cities with larger pre-existing Mexican communities

[7]For simplicity, the model assumes that Mexican workers had no option to relocate to U.S. cities with less strict repatriation policies, reflecting Section 2's historical evidence of systematic pressure to expel Mexicans nationwide.

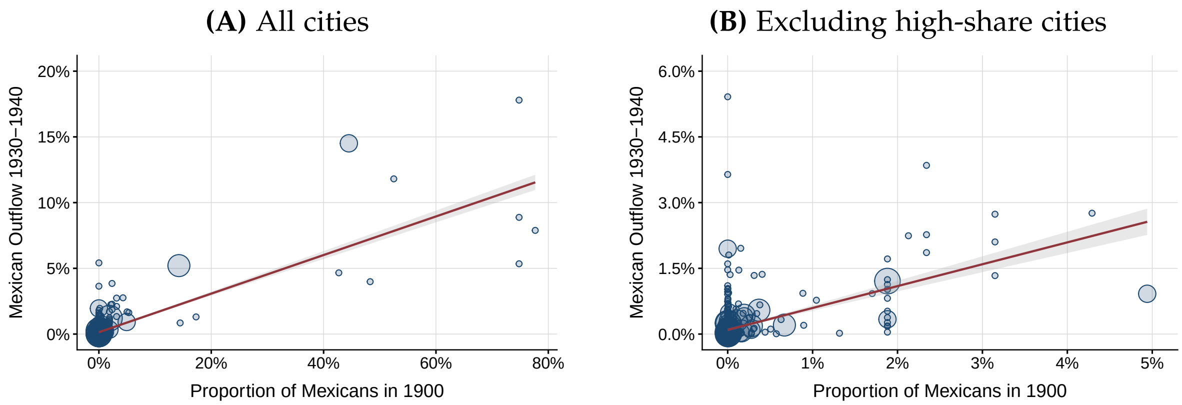


Figure 5. Positive correlation: Repatriation intensity vs. Mexican population shares from 1900. This figure shows the positive correlation between *Mexican Outflow* (cf. Eq. (5)) and the 1900 county-level share of Mexicans (before the repatriation). Panel A presents the scatter plot for all cities in our sample, and Panel B repeats the scatter plot after excluding cities with more than 5% share of Mexicans in 1900.

Alt text: Two scatter plots: 1930–1940 Mexican outflow versus 1900 Mexican share, with a positive-sloping fitted line; Panel A all cities, Panel B drops high-share cities.

should experience stronger repatriation efforts and, consequently, larger Mexican outflows. Once those outflows occur, they reduce local housing demand.

The data show that counties with larger Mexican populations also experienced higher outflow rates in the 1930s. We present this correlation more clearly in Panel A of Figure 5, which shows a strong linear relationship between the 1900 Mexican population share and the 1930–1940 outflow of Mexicans. Although inflows and outflows were more substantial in counties near the Mexico border, they were notably widespread. We observe counties with considerable migration flows in the West (Oregon, Nevada, Washington), the Midwest (Illinois, Indiana, Michigan, Ohio), and the East (Pennsylvania). The relationship remains strong for cities throughout the entire geographic span of the continental U.S., even if we exclude very high-share cities (usually at the border), as shown in Panel B of Figure 5.

Housing Prices. The previous block explains where repatriation pressure is strongest. We now describe how the resulting outflow affects local housing markets. From the seminal work of Glaeser and Gyourko (2005), we assume that the price of housing is a function of the number of workers in the city $L_T \equiv L_N + L_M$, and that, conditional on construction costs, the decisions of the construction sector determine the price elasticity of housing supply. In this case, the housing supply curve will be kinked, generating an asymmetric response to changes in demand: an increase in demand leads to new construction, but a decline in demand does not lead to an immediate reduction in the housing stock.

During the 1930s, most U.S. cities were in a state of low construction and housing market decline. Using various housing price series, Fishback and Kollmann (2014) show that 1940 prices were significantly lower than in 1930. In addition, Table 2 presents summary statistics from the U.S. Census data and shows a substantial decline in the median house value, median rents, and building permit value for the average U.S. city.[8] In this context, repatriation-induced outflows reduce total local housing demand. Depending on local economic conditions, population loss reduces house prices and discourages new construction, especially when the housing stock adjusts sluggishly. From this reasoning, we predict that *if natives engage in the repatriation of Mexican immigrants, the resulting outflow will reduce city housing prices and the construction of new houses.*

What are the expected effects of the Mexican repatriation on house values and rents? In equilibrium, the cost of renting should equal the user cost of homeownership (Poterba, 1992; Gilbukh et al., 2023). We can formalize the rent-to-price ratio as the user cost of homeownership: $R/P = [(r + \tau_p) \times (1 - \tau) + \delta - g + \gamma]$, where r is the interest rate, τ_p is property taxes, τ is the marginal income tax, δ is maintenance and economic depreciation costs, g is the expected rate of price appreciation, and γ is the housing risk premium. While we expect the repatriation-induced demand shock to depress both rents and house values, whether the proportional effect is larger for house values or for rents depends on the net effect on the user cost of homeownership. For instance, if households perceived the repatriation as permanent, and expected continued out-migration or sustained low Mexican population—as one would expect from a policy-driven, ethnically targeted population removal—this would lower expected house price growth (g), raising the user cost of ownership, amplifying the percentage decline in house prices relative to rents. Likewise, heightened uncertainty about local housing demand induced by the repatriation would have raised the housing risk premium (γ), thereby increasing the user cost of ownership. Therefore, we predict that *the absolute effect on house values should exceed that on rents to the extent that the repatriation plausibly raised the user cost of ownership in affected cities.*

How will the effects of the Mexican repatriation be distributed within each city? The answer to this question will depend on the level of residential segregation between Mexican immigrants and the rest of the population within cities. If the housing market is segmented, with immigrants

[8] Although recent work documents supply-side effects of immigrants through construction employment (Monras, 2020; Howard, Wang, and Zhang, 2024), Mexicans comprised less than 1% of the 1930 construction workforce (Table B.2), and Table B.3 shows that the repatriation-induced reduction in Mexican construction employment was offset by a proportional increase in U.S.-born employment. Combined with the historically low construction activity of the period (U.S. Bureau of the Census, 1949), supply-side forces were unlikely to be the primary driver of house prices in our sample.

clustering in neighborhoods within cities, then the decline in house prices should be greater in areas where immigrant presence is more pronounced. In fact, Eriksson and Ward (2019) shows relatively high levels of residential segregation for Mexican immigrants in U.S. cities. In this context, we predict that *the effects of the Mexican repatriation will be larger for houses inhabited by Mexicans in 1930 and for other houses that were neighbors of Mexican immigrants.*

In this framework, we have considered how housing prices are affected by immigrant outflow pushed by repatriation efforts. However, changes in the local economy may also have affected immigrants' decision to leave the U.S. For instance, a decrease in cities' productivity (z) could simultaneously have affected the outflow of immigrants and house prices. This process could generate a spurious correlation between outflow migration and house prices. The simple spatial framework helps rationalize the importance of focusing on the ex-ante distribution of a specific immigrant population as a source of exogenous variation in efforts to repatriate that group and, consequently, in outflow rates in U.S. cities. In the next section, we outline our instrumental variable approach and discuss its underlying assumptions.

4 Empirical Strategy

4.1 Identifying the Mexican Repatriation Effect

The objective of our baseline empirical work is to study how the 1930s Mexican repatriation affected the evolution of housing prices in the United States. Our goal is to isolate the effect of the ethnically motivated out-migration shock from any other factor associated with housing markets. Following Lee, Peri, and Yasenov (2022), we define the city-level measure of Mexican outflow from city c between 1930 and 1940 as:

$$O^{MEX}_{c,1930:40} = -\left(\frac{Pop^{MEX}_{c,1940} - Pop^{MEX}_{c,1930}}{Pop_{c,1930}}\right), \tag{5}$$

where $Pop^{MEX}_{c,t}$ is the Mexican working-age population in city c in year t (1930 or 1940), and $Pop_{c,1930}$ is the total working age population in city c in 1930.[9] Because the repatriation com-

[9] We define the working-age population as individuals aged between 18 and 65 years, not living in group quarters.

prises a decline in the population of Mexican workers, we multiply the growth rate by minus one for a more straightforward interpretation.[10]

The main drawback of estimating the relationship between Mexican outflows and housing price changes is that, even after controlling for observable characteristics, migration flows may be correlated with other local economic conditions. The primary concern is that the intensity of the Mexican repatriation might have been correlated with other local economic conditions that could have influenced the evolution of housing markets between 1930 and 1940. We employ an instrumental-variable (IV) approach to address potential endogeneity between Mexican outflow and housing market outcomes and to produce causal estimates.

Our instrument combines the infrastructure of the U.S. road network and the historical presence of Mexican immigrants in a county at the turn of the century. The relevance condition for our IV is supported by historical evidence that repatriation occurred more intensively in counties with larger pre-existing Mexican communities (cf. Figure 5). To avoid contemporaneous confounders, we use the share of the county's Mexican population in 1900 as an *ex-ante* measure of repatriation exposure.[11] This has at least two advantages. First, it is less likely that the 1900s Mexican population share in a county is correlated with the economic fundamentals of the Great Depression in the 1930s.[12] Second, for historical and cultural persistence reasons, the 1900 share is still sufficiently correlated with the size of the Mexican population in 1930 and, therefore, with the population potentially exposed to the repatriation.

Transportation infrastructure also played an essential role in repatriation. Historical evidence shows that existing highways and railroad trackage governed migration pathways between Mexico and the United States. As documented by Hoffman (1974), the infrastructure of the years before the Great Depression funneled travelers through the U.S.–Mexico main border crossing stations. Panel A of Figure B.1 in the Internet Appendix shows these stations in Texas (El Paso, Brownsville, Laredo), Arizona (Nogales, Douglas), and California (Calexico). We use data on county-to-county travel times in 1930, constructed by Morin and Swisher (2016) using the United States' road network (see Panel B of Figure B.1 in the Internet Appendix). Specifically, we define $Proximity_{cty,1930} \equiv \left[T^{\min}_{cty,1930}\right]^{-1}$, where $T^{\min}_{cty,1930} \equiv \min_{s \in \mathcal{S}} T_{1930}(cty, s)$ is

[10]The measure $O^{MEX}_{c,1930:40}$ can be interpreted as the normalized drop in Mexican workers in city c over the decade. Therefore, higher values of the Mexican outflow measure are associated with greater declines in the Mexican working-age population in city c between 1930 and 1940.

[11]Previous studies have used *ex-ante* immigrant settlement patterns to produce causal estimates of in-migration effects (see Jaeger, Ruist, and Stuhler, 2018). In our context, the IV follows the opposite intuition: regions with more Mexicans experienced greater repatriation pressure from local nationalist groups and authorities.

[12]Indeed, Section 6.2 shows event study evidence supporting the absence of pre-trends.

the shortest 1930 road travel time between county *cty* and any chief border-crossing station $s \in \mathcal{S}$. The instrumental variable is defined as:

$$IV_{cty} = Proximity_{cty,1930} \times \left(\frac{Pop^{MEX}_{cty,1900}}{Pop_{cty,1900}} \right). \quad (6)$$

The first term, $Proximity_{cty,1930}$, is negatively associated with the cost of local authorities to engage in repatriation measures because it was easier and cheaper to encourage (or force) repatriation if people could reach one of the border-crossing stations more quickly.[13] The second term captures the share of Mexican workers in a county in 1900. It is positively related to the share of the Mexican population in 1930, given the persistence of migrant networks; hence, this term is correlated with the size of the population "at risk" of repatriation.

Although IV_{cty} is constructed as the interaction of these two terms, it is worth noting the main source of identifying variation. Nearly all of the cross-sectional IV variation traces to the 1900 Mexican settlement share rather than to proximity, as we discuss in Section 6.1. The practical implication is that the credibility of our research design rests primarily on the exogeneity of the 1900 Mexican settlement pattern with respect to 1930s housing-market trends.

Therefore, conditional on state fixed effects, employment sector shares, and our set of controls, our exclusion restriction requires that the 1900 Mexican settlement share affect the 1930–1940 evolution of local housing markets only through its influence on the intensity of the Mexican repatriation. The three-decade gap between the IV measurement (1900) and our outcome period (1930–1940) makes it less plausible that the turn-of-the-century settlement of Mexicans is mechanically tied to the spatial incidence of the Great Depression or to other contemporaneous housing shocks. In Sections 6.1 and 6.2, we formally test the validity of our instrument and show that it is uncorrelated with the intensity of the Depression and with the out-migration of other nationalities, and that there is no evidence of differential pre-trends.

4.2 Data Sources and Descriptive Statistics

Linked Address Sample. A natural challenge in studying housing markets in the pre-World War II United States is the lack of house-level data measuring the evolution of house prices. We develop a novel automated matching technique (detailed in Internet Appendix D) to link

[13] Historical evidence suggests these border stations were established gateways well before the repatriation. Escamilla-Guerrero (2020) shows that El Paso, Brownsville, Laredo, Nogales, and Douglas accounted for 81% of registered crossings between 1906 and 1908 (see also Romer, 1922).

addresses across the 1930 and 1940 Censuses from the IPUMS Restricted Complete Count Data (Ruggles et al., 2020). The address-matched sample allows us to track the evolution of each housing unit's house values, rents, and inhabitants' characteristics across the 1930 and 1940 censuses. Our sample ends in 1940 to avoid the confounding effects of World War II and the Bracero Program. Our matching procedure relies on matching addresses based on state, city, street name, and house number. Our approach to linking addresses across censuses is similar to previous approaches in the literature that link individuals across censuses using their names (Abramitzky et al., 2021). The first study to match addresses across the 1930 and 1940 Censuses is Akbar et al. (2025), which builds a sample of matched addresses for ten major northern cities. We extend their approach in our algorithm to construct a sample that covers the entire country, offering a more comprehensive spatial coverage of the evolution of house values and rents.

Our full 1930–1940 matched address sample contains 4.03 million linked addresses spanning over 900 U.S. cities. In the Internet Appendix D, we perform a series of sample restrictions to obtain addresses with consistent house values and ownership status across both censuses, further restricting to states where Mexican immigrants constituted at least 0.25% of the urban workforce in 1930.[14]

We collect information on the percentage change in house values, the percentage change in rents, the average age of residents, the total number of residents, the shares of Mexican- and U.S.-born residents, and the homeownership status of each address. Table 1 presents the summary statistics of these variables and compares them across full-count 1930 Census data (column 1), the full address-matched sample (column 2), and the two sub-samples for states more affected by the Mexican repatriation (columns 3 and 4).

— PLACE TABLE 1 ABOUT HERE —

Comparing the full data with our matched sample, we conclude that the average dwelling characteristics are relatively similar across the two datasets. Our matching procedure seems to favor houses with slightly larger shares of U.S.-born residents or houses owned in 1930. The average house value and rents are similar for the two samples. When comparing the state-restricted samples (columns 3 and 4), we observe larger shares of Mexican residents and smaller shares of U.S.-born residents relative to columns 1 and 2. We also observe smaller house values and rents due to the sample restrictions.

[14]The states in this sub-sample are AZ, TX, CA, NM, KS, CO, WY, UT, ID, NE, and IN. This ensures geographic comparability and consistency with our state-level fixed effects and clustering.

City Demographics, Immigration, and Economic Activity. To obtain measures of cities' economic and demographic characteristics, we use data from 868 U.S. cities that we can consistently identify in the 1930 and 1940 full-count U.S. Censuses. We use the IPUMS "City" variable, which is comparable across 1930 and 1940, but does not identify all cities in both censuses. The censuses provide the data used to construct our main variables of immigration flows. They also provide information on economic and demographic characteristics used as controls in our estimations, including city-level averages of workers' age (*Average Age*), school attendance status (*School Attendance*), and the share of unemployed workers among the working-age population (*Unemployed*). The share of *Unemployed* workers and employment *Sector Shares* capture the heterogeneous effects of the Great Depression on housing market outcomes across cities, as they are associated with sector employment composition.

In addition, we use county-level geographical and economic variables as controls. The information comes from the dataset assembled by Fishback, Horrace, and Kantor (2005) and Hornbeck (2012). In the 1930s, the Dust Bowl was a natural disaster that caused hardship in rural American states and induced migration out of the affected areas (Hornbeck, 2012, 2023). To measure and control for the exposure of a county to the Dust Bowl, we use the months of severe drought interacted with the share of farming land to measure the impact on a county of the Dust Bowl from Fishback, Horrace, and Kantor (2005) and the fraction of each county exposed to medium and high permanent soil erosion from Hornbeck (2012). We also use county-level retail sales growth between 1929 and 1935 from Fishback, Horrace, and Kantor (2005) as a proxy for consumption and economic activity.[15] Finally, we use the log of the median house value in 1930 as an additional control to capture the contemporaneous conditions of the housing market.[16]

City-Level Outcomes. To study the consequences of the Mexican repatriation on city-level housing market outcomes, we use the information on house prices from the U.S. Census and aggregate it to the city level. The 1930 and 1940 Censuses include the self-reported values of housing units and rents in nominal U.S. dollars. To reduce the influence of outliers when averaging self-reported variables, we calculate the median house value and rent for each city from the

[15] We opt for not including New Deal spending among our controls. Fishback, Horrace, and Kantor (2005) document that the New Deal endogenously targeted counties with severely deteriorated housing markets *after* the drop in Mexican population. As such, controlling for it would introduce a "bad control" problem in our estimation (Angrist and Pischke, 2009).

[16] Previous studies argued that the mid-1920s real estate boom contributed to the severity of the Great Depression (Goetzmann and Newman, 2010; Brocker and Hanes, 2014; White, 2014; Gjerstad and Smith, 2014). Therefore, the 1930 median house value helps to capture across-city heterogeneity in housing market conditions.

working-age population not living in group quarters.[17] In some specifications, we consider alternative percentiles of the within-city distributions of house values and rents. We convert nominal values to real 1930 U.S. dollars using the Consumer Price Index from the Federal Reserve Bank Database (FRED) of the Federal Reserve Bank of St. Louis.

The self-reported nature of Census housing values requires a careful discussion of the potential for measurement error. Census enumerators recorded house value estimates directly from households, who were asked to state the market value of their property at the time of enumeration (Ruggles et al., 2020). Lyons et al. (2026) provide external validation by comparing owner-reported Census house values with contemporary newspaper sales listings and find that they track each other closely across many cities. This alignment suggests that self-reporting does not systematically distort levels.

Nonetheless, we consider three potential sources of bias specific to our case. First, the 1930 Census was enumerated in April 1930, at the leading edge of the 1930–1933 repatriation peak—a period of intense anti-Mexican mobilization, local raids, and hostile press coverage (Balderrama and Rodríguez, 2006; Hoffman, 1974). If uncertainty about their future in the U.S. depressed Mexican respondents' estimates of what their homes would sell for, their 1930 self-reports would be lower than true market values. By 1940, this acute period had reversed: the war effort buildup was underway (Cortes, Vossmeyer, and Weidenmier, 2024), labor markets were tightening, and the Bracero Program (formalized in 1942) would soon begin actively recruiting Mexican labor again. If any residual pessimism in 1940 was attenuated relative to 1930, the baseline from which we measure the decline would be biased downward. As a result, the decline in value and rents of Mexican-occupied homes we observe would be attenuated toward zero. Second, housing units vacated and left unoccupied by 1940 are excluded from our linked address sample, since Census information is available only for occupied units in both years. Under the reasonable assumption that vacated units experienced the largest price declines, this selection mechanically biases our estimates toward zero. Third, new occupants who moved into previously Mexican-occupied homes report values based on their own assessments; to the extent that non-Mexican buyers are less exposed to neighborhood-specific pessimism than the departing families, they may plausibly report higher values, further attenuating toward zero. Taken together, all three forces push our

[17] Group quarters are places where people live or stay in group-living arrangements. Quarters are usually owned or managed by an organization (e.g., nursing homes, college dorms, military barracks, prisons).

estimates toward zero, so the results we document below should be read as conservative lower bounds on the true effect of the Repatriation on house values.[18]

We also use the number and value of building permits as additional indicators of city-level housing and construction activity, drawing on two distinct series: the value of permits from *Dun & Bradstreet's Review* (see, e.g., Cortes and Weidenmier, 2019; Cortes and LaPoint, 2024) and the number of permits from Snowden (2006). Because the two series are not directly comparable—the number series captures single-family residential units while the value series also includes commercial construction—we rely on Census median house values as our primary house price measure. Full details on these data sources are in Internet Appendix D.

Descriptive Statistics. Table 2 presents sample summary statistics. All growth rates and share variables are presented in percentages. We can see that the average city experienced a decrease in the Mexican population equal to 0.2% of the city's total working-age population.

— Place Table 2 About Here —

Table 2 also reveals substantial heterogeneity. San Benito and El Paso in Texas experienced the two greatest outflows. By 1940, they had lost Mexican inhabitants equivalent to 17.8% (San Benito) and 14.5% (El Paso) of their total population. East Chicago, Indiana, part of Chicago's commuting zone, was the seventh-most-affected city. As the East Chicago example shows, some cities distant from the Mexican border felt a measurable effect of the Mexican repatriation—Simon (1974) estimates that by 1932, over a third of East Chicago's Mexican population had left the city. States on the list of the top 10 most affected cities include Texas, California, Indiana, and Arizona (see Table B.1 in the Internet Appendix). Panels A through D of Figure B.2 in the Internet Appendix illustrate the geographic span of our city-level housing variables. Despite the smaller number of cities shown on the building permit maps, our samples span a significant share of U.S. territory, including the most populous and economically important cities of the period.

5 The 1930s Mexican Repatriation and Housing

We conduct the empirical analysis in three parts. First, we investigate the effects of Mexican repatriation on house-level values and rents, with particular focus on Mexican-occupied

[18] A related concern is that partial move-outs within multi-family buildings may shift the median observed rent compositionally. Internet Appendix Table B.5 shows that our results are robust to restricting the sample to single-household addresses, where this mechanism is ruled out by construction.

housing, which was more exposed to the out-migration shock and for which we expect to observe the largest effects. This allows us to precisely estimate the repatriation effects on the most exposed neighborhoods within each city.

The second part focuses on the effects on neighbors' house values. Leveraging the house-level data, this part provides important results about the consequences of repatriating U.S.-born residents living near Mexican immigrants. Finally, the third part of the analysis investigates the aggregate effects of the repatriation. The goal is to assess how much the Mexican repatriation mattered to city-level housing markets. We close the section by combining the evidence across aggregation levels and probing the mechanisms behind these effects—including changes in homeownership status and resident nativity within Mexican-occupied homes, a housing-supply channel, and a quasi-counterfactual exercise that maps our estimates onto the distribution of U.S. cities under a no-repatriation scenario.

5.1 Effects of the Repatriation at the House Level

Considering the nature of the Mexican repatriation, a local shock due to the out-migration of a specific part of the population, it is plausible to expect that regions within a city were differentially affected, depending on where Mexicans lived. As argued in Section 3, if Mexican immigrants concentrated in some neighborhoods, the expected impact of the Mexican repatriation on housing should be more prominent in those areas.

We begin by estimating the effects of the Mexican repatriation on house values and rents. With the address-matched sample available, we estimate the following house-level specification using two-stage least squares (2SLS) regression analysis with our instrumental variable IV_{cty}:

$$\underset{1930:40}{\Delta} Y_h = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h \tag{7}$$

$\Delta_{1930:40} Y_h$ is a house's 1930–1940 percentage change in its *House Value* or *Rents*. $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 instrumented by IV_{cty} in the city c, where the house is located; λ_s represents state fixed effects to capture state-specific, unobservable heterogeneity (e.g., differences in the intensity of the Great Depression between states); $\mathbf{X}_{h,1930}$ is a set of 1930 controls for house h that include the house-level *Average Age of Residents*, the *Number of Residents*, the *Share of Black Residents*, the *Share of Female Residents*, the house value or rent in 1930, the city-level *Average Age*, average *School Attendance*, the share of *Unemployed* workers, and county-level *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the Dust Bowl disaster. The specifications also include a set of city-level employment *Sector Shares* as a proxy for the economic conditions of cities in 1930. The standard errors are clustered by state.[19]

In studying the effects on individual house values and rents, we separate the sample into different types of houses. We define a Mexican-occupied house as an address where more than half of its residents in 1930 are of Mexican origin. We define a U.S.-born-occupied house as an address where all residents were U.S.-born and not of Mexican descent. Table 3 presents the estimates of the effect of repatriation on the percent changes in house values (columns 1 and 2) or rents (columns 3 and 4) for Mexican-occupied and U.S.-born-occupied houses. The table presents the estimates for Mexican-occupied houses (columns 1 and 3) and U.S.-born-occupied houses (columns 2 and 4). The results show a large and statistically significant effect on Mexican-inhabited houses. We find that houses inhabited by Mexicans in 1930 devalued by 14 percentage points on average between 1930 and 1940 for every 1 percentage-point drop in the city's Mexican population. Houses rented by Mexicans in 1930 also experienced lower rental rates by over eight percentage points for every percentage point of Mexican outflow. For U.S.-born-occupied houses, by contrast, the Mexican outflow coefficient is not statistically significant on average.

— Place Table 3 About Here —

These findings provide compelling evidence that the Mexican repatriation had a substantial and concentrated impact on the housing outcomes of Mexican immigrants. The large and statistically significant effects on house values and rents for Mexican-occupied homes demonstrate the direct economic consequences faced by the targeted population. These results suggest that the repatriation severely affected housing wealth and economic well-being among the Mexican immigrant population in the 1930s U.S., including the non-repatriated families. While we do not find a statistically significant average effect on U.S.-born houses, this result masks substantial heterogeneity by proximity to Mexican homes, which we investigate more closely in the next section.

[19] We cluster standard errors at the state level for two reasons. First, repatriation efforts in the 1930s were often coordinated, administered, and encouraged by state authorities (Balderrama and Rodríguez, 2006; Hoffman, 1974), potentially generating a systematic state-level structure in treatment intensity. Second, state clustering allows arbitrary within-state residual correlation, accommodating common state-level shocks that affected 1930s housing markets—e.g., macroeconomic conditions, regional labor market dynamics, housing regulation (Cameron and Miller, 2015).

5.2 Effects on the neighbors of Mexicans

We now document the spillover on U.S.-born-occupied houses that had at least one Mexican-occupied neighbor in 1930, estimating Eq. (7) separately for U.S.-born houses that were exposed to such neighbors and those that were not.

The main challenge is determining which houses are near each other using historical Census records. Following Logan and Parman (2017), Eriksson and Ward (2019), and Quincy (2022), we use the sequential order in which households appear in the Census enumerator's records to identify next-door *addresses* (see Internet Appendix D for details). This yields two samples: houses with at least one Mexican-occupied next-door neighbor in 1930, and houses with no Mexican immediate neighbors. We estimate Eq. (7) for each sample; results are in Table 4.

— PLACE TABLE 4 ABOUT HERE —

Several insights emerge from Table 4. Column 1 shows that U.S.-born-occupied homes neighboring Mexican residences experienced a 5-percentage-point decrease in value for each 1-percentage-point decline in the city's Mexican population. This statistically and economically significant effect—albeit smaller than that observed in Table 3 for Mexican-occupied homes—suggests the repatriation's impact extended beyond Mexican homes, and also affected nearby U.S.-born households. Conversely, column 2 shows that U.S.-born-occupied homes without Mexican neighbors showed no significant effect from the repatriation. The repatriation's impact was concentrated in areas with larger Mexican populations, with Mexican-occupied homes experiencing the most substantial declines in value. These findings indicate a localized effect of the Mexican repatriation on housing values, primarily affecting areas with significant Mexican presence.

Columns 3 and 4 reveal a different pattern when examining the effects on neighbors' rents. We find no statistically significant impact of the repatriation on rents for either Mexican-occupied or neighboring U.S.-born-occupied homes. Despite a slightly larger magnitude coefficient for rental units with Mexican neighbors, the standard errors are too large to draw strong inferences about rent spillovers. As an alternative to the partitioned-sample design, we also implement an interaction specification that benefits from greater statistical power due to using the full extent of the sample. The interaction estimates of Table B.6 in the Internet Appendix are consistent with the partitioned design in Table 4.

5.3 The Aggregate Consequences of the Repatriation at the City Level

Were the effects of the Mexican repatriation strong enough to impact aggregate city-level housing growth? To answer this, we now examine the aggregate housing market. We construct alternative outcome measures based on the full-count census, thereby eliminating potential biases associated with the sample of linked addresses. Utilizing our established instrument, we estimate the following two-stage least squares (2SLS) regressions:

$$\underset{1930:40}{\Delta} Y_c = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c, \tag{8}$$

where $\Delta_{1930:40} Y_c$ is a city's 1930–1940 growth rate in percentage terms of the housing market outcomes: median reported *House Value*; *Rents*; *Number of Building Permits*; and *Value of Building Permits*; $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the instrumented Mexican outflow as defined before; λ_s represents state fixed effects; and $\mathbf{X}_{c,1930}$ is the set of 1930 city and county level controls we added in the specifications estimated in previous sections, which include the *Average Age*, average *School Attendance*, share of *Unemployed* workers, employment *Sector Shares*, and county-level *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. All regressions are weighted by the city's working-age population in 1930.[20] Standard errors are clustered by state.

— Place Table 5 About Here —

Table 5 presents the estimates of the effect of repatriation on the growth rate of the number of building permits (columns 1–2); the effect on the growth rate of the value of building permits (columns 3–4); the effect on the growth rate of cities' median house value (columns 5–6); and the effect on the growth rate of cities' median rent (columns 7–8). Within each real estate outcome in Table 5, the first-ordered columns (columns 1, 3, 5, and 7) show the weighted least squares (WLS) estimates. The second-ordered columns (columns 2, 4, 6, and 8) present 2SLS results.

The results reveal a negative and statistically significant relationship between the Mexican outflow and all the housing market outcomes. Specifically, a one standard deviation increase in exposure to the Mexican outflow (1 percentage point) leads to a 1.79 percentage point decrease in the average growth rate of the median house value (column 6) and a 1.25 percentage point decrease in the median rent (column 8) growth rate between 1930 and 1940. These are relatively

[20]Following Solon, Haider, and Wooldridge's (2015) recommendation, a Breusch–Pagan test reveals significant heteroskedasticity across our outcome variables. Weighting by population corrects for this and improves efficiency.

large compared to the baseline growth rates of these variables in the period. On average, median house values decreased 20.5% while median rents decreased 4.6% between 1930 and 1940. Therefore, a one standard deviation increase in exposure to the Mexican repatriation explains approximately 8.7% and 27% of the average decrease in house values and rents, respectively. These effects are significant across different percentiles of the house value and rent distributions (Section B.2) and are robust to different sample restrictions (Section B.3), as shown in the Internet Appendix. We also find that the repatriation negatively affected the growth rate of building permits. An increase of one standard deviation in the exposure to the Mexican outflow (1 percentage point) decreased the average growth rates of the value of permits (5.4 percentage points) and the number of building permits (16 percentage points).

5.4 Discussion and Economic Magnitudes

We now combine our evidence across the three levels of aggregation, discuss the mechanisms consistent with the observed patterns, and translate our estimates into aggregate city-level economic magnitudes through a quasi-counterfactual exercise.

5.4.1 Summary of Results by Aggregation Level

To put our findings from all sections into perspective, Figure 6 combines the estimates from our baseline specifications across aggregation levels. Panel A shows the impacts of the Mexican Repatriation on house values, while Panel B shows the estimated coefficients for rents. The effect on house values increases monotonically with the level of aggregation, with the greatest impact concentrated among Mexican houses. The rent results suggest a similar relationship, except for the neighborhood-level aggregation. Taken together, our results confirm that the Mexican repatriation had a strong negative impact on local housing markets, primarily in addresses predominantly occupied by Mexican immigrants.

Previous studies using IV approaches focusing on the city-level impact of immigrant inflows have found that, on average, for every percent increase in the immigrant population, house prices increase by 1% (Saiz, 2007), 0.66% (Akbari and Aydede, 2012), and 0.5% (Accetturo et al., 2014). Our results suggest that the impact of out-migration flows is larger, in absolute terms, than the in-migration estimates. This result is consistent with the kinked supply curve idea in Glaeser and Gyourko (2005) and underscores the asymmetric responses of immigrant inflows and outflows

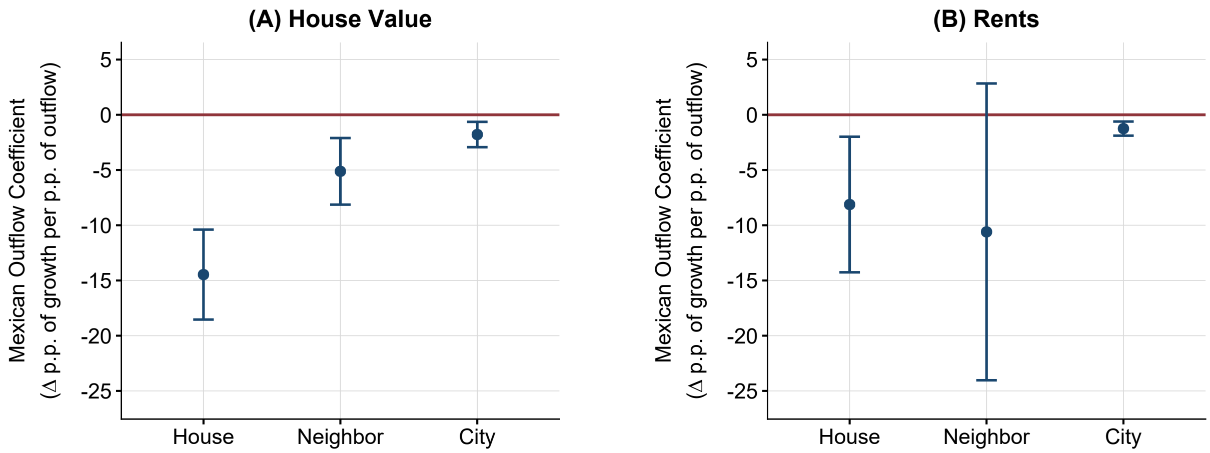


Figure 6. Summary of results by aggregation level. This figure summarizes the estimated effects (and 95% confidence intervals) of the *Mexican Outflow* on the 1930–1940 growth of *House Values* (Panel A) and *Rents* (Panel B) at three different levels of aggregation. *House* reports the address-level 2SLS estimate on Mexican-occupied dwellings from Table 3; *Neighbor* reports the spillover estimate on U.S.-born households living next door to a Mexican-occupied house from Table 4; and *City* reports the city-level 2SLS estimate from Table 5. Both panels share a common y-axis whose units are percentage points of decade outcome growth per percentage point of *Mexican Outflow* (Δ p.p. of growth per p.p. of outflow). Negative estimates indicate that more intense repatriation depressed local housing markets. All specifications instrument the *Mexican Outflow* with IV_{cty} (defined in Eq. (6)), include the baseline set of controls, sector employment shares, and state fixed effects, and cluster standard errors at the state level.

Alt text: Coefficient plot in two panels, House Value (A) and Rents (B), each with point estimates and confidence intervals for House, Neighbor, and City level estimates.

in housing markets. Moreover, the larger magnitudes of house-value coefficients relative to rents suggest that the repatriation also increased the user cost of homeownership in affected cities.

How do the house-level and city-level estimates compare? As a back-of-the-envelope aggregation, we weight the house-level coefficients by their sample shares of affected dwellings. The linked-sample shares of Mexican-occupied dwellings (Table 3) and adjacent U.S.-born dwellings (Table 4) (2.62% and 1.75%, respectively) imply direct aggregate effects of −0.47% for house values and −0.21% for rents per percentage point of Mexican outflow, compared with city-level estimates of −1.79% and −1.25%.[21] These weights, however, understate the population share of Mexican-occupied dwellings, since vacated and demolished properties are disproportionately dropped during linking. Re-weighting with full-count 11-state shares (3.72% Mexican-occupied and 2.12% U.S.-born with a Mexican neighbor) raises the implied effects to −0.65% for values and −0.30% for rents. The house-level estimates thus recover roughly one-quarter to one-third of the aggregate effect on house values, and one-sixth to one-quarter of the effect on rents.

While these aggregations capture a substantial portion of the estimated city-level effects, the house-level specifications isolate direct effects on specific subsets of addresses within

[21] The neighbor coefficient on rents is not statistically significant. Therefore, we set it to zero in this calculation.

cities. Other portions of the housing market are also plausibly affected by repatriation but harder to isolate empirically—for instance, properties that switched tenure status over the decade, or U.S.-born dwellings subject to spatial spillovers beyond their immediate next-door neighbor. The city-level specification captures these subgroups together with general-equilibrium responses that the matched design cannot identify, which explains why the city-level estimates exceed the back-of-the-envelope aggregations.

5.4.2 Did the Repatriation affect Homeownership Status and Resident Nativity?

The results show a large and significant effect of Mexican repatriation on house values and rents. However, the Mexican repatriation could have affected the housing landscape beyond prices, reshaping patterns of homeownership and resident composition. By studying these dynamics, we can uncover the more profound structural changes in affected communities and test competing hypotheses about the long-term effects of repatriation. We investigate these impacts using our matched address sample, estimating the following house-level specifications:

$$\underset{1930:40}{\Delta} y_h = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c} \left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h, \tag{9}$$

where $\underset{1930:40}{\Delta} y_h$ captures two measures of property occupancy change. First, as *Ownership Status Change*, it equals one if a home's tenure shifted from owner-occupied to rented between 1930 and 1940, or *vice-versa*. In the first specification, γ captures the average effect among all units occupied by Mexican immigrants in 1930. A positive γ would indicate that the repatriation led to greater changes in the tenure status of properties, signaling shifts in the optimal tenure choice.

Second, we also consider $\underset{1930:40}{\Delta} y_h$ defined as *Resident Nativity Change*, an indicator variable that equals one if a home transitioned from Mexican-occupied in 1930 to U.S.-born occupied in 1940, and zero otherwise. In this case, a positive γ would indicate that the repatriation allowed the U.S.-born population to occupy previously Mexican homes at lower prices, suggesting greater housing affordability for the local native population.

To explore potential heterogeneous effects from different tenure statuses, we also estimate a conditional specification, allowing us to further disentangle these effects:

$$\begin{aligned} \underset{1930:40}{\Delta} y_h = \alpha &+ \gamma_1 \cdot \tilde{O}^{MEX}_{1930:40,c} \left(IV_{cty}\right) \times \mathbb{1}^{Homeowner}_{h,1930} \\ &+ \gamma_2 \cdot \tilde{O}^{MEX}_{1930:40,c} \left(IV_{cty}\right) \times \mathbb{1}^{Renter}_{h,1930} + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h, \end{aligned} \tag{10}$$

where γ_1 captures the repatriation's impact on previously owner-occupied homes, while γ_2 does the same for rental properties. This decomposition allows us to measure whether the repatriation affected owner-occupied and rental properties differently, revealing potential disparities across socioeconomic groups within the Mexican immigrant community. Table 6 reports the results.

— Place Table 6 About Here —

Columns (1) and (2) show that, for houses inhabited by Mexicans in 1930, the repatriation caused a higher probability of ownership status change, with the effects concentrated among the renter-occupied homes. In other words, houses rented by Mexicans in 1930 were more likely to be converted to owner-occupied units in response to the repatriation. We find no evidence of owned units converting into renter-occupied. To test if these effects were common to all properties, columns (3) and (4) estimate similar specifications but on U.S.-born occupied homes. They show no evidence of such an effect on U.S.-born homeowners. The results suggest that repatriation induced a *lower* probability of U.S.-born units being converted to a different tenure status. These results suggest that the repatriation led to a lower demand for rental housing, particularly in areas inhabited by the Mexican immigrant population. These results could also suggest that the repatriation altered the trade-offs in residents' optimal tenure choices (Henderson and Ioannides, 1983).

Columns (5) and (6) of Table 6 show the results for resident nativity changes. We restricted the sample to the Mexican-inhabited houses in 1930. We find a negative and statistically significant coefficient for Mexican repatriation on the probability that a Mexican-inhabited house becomes U.S.-born-occupied. The effect is stronger for rented houses. From column (5), a 1 percentage-point Mexican outflow is associated with a 1.5 percentage-point decrease in the probability that Mexican houses were fully inhabited by U.S.-born households by 1940. In other words, cities with higher repatriation saw a lower probability that previously Mexican-occupied properties would be converted to U.S.-born occupancy.

Our findings on changes in resident nativity paint a nuanced picture of how the Mexican repatriation affected neighborhood dynamics. Despite the significant effects on house prices and rents in Mexican enclaves, it did not lead to widespread integration or increased occupancy of housing units previously occupied by Mexican immigrants by U.S.-born residents. Instead, cities with higher repatriation rates were less likely to see Mexican-inhabited homes become occupied by U.S.-born residents, suggesting a pattern of persistent segregation and disinvestment in these neighborhoods. This outcome could be reflective of an entrenched anti-immigrant sentiment that drove the repatriation efforts, which may have also shaped U.S.-born residents' preferences to avoid living in areas associated with Mexican immigrant communities. Consequently, while the repatriation substantially affected housing costs, it did not necessarily translate into improved access to affordable housing for U.S.-born residents due to persistent social and cultural barriers—reinforced by the repatriation campaign. These results align well with Lee, Peri, and Yasenov's (2022) findings that U.S.-born residents did not occupy jobs left by Mexican immigrants following the repatriation.

Our house-level analysis provides some insight into the channels through which the repatriation affected housing markets. The larger effects on house values and rents for Mexican-occupied houses (Table 3) and the spillover effects on U.S.-born neighbors of Mexican houses (Table 4) suggest that the decline in housing demand was driven both by the direct effect of Mexican households leaving and potential spillovers to nearby U.S.-born households. These results, combined with the patterns identified in Table 6 with the repatriation being associated with lower probabilities of a change in the resident type from Mexican to U.S.-born, are consistent with our theoretical predictions in the presence of high residential segregation for Mexican immigrants living in the United States.

5.4.3 Was there a Housing Supply Channel?

Another mechanism by which the Mexican repatriation could have affected house prices is through changes in housing supply. To assess this channel, we estimate our baseline 2SLS specification with the city-level change in construction employment between 1930 and 1940 as the outcome, separately for three groups of workers: Mexicans, U.S.-born, and total employment. We consider two complementary definitions of the construction sector: a *narrow* definition restricted to the construction industry itself, and a *broad* definition that additionally includes construction-related durable-manufacturing industries, such as logging, wood products, furniture, glass, cement, structural clay, pottery, primary iron and steel, and fabricated metal products. All specifications include our standard 1930 city-level controls, state fixed effects, and 16 employment-sector shares. The results, reported in Table B.3 of the Internet Appendix, show that the repatriation reduced Mexican employment in construction and that U.S.-born employment in construction increased proportionally. However, overall employment growth in the construction sector was not differentially impacted by the repatriation.

Furthermore, our findings on the negative effects of repatriation on building permits (Table 5) provide insights into changing expectations about future housing market conditions. The reduction in building permit activity in areas more exposed to repatriation suggests that the repatriation episode likely affected expectations about future housing demand and prices—which are crucial forces behind housing price cycles (Kaplan, Mitman, and Violante, 2020).

5.4.4 Economic Magnitudes: A Quasi-Counterfactual Exercise

To gauge the economic magnitudes of these effects across the distribution of U.S. cities, we conduct a quasi-counterfactual exercise in the spirit of Burchardi, Chaney, and Hassan (2018). We use the first-stage coefficient to construct a hypothetical Mexican outflow in the absence of the repatriation and, in turn, apply the 2SLS estimates from Table 5 to recover, for each city, the predicted 1930–1940 growth rates of the value of building permits, median house values, and median rents under a no-repatriation scenario. The exercise is not a formal counterfactual—it abstracts from general-equilibrium responses—but it offers a transparent, reduced-form way to translate our estimates into city-level magnitudes. We describe the derivation in detail in Section B.6 of the Internet Appendix.

Figure 7 summarizes the results. The maps on the left show that the predicted gains in growth rates under the no-repatriation scenario are strongly concentrated in cities near the U.S.–Mexico border, where both the 1900 Mexican settlement share and proximity to the border magnify the instrument. The bar graphs on the right focus on the ten cities with the largest observed Mexican outflow between 1930 and 1940. For instance, El Paso shows a predicted no-repatriation growth rate of building-permit values (Panel A) of 0.51 percentage points higher, almost fully offsetting its observed decline; the median house-value decline (Panel B) would be roughly cut in half under the same counterfactual. The pattern is similar—though of smaller magnitude—for cities farther from the border. Taken together, these results reinforce our baseline estimates: the 1930s Mexican repatriation materially depressed local housing-market growth, especially in the cities most exposed to the shock.

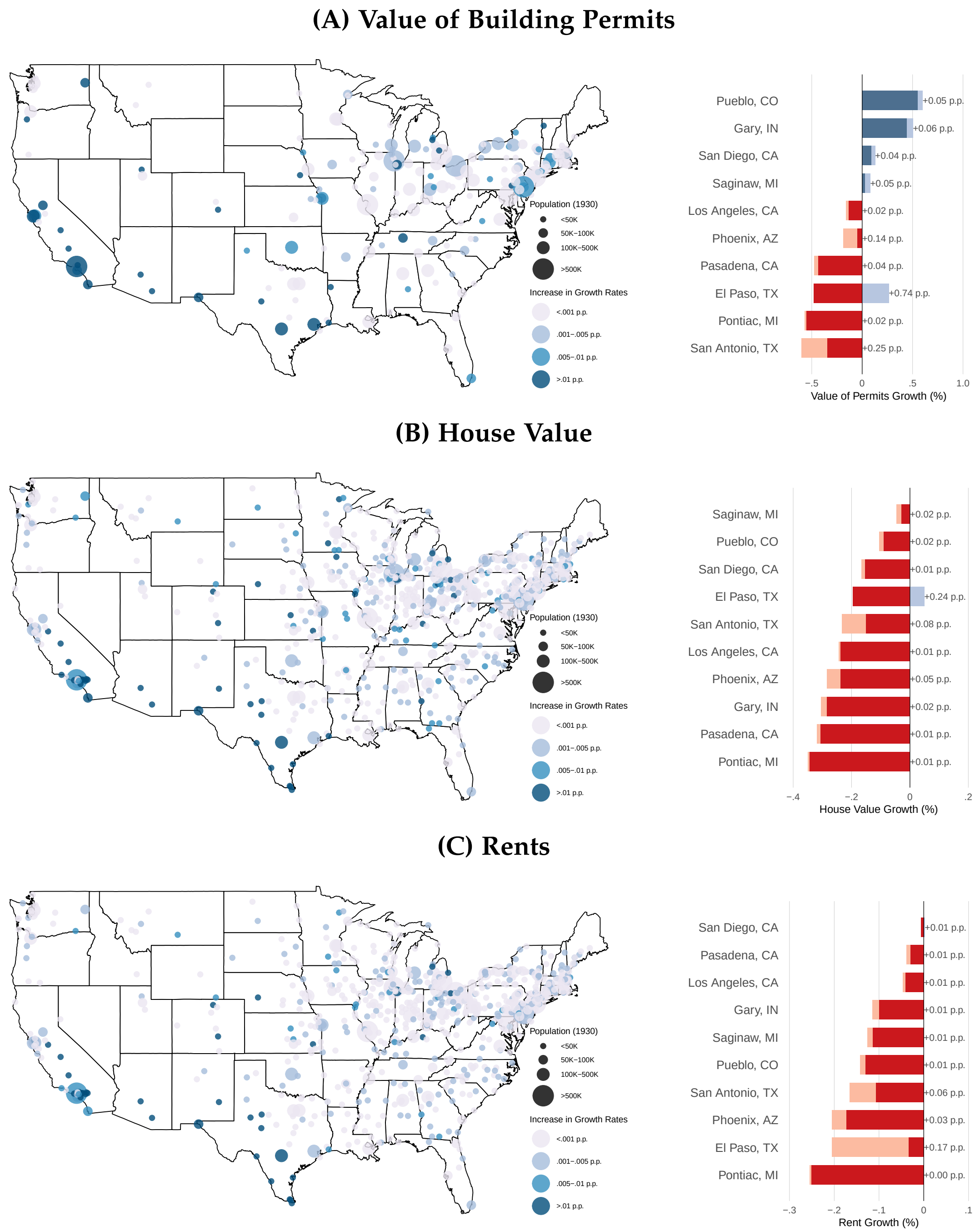


Figure 7. Real estate outcomes without the Mexican repatriation. This figure summarizes the quasi-counterfactual estimates for the housing market outcomes of interest: value of building permits (Panel A), house value (Panel B), and rents (Panel C). We omit the number of building permits, as the estimated coefficients are not statistically significant in Table 5. The maps depict the predicted increase in the growth rates of the city's respective real estate outcome, as detailed in Section B.6. The size of each bubble is proportional to the city's working-age population in 1930. Darker shades of blue represent greater predicted increases in percentage points. The bar graphs show the top 10 cities with the highest Mexican outflow between 1930 and 1940, with non-missing information on all outcome variables. For cities with negative growth rates (red bars), the counterfactual (depicted in light red) cancels out the observed decline (depicted in dark red). For cities with positive growth rates (blue bars), the counterfactual (in light blue) increments the observed growth rate (in dark blue).

Alt text: Three panels (permit value A, house value B, rents C), each pairing a U.S. bubble map on the left with a bar chart of the top-10 outflow cities on the right.

6 Identification Validity and Robustness

6.1 Instrument Validity

In this section, we present standard tests to validate our constructed instrument. The first-stage regressions are reported in Section C.1 of the Internet Appendix: at both the address and city levels, our instrument predicts Mexican outflow with a positive coefficient that is significant at the 1% level, and the Kleibergen–Paap F-statistics are large, alleviating concerns about weak-instrument bias. In Section C.2, we perform a Sargan–Hansen J overidentification test that uses both components of our instrument separately (per Goldsmith-Pinkham, Sorkin, and Swift, 2020). Our overidentification tests fail to reject the null, suggesting that the parameter estimates are similar across estimators and further validating our instrumental variable approach.

We next ask whether the instrument satisfies the exclusion restriction. This requires that IV_{cty} be uncorrelated with unobserved factors that drove changes in the 1930–1940 housing market, after conditioning on demographic and economic controls, state fixed effects, and employment sector shares. We bring two complementary pieces of evidence to bear on this requirement, summarized in Table 7.

First, Table 7 Panel A tests whether IV_{cty} is mechanically correlated with the intensity of the Great Depression. Our motivation for using the shift-share instrument is precisely that it should not inherit the Depression's spatial heterogeneity. We estimate:

$$\Delta GD_c = \alpha + \beta \cdot IV_{cty} + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c, \tag{11}$$

where ΔGD_c is one of four measures of the intensity of the Great Depression in city c: the 1930–1940 change in the total number of unemployed workers; the change in the number of unemployed workers in agriculture; the change in the number of unemployed workers in non-agriculture (each normalized by the city's working-age population in 1930); and the 1929–1935 growth rate of retail sales in the county where city c is located, from Fishback, Horrace, and Kantor (2005). The coefficient on IV_{cty} is small and statistically indistinguishable from zero across all four measures, consistent with the exclusion restriction.

Second, Table 7 Panel B asks whether IV_{cty} predicts the outflow of immigrants from other nationalities or exposure to the 1920s immigration quotas. A non-null relationship here would raise concerns that our instrument is capturing broader immigration-related dynamics rather than a Mexican-specific shock. We choose three nationalities: *Latin Americans other than Mexicans* (cultural and occupational similarity to Mexicans); *Canadians* (also a U.S. border country); and *Asians* (the first group targeted by race- and country-based U.S. immigration restrictions). We also include the two quota-exposure measures (*Indicator* and *Intensity*) from Abramitzky et al. (2023) as dependent variables. While IV_{cty} is strongly positive and significant for the Mexican outflow (column 1, replicating the first stage), none of the coefficients for the other five outcomes is statistically significant, a reassuring result that the instrument is not contaminated by unobserved factors driving non-Mexican migration flows or by European quota exposure.

— PLACE TABLE 7 ABOUT HERE —

Our estimates are also robust to controlling for other major events of the time, such as the 1920s immigration quotas (Abramitzky et al., 2023), the first wave of the Great Migration (Boustan, 2010), and residential racial segregation (Logan and Parman, 2017) (see Table C.4 of Section C.4 in the Internet Appendix).

A natural question with the shift-share construction is whether identification comes from the interaction IV_{cty} *per se* or from one of its components alone. As previewed in Section 4.1, in our sample IV_{cty} is nearly collinear with the 1900 Mexican settlement share (correlation 0.988) and only weakly correlated with proximity (0.181). This asymmetry has a direct implication for robustness. Controlling linearly for the settlement share absorbs nearly all of the instrument's identifying variation—mechanically weakening the first stage—while controlling for proximity does not (cf. Goldsmith-Pinkham, Sorkin, and Swift, 2020).[22] The validity of our design, therefore, rests on the exogeneity of the 1900 Mexican settlement pattern relative to 1930s housing-market trends—a plausible assumption given the three-decade interval and the evidence amassed in Table 7.

[22]We document this empirically in Internet Appendix Table C.3. Panel A is the baseline with no main-effect controls (Kleibergen–Paap $F = 239$); Panel B adds proximity linearly ($F = 308$); Panel C adds proximity plus flexible dummies for non-zero and above-median 1900 Mexican settlement share ($F = 301$); and Panel D adds both main effects linearly ($F = 48$). Across Panels A–C, where the instrument retains its identifying variation, the 2SLS coefficients on median house value growth and median rent growth are stable and significant at the 1% level. Panel D, which controls linearly for both main components, illustrates the collinearity bound: point estimates remain negative and larger in absolute value, but standard errors inflate as the first stage collapses. Panel D is therefore not evidence against our baseline; it is a diagnostic that the instrument has been fully absorbed by the linear controls.

6.2 Pre-trends Assessment

A remaining concern with our identification strategy is that the 1930 share of Mexican workers may be associated with pre-existing trends in socioeconomic or housing market conditions. Because the U.S. Census did not collect housing prices or rents before 1930, we cannot construct a direct event study of house values. We address this concern through two complementary exercises in Internet Appendix Section C.5.

First, we use annual city-level building permits, 1921–1940, as a proxy for housing market conditions, and estimate an event-study specification:

$$\underset{t-1:t}{\Delta} Y_c = \alpha + \sum_{t=1921}^{1940} \beta_t \cdot \left(\frac{Pop^{MEX}_{c,1930}}{Pop_{c,1930}} \right) + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c, \tag{12}$$

where $\Delta_{t-1:t} Y_c$ is a city's annual growth rate in either the number or the value of building permits. Figure C.1 in the Internet Appendix plots the estimated β_t with 95% confidence intervals. The pre-1929 coefficients show no systematic pattern or trend, supporting our identification assumption. The negative effect of the 1930 Mexican share on permit growth emerges in 1930–1934 precisely when the repatriation was most intense. A temporary positive rebound in 1935–1936, plausibly reflecting New Deal housing-support policies (Courtemanche and Snowden, 2011; Fishback et al., 2011; Fishback, 2017), works against our decennial estimates and suggests they are a lower bound on the repatriation's true effect.

Second, we re-estimate our baseline specifications at all three levels of aggregation with additional controls for pre-repatriation non-Mexican population growth (decennial changes for 1900–1910, 1910–1920, 1920–1930, and the long-term 1900–1930 change). The results, reported in Table C.5 of the Internet Appendix, remain consistent and significant. The only substantive change is in Panel A's rent coefficient for Mexican-occupied houses, which *increases* in absolute value after conditioning on pre-trends, indicating that our baseline estimates are, if anything, conservative. Taken together, neither exercise shows evidence that our findings are driven by differential pre-trends in building permits or non-Mexican population growth.

7 Concluding Remarks

The Mexican repatriation of the 1930s profoundly impacted U.S. housing markets, extending beyond direct effects on values and rents. Our study reveals significant shifts in the ownership structure of Mexican-inhabited homes, persistent residential segregation, and negative spillover effects on the property values of nearby U.S.-born residents. Houses inhabited by Mexicans in 1930 devalued by 14 percentage points between 1930 and 1940 for every 1 percentage-point drop in a city's Mexican population, while rents fell by over 8 percentage points. Moreover, repatriation decreased growth rates of cities' median house values, rents, and building permit values. These findings challenge the notion that repatriation created new housing opportunities for U.S.-born households and illustrate how forced migrations can significantly impact local economies beyond labor markets.

Our findings are a cautionary tale for policymakers in advanced and emerging economies pursuing mass-deportation policies. Implementing large-scale repatriation can be costly to housing markets, with substantial negative effects concentrated in areas where targeted immigrant groups lived and no apparent benefit to local native residents. Repatriations have a long-lasting impact not only on cultural and social dimensions but also on economic activity and urban spaces, affecting various types of real estate activity, including single-family homes and commercial construction.

Given a renewed escalation of political rhetoric against immigration, our study of the 1930s Mexican repatriation provides valuable lessons for modern immigration policies. While historical circumstances differ, the fundamental mechanisms through which large-scale out-migration affects housing markets remain relevant. As policymakers conceive large-scale deportation programs, they should consider the potential unintended consequences on local housing markets and community stability against any perceived benefits. Our study underscores the importance of comprehensive policy analysis that considers the wide-ranging effects of immigration decisions on all aspects of society. It demonstrates that the impacts of large-scale repatriation extend far beyond the targeted population, affecting housing markets, community structures, and local economies in complex and often unforeseen ways. Finally, our paper opens avenues for future research to explore the long-term intergenerational effects of the 1930s repatriation on wealth accumulation and homeownership rates among Mexican-American families.

Data Availability

The data and code replicating the results and figures in this article can be found in the Harvard Dataverse, https://doi.org/10.7910/DVN/V3XDDS_2026 (Cortes and Sant'Anna, 2026).

References

Abramitzky, Ran, Philipp Ager, Leah Platt Boustan, Elior Cohen, and Casper Worm Hansen. 2023. "The Effects of Immigration on the Economy: Lessons from the 1920s Border Closure." *American Economic Journal: Applied Economics* 15 (1):164–191.

Abramitzky, Ran, Leah Platt Boustan, Katherine Eriksson, James Feigenbaum, and Santiago Pérez. 2021. "Automated Linking of Historical Data." *Journal of Economic Literature* 59 (3):865–918.

Accetturo, Antonio, Francesco Manaresi, Sauro Mocetti, and Elisabetta Olivieri. 2014. "Don't Stand So Close to Me: The Urban Impact of Immigration." *Regional Science and Urban Economics* 45:45–56.

Akbar, Prottoy A., Sijie Li Hickly, Allison Shertzer, and Randall P. Walsh. 2025. "Racial Segregation in Housing Markets and the Erosion of Black Wealth." *Review of Economics and Statistics* 107 (1):42–54.

Akbari, Ather H. and Yigit Aydede. 2012. "Effects of Immigration on House Prices in Canada." *Applied Economics* 44 (13):1645–1658.

Albert, Christoph and Joan Monras. 2022. "Immigration and Spatial Equilibrium: The Role of Expenditures in the Country of Origin." *American Economic Review* 112 (11):3763–3802.

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva. 2023. "Immigration and Redistribution." *Review of Economic Studies* 90 (1):1–39.

Alesina, Alberto and Marco Tabellini. 2024. "The Political Effects of Immigration: Culture or Economics?" *Journal of Economic Literature* 62 (1):5–46.

Andersson, Henrik, Heléne Berg, and Matz Dahlberg. 2021. "Migrating Natives and Foreign Immigration: Is There a Preference for Ethnic Residential Homogeneity?" *Journal of Urban Economics* 121:103296.

Andries, Marianne, Leonardo Bursztyn, Thomas Chaney, and Milena Djourelova. 2023. "In Their Shoes." Working Paper, University of Southern California.

Ang, Amanda, Eunjee Kwon, and Siqi Zheng. 2026. "Asian Immigrants, School Quality, and the US Housing Market." *Journal of Urban Economics* 153:103860.

Angrist, Joshua D. and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Baerlocher, Diogo, Gustavo S. Cortes, and Vinicios P. Sant'Anna. 2025. "Finding Home When Disaster Strikes: Dust Bowl Migration and Housing in Los Angeles." Working Paper, University of South Florida.

Balderrama, Francisco E. and Raymond Rodríguez. 2006. *Decade of Betrayal: Mexican Repatriation in the 1930s*. Albuquerque, NM: The University of New Mexico Press.

Becker, Sascha O. and Andreas Ferrara. 2019. "Consequences of Forced Migration: A Survey of Recent Findings." *Labour Economics* 59:1–16.

Benhabib, Jess. 1996. "On the Political Economy of Immigration." *European Economic Review* 40 (9):1737–1743.

Bennett, David Harry. 1969. *Demagogues in the Depression: American Radicals and the Union Party, 1932-1936*. New Brunswick, NJ: Rutgers University Press.

Boje-Kovacs, Bence, Ismir Mulalic, Albert Saiz, Vinicios P. Sant'Anna, and Marie Louise Schultz-Nielsen. 2024. "Immigrants and Native Flight: Geographic Extent and Heterogeneous Preferences." MIT Center for Real Estate Research Paper 24/08, Massachusetts Institute of Technology.

Borjas, George J. and Joan Monras. 2017. "The Labour Market Consequences of Refugee Supply Shocks." *Economic Policy* 32 (91):361–413.

Boustan, Leah Platt. 2010. "Was Postwar Suburbanization "White Flight"? Evidence from the Black Migration." *Quarterly Journal of Economics* 125 (1):417–443.

Brocker, Michael and Christopher Hanes. 2014. "The 1920s American Real Estate Boom and the Downturn of the Great Depression: Evidence from City Cross-Sections." In *Housing and Mortgage Markets in Historical Perspective*, edited by Eugene N. White, Kenneth Snowden, and Price V. Fishback. Chicago: University of Chicago Press, 161–202.

Burchardi, Konrad B., Thomas Chaney, and Tarek A. Hassan. 2018. "Migrants, Ancestors, and Foreign Investments." *Review of Economic Studies* 86 (4):1448–1486.

Bursztyn, Leonardo, Thomas Chaney, Tarek Alexander Hassan, and Aakaash Rao. 2024. "The Immigrant Next Door: Long-Term Contact, Generosity, and Prejudice." *American Economic Review* 114 (2):348–384.

Bursztyn, Leonardo, Georgy Egorov, Ingar Haaland, Aakaash Rao, and Christopher Roth. 2022. "Scapegoating during Crises." *AEA Papers and Proceedings* 112:151–155.

Cameron, Colin A. and Douglas L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50 (2):317–372.

Campbell, John Y., Stefano Giglio, and Parag Pathak. 2011. "Forced Sales and House Prices." *American Economic Review* 101 (5):2108–2131.

Card, David. 1990. "The Impact of the Mariel Boatlift on the Miami Labor Market." *Industrial and Labor Relations Review* 43 (2):245–257.

———. 2009. "Immigration and Inequality." *American Economic Review* 99 (2):1–21.

Chaney, Eric and Richard Hornbeck. 2016. "Economic Dynamics in the Malthusian Era: Evidence from the 1609 Spanish Expulsion of the Moriscos." *Economic Journal* 126 (594):1404–1440.

Cikara, Mina, Vasiliki Fouka, and Marco Tabellini. 2022. "Hate Crime towards Minoritized Groups Increases as They Increase in Sized-Based Rank." *Nature Human Behaviour* 6 (11):1537–1544.

Clemens, Michael A., Ethan G. Lewis, and Hannah M. Postel. 2018. "Immigration Restrictions as Active Labor Market Policy: Evidence from the Mexican Bracero Exclusion." *American Economic Review* 108 (6):1468–87.

Cooke, Timothy W. and Bruce W. Hamilton. 1984. "Evolution of Urban Housing Stocks: A Model Applied to Baltimore and Houston." *Journal of Urban Economics* 16 (3):317–338.

Cortes, Gustavo S. and Cameron LaPoint. 2024. "Housing Is the Financial Cycle: Evidence from 100 Years of Local Building Permits." Working Paper, University of Florida and Yale University.

Cortes, Gustavo S. and Vinicios P. Sant'Anna. 2026. "Replication Data for: Send Them Back? The Real Estate Consequences of Repatriations." Harvard Dataverse. https://doi.org/10.7910/DVN/V3XDDS.

Cortes, Gustavo S., Angela Vossmeyer, and Marc D. Weidenmier. 2024. "Stock Volatility and the War Puzzle: The Military Demand Channel." NBER Working Paper 29837, National Bureau of Economic Research.

Cortes, Gustavo S. and Marc D. Weidenmier. 2019. "Stock Volatility and the Great Depression." *Review of Financial Studies* 32 (9):3544–3570.

Cortes, Patricia. 2008. "The Effect of Low-Skilled Immigration on US Prices: Evidence from CPI Data." *Journal of Political Economy* 116 (3):381–422.

Courtemanche, Charles and Kenneth Snowden. 2011. "Repairing a Mortgage Crisis: HOLC Lending and Its Impact on Local Housing Markets." *Journal of Economic History* 71 (2):307–337.

Derenoncourt, Ellora. 2022. "Can You Move to Opportunity? Evidence from the Great Migration." *American Economic Review* 112 (2):369–408.

Enciso, Fernando Saúl Alanís. 2017. *They Should Stay There: The Story of Mexican Migration and Repatriation During the Great Depression*. University of North Carolina Press.

Eriksson, Katherine and Zachary Ward. 2019. "The Residential Segregation of Immigrants in the United States from 1850 to 1940." *Journal of Economic History* 79 (4):989–1026.

Escamilla-Guerrero, David. 2020. "Revisiting Mexican Migration in the Age of Mass Migration: New Evidence from Individual Border Crossings." *Historical Methods* 53 (4):207–225.

Feigenbaum, James J. 2016. "Automated Census Record Linking: A Machine Learning Approach." Working Paper, Boston University.

Feigenberg, Benjamin. 2020. "Fenced Out: The Impact of Border Construction on US-Mexico Migration." *American Economic Journal: Applied Economics* 12 (3):106–139.

Ferrara, Andreas and Price V. Fishback. 2024. "Discrimination, Migration, and Economic Outcomes: Evidence from World War I." *Review of Economics and Statistics* 106 (5):1201–1219.

Fishback, Price V. 2017. "How Successful was the New Deal? The Microeconomic Impact of New Deal Spending and Lending Policies in the 1930s." *Journal of Economic Literature* 55 (4):1435–85.

Fishback, Price V., Alfonso Flores-Lagunes, William C. Horrace, Shawn Kantor, and Jaret Treber. 2011. "The Influence of the Home Owners' Loan Corporation on Housing Markets During the 1930s." *Review of Financial Studies* 24 (6):1782–1813.

Fishback, Price V., William C. Horrace, and Shawn Kantor. 2005. "Did New Deal Grant Programs Stimulate Local Economies? A Study of Federal Grants and Retail Sales During the Great Depression." *Journal of Economic History* 65 (3):36–71.

Fishback, Price V. and Trevor Kollmann. 2014. "New Multicity Estimates of the Changes in Home Values, 1920–1940." In *Housing and Mortgage Markets in Historical Perspective*, edited by Eugene N. White, Kenneth Snowden, and Price V. Fishback. Chicago: University of Chicago Press, 203–244.

Gilbukh, Sonia, Andrew Haughwout, Rebecca J. Landau, and Joseph Tracy. 2023. "The Price-to-Rent Ratio: A Macroprudential Application." *Real Estate Economics* 51 (2):503–532.

Gjerstad, Steven D. and Vernon L. Smith. 2014. *Rethinking Housing Bubbles: The Role of Household and Bank Balance Sheets in Modeling Economic Cycles*. Cambridge University Press.

Glaeser, Edward L. and Joseph Gyourko. 2005. "Urban Decline and Durable Housing." *Journal of Political Economy* 113 (2):345–375.

Goetzmann, William N. and Frank Newman. 2010. "Securitization in the 1920s." NBER Working Paper 15650, National Bureau of Economic Research.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik Instruments: What, When, Why, and How." *American Economic Review* 110 (8):2586–2624.

Gratton, Brian and Emily Merchant. 2013. "Immigration, Repatriation, and Deportation: The Mexican-Origin Population in the United States, 1920–1950." *International Migration Review* 47 (4):944–975.

Greulich, Erica, John M. Quigley, Steven Raphael, Joseph Tracy, and Guillermina Jasso. 2004. "The Anatomy of Rent Burdens: Immigration, Growth, and Rental Housing." *Brookings-Wharton Papers on Urban Affairs* :149–205.

Guerin-Gonzales, Camille. 1994. *Mexican Workers and American Dreams: Immigration, Repatriation, and California Farm Labor, 1900-1939*. New Brunswick, NJ: Rutgers University Press.

Gupta, Arpit. 2019. "Foreclosure Contagion and the Neighborhood Spillover Effects of Mortgage Defaults." *Journal of Finance* 74 (5):2249–2301.

Henderson, J. Vernon and Yannis M. Ioannides. 1983. "A Model of Housing Tenure Choice." *American Economic Review* 73 (1):98–113.

Hoffman, Abraham. 1972. "Mexican Repatriation Statistics: Some Suggested Alternatives to Carey McWilliams." *Western Historical Quarterly* 3 (4):391–404.

———. 1974. *Unwanted Mexican Americans in the Great Depression: Repatriation Pressures, 1929-1939*. Tucson, AZ: University of Arizona Press.

Hornbeck, Richard. 2012. "The Enduring Impact of the American Dust Bowl: Short- and Long-Run Adjustments to Environmental Catastrophe." *American Economic Review* 102 (4):1477–1507.

———. 2023. "Dust Bowl Migrants: Environmental Refugees and Economic Adaptation." *Journal of Economic History* 83 (3):645–675.

Hornung, Erik. 2014. "Immigration and the Diffusion of Technology: The Huguenot Diaspora in Prussia." *American Economic Review* 104 (1):84–122.

Howard, Greg. 2020. "The Migration Accelerator: Labor Mobility, Housing, and Demand." *American Economic Journal: Macroeconomics* 12 (4):147–79.

Howard, Troup, Mengqi Wang, and Dayin Zhang. 2024. "How Do Labor Shortages Affect Residential Construction and Housing Affordability?" Working Paper, University of Utah.

Isaksen, Joachim Vogt. 2019. "The Impact of the Financial Crisis on European Attitudes toward Immigration." *Comparative Migration Studies* 7 (1):1–20.

Jaeger, David A., Joakim Ruist, and Jan Stuhler. 2018. "Shift-share Instruments and the Impact of Immigration." NBER Working Paper 24285, National Bureau of Economic Research.

Johansen, Leif. 1959. "Substitution Versus Fixed Production Coefficients in the Theory of Economic Growth: A Synthesis." *Econometrica* 27 (2):157–176.

Johnson, Kevin R. 2005. "The Forgotten Repatriation of Persons of Mexican Ancestry and Lessons for the War on Terror." *Pace Law Review* 26:1.

Kaplan, Greg, Kurt Mitman, and Giovanni L. Violante. 2020. "The Housing Boom and Bust: Model Meets Evidence." *Journal of Political Economy* 128 (9):3285–3345.

Lach, Saul. 2007. "Immigration and Prices." *Journal of Political Economy* 115 (4):548–587.

Lee, Jongkwan, Giovanni Peri, and Vasil Yasenov. 2022. "The Labor Market Effects of Mexican Repatriations: Longitudinal Evidence from the 1930s." *Journal of Public Economics* 205:104558.

Logan, Trevon D. and John M. Parman. 2017. "The National Rise in Residential Segregation." *Journal of Economic History* 77 (1):127–170.

Lyons, Ronan C., Allison Shertzer, Rowena Gray, and David N. Agorastos. 2026. "The Price of Housing in the United States, 1890-2006." *Quarterly Journal of Economics* 141 (1):559–603.

Manson, Steven, Jonathan Schroeder, David Van Riper, Tracy Kugler, and Steven Ruggles. 2020. "IPUMS National Historical Geographic Information System: Version 15.0 [dataset]." Minneapolis, MN: IPUMS, University of Minnesota.

Mathy, Gabriel and Nicolas L. Ziebarth. 2017. "How Much does Political Uncertainty Matter? The Case of Louisiana under Huey Long." *Journal of Economic History* 77 (1):90–126.

Mian, Atif, Amir Sufi, and Francesco Trebbi. 2015. "Foreclosures, House Prices, and the Real Economy." *Journal of Finance* 70 (6):2587–2634.

Monras, Joan. 2020. "Immigration and Wage Dynamics: Evidence from the Mexican Peso Crisis." *Journal of Political Economy* 128 (8):3017–3089.

Moraga, Jesús Fernández-Huertas, Ada Ferrer-i-Carbonell, and Albert Saiz. 2019. "Immigrant Locations and Native Residential Preferences: Emerging Ghettos or New Communities?" *Journal of Urban Economics* 112:133–151.

Morin, Miguel and Scott N. Swisher. 2016. "The Effect of Infrastructure Investment in a Low-Growth Environment: Evidence from the Great Depression." Working Paper, University of Cambridge.

Ortega, Francesc. 2005. "Immigration Quotas and Skill Upgrading." *Journal of Public Economics* 89 (9-10):1841–1863.

Ottaviano, Gianmarco I.P. and Giovanni Peri. 2006. "Wages, Rents and Prices: The Effects of Immigration on US Natives." *Center for Research and Analysis of Migration, Discussion Paper* 13 (07).

Pastor, Lubos and Pietro Veronesi. 2021. "Inequality Aversion, Populism, and the Backlash against Globalization." *Journal of Finance* 76 (6):2857–2906.

Poterba, James M. 1992. "Taxation and Housing: Old Questions, New Answers." *American Economic Review* 82 (2):237–242.

Price, Joseph, Kasey Buckles, Jacob Van Leeuwen, and Isaac Riley. 2021. "Combining Family History and Machine Learning to Link Historical Records: The Census Tree Data Set." *Explorations in Economic History* 80:101391.

Quincy, Sarah. 2022. "Income Shocks and Housing Spillovers: Evidence from the World War I Veterans' Bonus." *Journal of Urban Economics* 132:103494.

Roback, Jennifer. 1982. "Wages, Rents, and the Quality of Life." *Journal of Political Economy* 90 (6):1257–1278.

Romer, Margaret. 1922. "A History of Calexico." *Annual Publication of the Historical Society of Southern California* 12 (2):26–66.

Rosen, Sherwin. 1979. "Wage-Based Indexes of Urban Quality of Life." In *Current Issues in Urban Economics*, edited by Peter Mieszkowski and Mahlon Straszheim. Baltimore: Johns Hopkins University Press, 74–104.

Ruggles, Steven, Catherine A. Fitch, Ronald Goeken, Josiah Grover, David Hacker, Matt Nelson, José Pacas, Evan Roberts, and Matthew Sobek. 2020. "IPUMS Restricted Complete Count Data: Version 2.0 [dataset]." Minneapolis, MN: IPUMS, University of Minnesota.

Sá, Filipa. 2015. "Immigration and House Prices in the UK." *Economic Journal* 125 (587):1393–1424.

Saiz, Albert. 2003. "Room in the Kitchen for the Melting Pot: Immigration and Rental Prices." *Review of Economics and Statistics* 85 (3):502–521.

———. 2007. "Immigration and Housing Rents in American Cities." *Journal of Urban Economics* 61 (2):345–371.

Saiz, Albert and Susan Wachter. 2011. "Immigration and the Neighborhood." *American Economic Journal: Economic Policy* 3 (2):169–88.

Schumann, Abel. 2014. "Persistence of Population Shocks: Evidence from the Occupation of West Germany After World War II." *American Economic Journal: Applied Economics* 6 (3):189–205.

Sharpe, Jamie. 2019. "Re-Evaluating the Impact of Immigration on the US Rental Housing Market." *Journal of Urban Economics* 111:14–34.

Simon, Daniel T. 1974. "Mexican Repatriation in East Chicago, Indiana." *Journal of Ethnic Studies* 2 (2):11–23.

Snowden, Kenneth A. 2006. "Construction, Housing, and Mortgages." In *Historical Statistics of the United States: Millennial Edition*, edited by Susan B. Carter, Scott S. Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. Cambridge: Cambridge University Press, 395–572.

Solon, Gary, Steven J. Haider, and Jeffrey M. Wooldridge. 2015. "What are We Weighting For?" *Journal of Human Resources* 50 (2):301–316.

Stock, James H. and Motohiro Yogo. 2005. "Testing for Weak Instruments in Linear IV Regression." In *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, edited by Donald W. K. Andrews and James H. Stock. Cambridge, UK: Cambridge University Press, 80–108.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *Review of Economic Studies* 87 (1):454–486.

Terry, Stephen J., Thomas Chaney, Konrad B. Burchardi, Lisa Tarquinio, and Tarek A. Hassan. 2026. "Immigration, Innovation, and Growth." *American Economic Review* 116 (3):828–861.

Testa, Patrick A. 2021. "The Economic Legacy of Expulsion: Lessons from Postwar Czechoslovakia." *Economic Journal* 131 (637):2233–2271.

U.S. Bureau of the Census. 1949. *Historical Statistics of the United States, 1789–1945*. Washington, DC: U.S. Government Printing Office.

Valdés, Dennis Nodín. 1988. "Mexican Revolutionary Nationalism and Repatriation during the Great Depression." *Mexican Studies/Estudios Mexicanos* 4 (1):1–23.

Wang, Tianyi. 2021. "Media, Pulpit, and Populist Persuasion: Evidence from Father Coughlin." *American Economic Review* 111 (9):3064–92.

White, Eugene N. 2014. "Lessons from the Great American Real Estate Boom and Bust of the 1920s." In *Housing and Mortgage Markets in Historical Perspective*, edited by Eugene N. White, Kenneth Snowden, and Price V. Fishback. Chicago: University of Chicago Press, 115–160.

Table 1. House-level descriptive statistics. This table presents the mean statistic for variables obtained at the house level. *Average Resident Age* is the average age for all working-age residents within the same address in the 1930 Census. *Dwelling Size* reports how many people lived within the entire dwelling in 1930. *Share Mexicans* is the number of working-age Mexican immigrants in any given address as a share of the total number of working-age residents in that same address. Similarly, *Share Black U.S.-born* and *Share U.S.-Born* are the shares of black, or total U.S.-born residents in an address, respectively. These measures are constructed using the information of race and place of birth of each resident from the 1930 Census. *Ownership* indicates the share of residents that were house owners in any given address. *House Value* and *Rent* are the self-reported house value and rent in 1930 U.S. dollars, respectively. For addresses with multiple households, we aggregate this information using the median house value and the median rent reported by the households in the same address. Panel A presents the summary statistics of these variables and compares them across the complete-count 1930 Census data (column 1), the full address-matched sample (column 2), and the equivalent complete (column 3) and matched (column 4) samples when restricting to states that contained a share of Mexican workers greater than 0.25% of the total state workforce in the studied cities: Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, Indiana. Panel B presents the mean characteristics in 1930 for Mexican or U.S.-born houses in the restricted and matched sample. Mexican houses are defined as those with more than half of their households born in Mexico, and U.S.-born houses are defined as those where all households were born in the U.S. and have no Mexican descent.

Panel A. Comparison between samples

	Unrestricted		Restricted	
	Complete	Matched	Complete	Matched
Average Resident Age	37.64	37.88	37.98	38.03
Dwelling Size	3.91	4.16	3.59	3.81
Share Mexicans	0.01	0.005	0.03	0.02
Share U.S.-Born	0.76	0.80	0.83	0.85
Share Black U.S.-Born	0.08	0.07	0.06	0.05
Ownership	0.41	0.52	0.44	0.53
Rent (1930 USD)	58.47	54.53	51.19	49.94
House Value (1930 USD)	7,598.83	7,611.27	6,310.36	6,258.27
Observations	14,324,076	4,019,198	2,288,532	730,380

Panel B. Comparison between Mexican and U.S.-born houses in 1930

	Restricted and Matched Sample	
	Mexican Houses	U.S.-Born Houses
Average Resident Age	34.99	37.78
Dwelling Size	5.07	3.70
Share Black U.S.-Born	0.004	0.07
Ownership	0.33	0.52
Rent (1930 USD)	32.29	49.34
House Value (1930 USD)	2,688.48	6,100.66
Observations	19,173	557,791

Table 2. City-level descriptive statistics. This table presents summary statistics for variables used in our empirical analysis. *Mexican Outflow* is constructed in Eq. (5) and is the city-level change in the Mexican labor force between 1930 and 1940 divided by the city's total working-age population in 1930. The dependent variables (the number of building permits growth rate, the value of building permits growth rate, the median house value growth rate, and the median rent growth rate are the main city-level real estate market outcomes used in our regressions and are described in detail in Section 4.2. Section 4.1 describes the construction of our instrumental variable, which combines the proximity to the Mexican border (calculated using travel times in 1930 throughout the U.S. road network to the nearest Mexican border crossing station) interacted with the historical Mexican settlements (share of Mexican immigrants in 1900). The baseline control variables are from the 1930 Census and consist of the city-level averages of the worker's age, school attendance status, and share of unemployed workers. We use additional county-level variables as the retail sales growth (a measure for the Great Depression intensity) from Fishback, Horrace, and Kantor (2005). We also control for the intensity of the Dust Bowl environmental catastrophe using county-level measures for months of drought interacted with the county's farmland share from Fishback, Horrace, and Kantor (2005), and the fraction of each county exposed to medium and high permanent soil erosion during the 1930s from Hornbeck (2012). Our full sample contains 868 U.S. cities.

Variables	**N**	**Mean**	**SD**	**Min**	**Median**	**Max**
Mexican Outflow, 1930–1940	868	0.204	1.086	–0.559	0	17.797
Dependent Variables (1930–1940)						
Median House Value Growth	868	–19.308	15.458	–76.157	–20.523	49.019
Median Rent Growth	868	–1.371	14.485	–42.777	–4.628	114.587
Number of Building Permits Growth	238	180.607	344.975	–100	84.757	2,780
Value of Building Permits Growth	193	–17.529	61.289	–96.658	–28.933	298.906
Instrumental Variable						
IV [1900 Settlement × Proximity to Mexico]	868	0.093	0.745	0	0	11.284
City Characteristics (1930)						
Average Age	868	37.160	1.395	32.659	37.313	41.494
School Attendance	868	3.957	1.672	1.413	3.540	13.083
Unemployment	868	9.506	3.657	1.343	9.081	36.179
County Characteristics						
Retail Sales Growth (1929-35)	868	–0.216	0.127	–0.801	–0.211	0.203
Months Drought × Farm Share (1930)	868	13.141	12.072	0	9.929	72.737
Dust Bowl Medium Erosion Exposure	868	7.147	21.736	0	0	100
Dust Bowl High Erosion Exposure	868	1.569	9.372	0	0	99.533

Table 3. Effects on house-level prices. This table presents our baseline estimates of the effect of the Mexican repatriation on house values and rents. We estimate the house-level regressions from Eq. (7):

$$\underset{1930:40}{\Delta} Y_h = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h,$$

where $\Delta_{1930:40} Y_h$ is a house's 1930–1940 percentage change in its *House Value* or *Rent*. $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 in the city c where the house is located instrumented by IV_{cty}, as defined in Eq. (6); λ_s represents state fixed effects; and $\mathbf{X}_{h,1930}$ is the set of variables used as controls for house h in 1930, which include (i) house-level characteristics *Average Age of Residents*, the *Dwelling Size*, the *Share of Black Residents*, the *Share of Female Residents*, and the house value or rent reported in 1930; (ii) city-level variables, the *Average Age*, average *School Attendance*, and the share of *Unemployed* workers; (iii) county-level variables, *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. All specifications also include a set of city-level employment *Sector Shares* as a *proxy* for the economic conditions of cities in 1930. We define a Mexican-occupied house as an address that had 50% or more of its households in 1930 of Mexican origin. U.S.-born-occupied houses are addresses that had only U.S.-born and no Mexican-descendant households in 1930. The table presents the estimates of the effect for Mexican-occupied houses (columns 1 and 3) and U.S.-born-occupied houses (columns 2 and 4). In columns 1 and 2, we restrict the sample to the houses that were owned in 1930 and 1940. In columns 3 and 4, we restrict it to houses that were rented in 1930 and 1940. All regressions restrict the sample to states with more than 0.25% Mexican population living in urban areas (Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, and Indiana). Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	House value change (%)		Rent change (%)	
	Mexican-Occupied	U.S.-born-Occupied	Mexican-Occupied	U.S.-born-Occupied
	(1)	(2)	(3)	(4)
Mexican Outflow	–14.467***	–1.258	–8.124**	–9.152*
	(2.078)	(1.218)	(3.132)	(4.728)
Observations	3,592	180,657	9,532	171,566
R-squared	0.021	0.001	0.010	0.001
Kleibergen–Paap F-statistic	751.4	171.4	488.0	106.2
Control Variables	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Table 4. Effects on neighbors. This table presents the estimates of the effect of the Mexican repatriation on house values and rents of neighbors. We estimate the house-level regressions from Eq. (7):

$$\underset{1930:40}{\Delta} Y_h = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h,$$

where $\Delta_{1930:40} Y_h$ is a house's 1930–1940 percentage change in its *House Value* or *Rent*. $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 in the city c where the house is located instrumented by IV_{cty}, as defined in Eq. (6); λ_s represents state fixed effects; and $\mathbf{X}_{h,1930}$ is the set of variables used as controls for house h in 1930, which include (i) house-level characteristics *Average Age of Residents*, the *Dwelling Size*, the *Share of Black Residents*, the *Share of Female Residents*, and the house value or rent reported in 1930; (ii) city-level variables, the *Average Age*, average *School Attendance*, and the share of *Unemployed* workers; (iii) county-level variables, *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. All specifications also include a set of city-level employment *Sector Shares* as a *proxy* for the economic conditions of cities in 1930. In all specifications, we restrict the sample to only houses fully occupied by U.S.-born individuals in 1930. We define a house as having a Mexican neighbor if it has an immediate neighbor house with residents of Mexican origin in 1930. A house has a U.S.-born neighbor if it has an adjacent house with only U.S.-born and no Mexican-descendant residents in 1930. The table presents the estimates for houses with Mexican neighbors (columns 1 and 3) and U.S.-born neighbors (columns 2 and 4). In columns 1 and 2, we restrict the sample to the houses that were owned in 1930 and 1940. Columns 3 and 4 restrict the sample to houses that were rented in 1930 and 1940. All regressions restrict the sample to states with more than 0.25% Mexican population living in urban areas (Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, and Indiana). Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	House value change (%)		Rent change (%)	
	Mexican Neighbor	U.S.-born Neighbor	Mexican Neighbor	U.S.-born Neighbor
	(1)	(2)	(3)	(4)
Mexican Outflow	–5.119**	–1.452	–10.600	–9.576
	(1.538)	(1.185)	(6.855)	(5.696)
Observations	2,576	122,115	3,792	109,567
R-squared	0.021	0.001	0.003	0.001
Kleibergen–Paap *F*-statistic	392.1	211.0	308.0	114.1
Control Variables	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Table 5. Effects on city-level housing, 1930–1940. This table presents our baseline regressions from estimating Eq. (8):

$$\underset{1930:40}{\Delta} Y_c = \alpha + \beta \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c,$$

where $\Delta_{1930:40} Y_c$ is a city's 1930–40 growth rate in one of its housing market outcomes: *Number of Building Permits*; *Value of Building Permits*; median reported *House Value*; and median reported *Rent*; $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 in city c instrumented by IV_{cty}, which is the instrument defined in Eq. (6); λ_s represents state fixed effects to capture state-specific, unobserved heterogeneity; and $\mathbf{X}_{c,1930}$ is a set of 1930 city-level controls, which include (i) city-level variables, the *Average Age*, average *School Attendance*, and the share of *Unemployed* workers; (ii) county-level variables, *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. The table presents the estimates of the effect of repatriation on the growth rates of the number of building permits (columns 1 and 2), the building permits value (columns 3 and 4), the cities' median house value (columns 5 and 6), and the cities' median rent (columns 7 and 8). Columns 1, 3, 5, and 7 show *weighted least squares* estimates of Eq. (8) without any instrumental variable or control. Columns 2, 4, 6, and 8 present the 2SLS results with IV_{cty} as an instrument for the *Mexican Outflow* and using the set of control variables and a set of city-level employment *Sector Shares* as a *proxy* for the economic conditions of cities in 1930. All regressions are weighted by the city's total working-age population in 1930. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Building Permit Growth (Number)		Building Permit Growth (Value)		Median House Value Growth		Median Rent Growth	
	WLS	2SLS	WLS	2SLS	WLS	2SLS	WLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mexican Outflow	–5.636	–15.934	–3.987***	–5.434***	–1.365*	–1.786***	–0.971***	–1.250***
	(4.384)	(9.643)	(0.825)	(1.881)	(0.747)	(0.585)	(0.245)	(0.326)
Observations	236	236	192	192	868	868	868	868
R-squared	0.42	0.27	0.49	0.14	0.43	0.49	0.35	0.23
Kleibergen–Paap *F*-statistic		1,499.405		553.835		239.181		239.181
Controls (City)		✓		✓		✓		✓
Sector Shares (16)		✓		✓		✓		✓
State FE		✓		✓		✓		✓

Table 6. Effects on the changes of ownership status and resident nativity. This table presents our house-level regressions for estimating the impact of the Mexican repatriation on the probability of a house changing its ownership status or its resident nativity. We estimate Eq. (9) and Eq. (10):

$$\underset{1930:40}{\Delta} y_h = \alpha + \gamma \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h$$

$$\underset{1930:40}{\Delta} y_h = \alpha + \gamma_1 \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) \times \mathbb{1}^{Homeowner}_{h,1930} + \gamma_2 \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) \times \mathbb{1}^{Renter}_{h,1930} + \lambda_s + \mathbf{X}_{h,1930} + \epsilon_h$$

$\underset{1930:40}{\Delta} y_h$ is the "ownership status change" and is a binary variable that equals one when there was a change in the ownership status of a home h between the 1930 and 1940 censuses (from rented to owner-occupied or vice-versa), and zero otherwise. $\underset{1930:40}{\Delta} y_h$ also represents the "resident nativity change", a binary variable that equals one when the home changed from being inhabited by a majority of Mexican immigrants in 1930 to being fully U.S.-born occupied in 1940. $\mathbb{1}^{Homeowner}_{h,1930}$ is a dummy that equals one if the unit was owner-occupied in 1930 and zero otherwise. $\mathbb{1}^{Renter}_{h,1930}$ is the analogous dummy variable for when the house h was rented in 1930. $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 in the city c where the house is located instrumented by IV_{cty}, as defined in Eq. (6); λ_s represents state fixed effects; and $\mathbf{X}_{h,1930}$ is the set of variables used as controls for house h in 1930, which include (i) house-level characteristics *Average Age of Residents*, the *Dwelling Size*, the *Share of Black Residents*, the *Share of Female Residents*, $\mathbb{1}^{Renter}_{h,1930}$, and the house value or rent reported in 1930; (ii) city-level variables, the *Average Age*, average *School Attendance*, and the share of *Unemployed* workers; (iii) county-level variables, *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. All specifications also include a set of city-level employment *Sector Shares* as a *proxy* for the economic conditions of cities in 1930. In columns 1, 2, 5, and 6, we restrict the sample to all houses inhabited by Mexican immigrants (at least 50% of the households) rented or owner-occupied in 1930; in columns 3 and 4, we restrict the sample to only U.S.-born occupied homes. All regressions are restricted to states with a Mexican population share greater than 0.25% (Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, and Indiana). Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Ownership Status Change				*Resident Nativity Change*	
	Mexican-Occupied$_{1930}$		*U.S.-Born-Occupied*$_{1930}$		*Mexican-Occupied*$_{1930}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Mexican Outflow	0.453*		–0.399***		–1.463**	
	(0.210)		(0.039)		(0.508)	
Mexican Outflow × *Renter*		0.640**		–0.553*		–1.724***
		(0.267)		(0.290)		(0.519)
Mexican Outflow × *Owner*		–0.155		–0.255		–0.611
		(0.161)		(0.252)		(0.338)
Observations	18,116	18,116	513,821	513,821	18,116	18,116
R-squared	0.052	0.052	0.008	0.008	0.055	0.057
Controls (House)	✓	✓	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓

Table 7. Instrument validity: correlations between the instrument and measures of Great Depression intensity and other migration outflows. This table presents two complementary instrument-validity tests, both of which we discuss in Section 6.1. The construction of IV_{cty} is described in Section 4.1. We estimate the following specifications:

$$\underset{1930:40}{\Delta} V_c = \alpha + \beta \cdot IV_{cty} + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c,$$

where $\Delta_{1930:40}\ V_c$ is one of the measures we test. Each entry is from a separate weighted city-level regression of the dependent variable on IV_{cty}, conditioning on the baseline set of controls $\mathbf{X}_{c,1930}$, employment sector shares, and state fixed effects. Regressions are weighted by the city's working-age population in 1930. Panel A tests whether the instrument is mechanically correlated with the intensity of the Great Depression: the dependent variables are the 1930–1940 change in the total number of unemployed workers, unemployed workers in agriculture, and unemployed workers in non-agriculture (all normalized by the 1930 working-age population), and the 1929–1935 growth rate of retail sales from Fishback, Horrace, and Kantor (2005). The column for retail sales growth excludes the same variable from the controls, since it enters as the dependent variable. Panel B tests whether the instrument predicts outflows of immigrants from other nationalities or exposure to the 1920s immigration quotas: the dependent variables are the 1930–1940 outflow rates of Mexicans (reference column), Latin Americans other than Mexicans, Canadians, and Asians, and the quota-exposure indices of Abramitzky et al. (2023) (Indicator and Intensity). In Panel B, we exclude cities with a net inflow of Mexicans. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A. Great Depression Intensity Measures

	Total Unemployed	*Unemployed in Agriculture*	*Unemployed in Non-Agriculture*	*Retail Sales Growth*
	(1)	(2)	(3)	(4)
IV_{cty}	0.018	0.019	–0.001	0.013
	(0.056)	(0.019)	(0.046)	(0.242)
R-squared	0.68	0.57	0.72	0.62
Observations	868	868	868	868
Controls (City)	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Panel B. Instrument and Outflow of Other Nationalities and Quota Exposure

	Outflow Measures				**1920s Quota Exposure**	
	Mexican	*Latin American*	*Canadian*	*Asian*	*Indicator*	*Intensity*
	(1)	(2)	(3)	(4)	(5)	(6)
IV_{cty}	1.345***	–0.000	0.008	–0.014	0.217	0.220
	(0.097)	(0.003)	(0.023)	(0.012)	(0.162)	(0.163)
Observations	664	664	662	664	664	664
R-squared	0.78	0.28	0.69	0.35	0.84	0.83
Controls (City)	✓	✓	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓

Appendix A Historical Newspaper Evidence of Harassment and Anti-Mexican Sentiment

(A) ***The New York Times*** **(1931)**

35,000 MEXICANS LEAVE CALIFORNIA

Migration, Expected to Reach 75,000, Due to Idleness and Sense of Unwelcome.

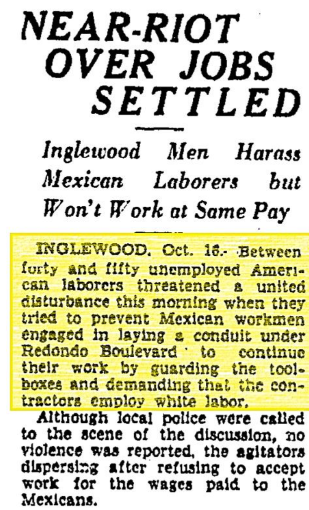
FEAR SPREAD BY ARRESTS

Deportation Campaign of Government Alarms Many—Sacrifices Made of Homes.

Special to The New York Times.

LOS ANGELES, April 11.—Moving southward by rail and automobile in families of from two to ten, more than 10,000 Mexicans, men, women and children, have been leaving Southern California monthly for more than three months and returning to Mexico. This was revealed today by Rafael de la Colina, local Mexican Consul. They are pressed by economic adversity, fearful over recently renewed activities of immigration authorities and perplexed by what they regard as anti-Mexican sentiment.

(B) ***The Los Angeles Times*** **(1930)**



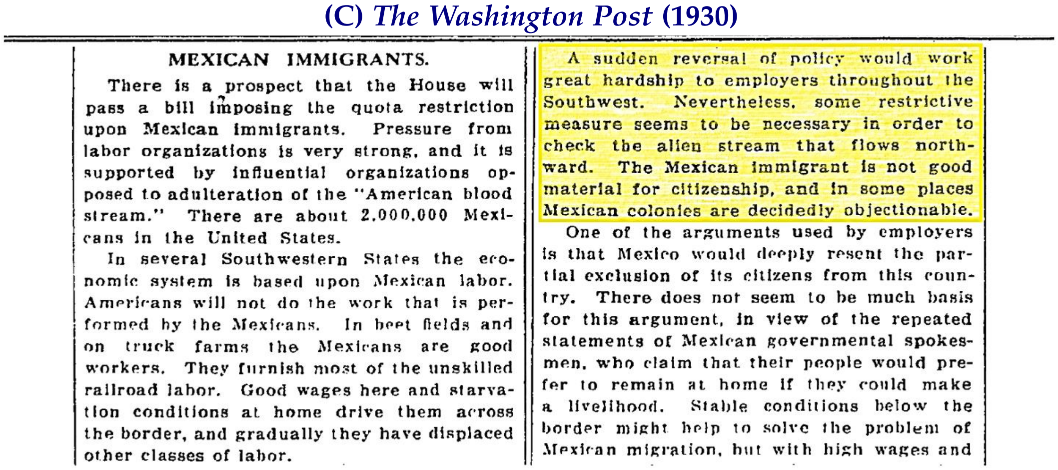
NEAR-RIOT OVER JOBS SETTLED

Inglewood Men Harass Mexican Laborers but Won't Work at Same Pay

INGLEWOOD, Oct. 16.- Between forty and fifty unemployed American laborers threatened a united disturbance this morning when they tried to prevent Mexican workmen engaged in laying a conduit under Redondo Boulevard to continue their work by guarding the toolboxes and demanding that the contractors employ white labor.

Although local police were called to the scene of the discussion, no violence was reported, the agitators dispersing after refusing to accept work for the wages paid to the Mexicans.

(C) ***The Washington Post*** **(1930)**



MEXICAN IMMIGRANTS.

There is a prospect that the House will pass a bill imposing the quota restriction upon Mexican immigrants. Pressure from labor organizations is very strong, and it is supported by influential organizations opposed to adulteration of the "American blood stream." There are about 2,000,000 Mexicans in the United States.

In several Southwestern States the economic system is based upon Mexican labor. Americans will not do the work that is performed by the Mexicans. In beet fields and on truck farms the Mexicans are good workers. They furnish most of the unskilled railroad labor. Good wages here and starvation conditions at home drive them across the border, and gradually they have displaced other classes of labor.

A sudden reversal of policy would work great hardship to employers throughout the Southwest. Nevertheless, some restrictive measure seems to be necessary in order to check the alien stream that flows northward. The Mexican immigrant is not good material for citizenship, and in some places Mexican colonies are decidedly objectionable.

One of the arguments used by employers is that Mexico would deeply resent the partial exclusion of its citizens from this country. There does not seem to be much basis for this argument, in view of the repeated statements of Mexican governmental spokesmen, who claim that their people would prefer to remain at home if they could make a livelihood. Stable conditions below the border might help to solve the problem of Mexican migration, but with high wages and

Figure A.1. Historical newspapers on the Mexican repatriation. This figure shows examples of newspaper articles from historical newspapers discussing the anti-Mexican sentiment after the Great Depression and during the period of the Mexican repatriation. Panel A shows a news piece from *The New York Times* from April 12, 1931. Panel B depicts a news piece from *The Los Angeles Times* dated from October 17, 1931. Panel C depicts a news piece from *The Washington Post* dated from January 20, 1930.

Alt text: Three 1930–1931 newspaper clippings on Mexican repatriation: Panel A New York Times, Panel B Los Angeles Times, Panel C Washington Post. Each article has a highlighting box around important excerpts.

Appendix B Data Description and Additional Empirical Results

Table B.1. Top 10 Cities in Mexican Outflow. This table shows the top 10 U.S. cities in terms of their observed *Mexican Outflow* as defined by Eq. (5) and calculated from the U.S. Census data. Panel A reports the ranking over the full analysis sample. Panel B restricts the ranking to cities with non-missing information on all housing market outcome variables (building permits and median prices).

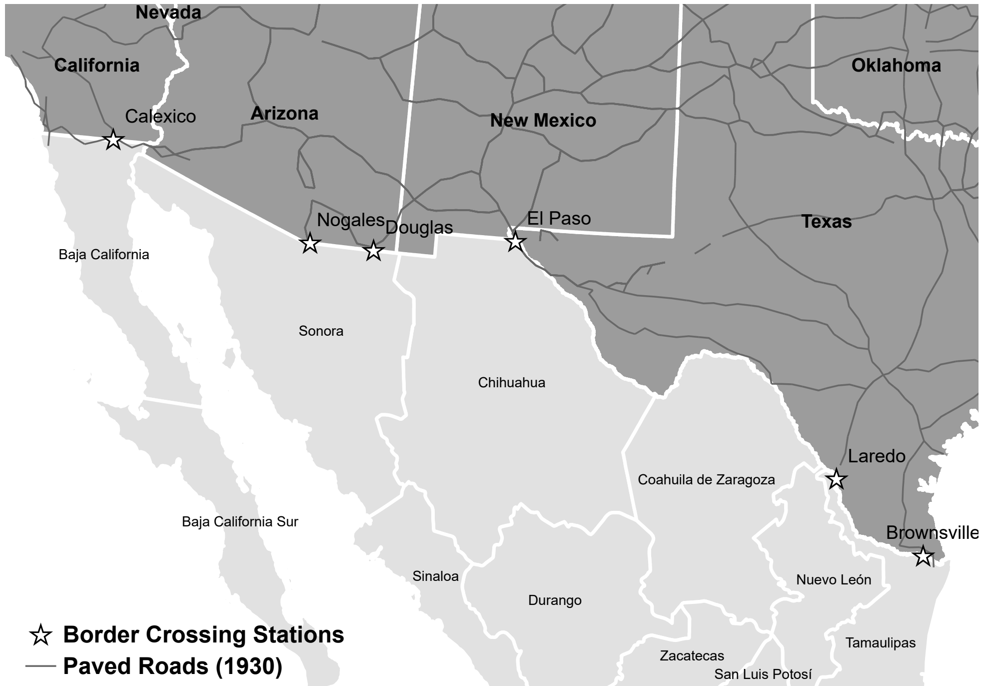
Panel A. Top 10 Cities in Mexican Outflow

	City	*Mexican Outflow Intensity, 1930–1940*
1	San Benito, TX	17.8%
2	El Paso, TX	14.5%
3	Del Rio, TX	11.8%
4	Brownsville, TX	8.9%
5	Laredo, TX	7.9%
6	East Chicago, IN	5.4%
7	Harlingen, TX	5.3%
8	San Antonio, TX	5.2%
9	Tucson, AZ	4.7%
10	Corpus Christi, TX	4.0%

Panel B. Top 10 Cities in Mexican Outflow (no missing outcome)

	City	*Mexican Outflow Intensity, 1930–1940*
1	El Paso, TX	14.5%
2	San Antonio, TX	5.2%
3	Pueblo, CO	2.0%
4	Gary, IN	1.9%
5	Los Angeles, CA	1.2%
6	Saginaw, MI	1.1%
7	San Diego, CA	0.9%
8	Phoenix, AZ	0.8%
9	Pasadena, CA	0.8%
10	Pontiac, MI	0.7%

(A) Border Crossing Stations



(B) Paved Roads Network (1930)

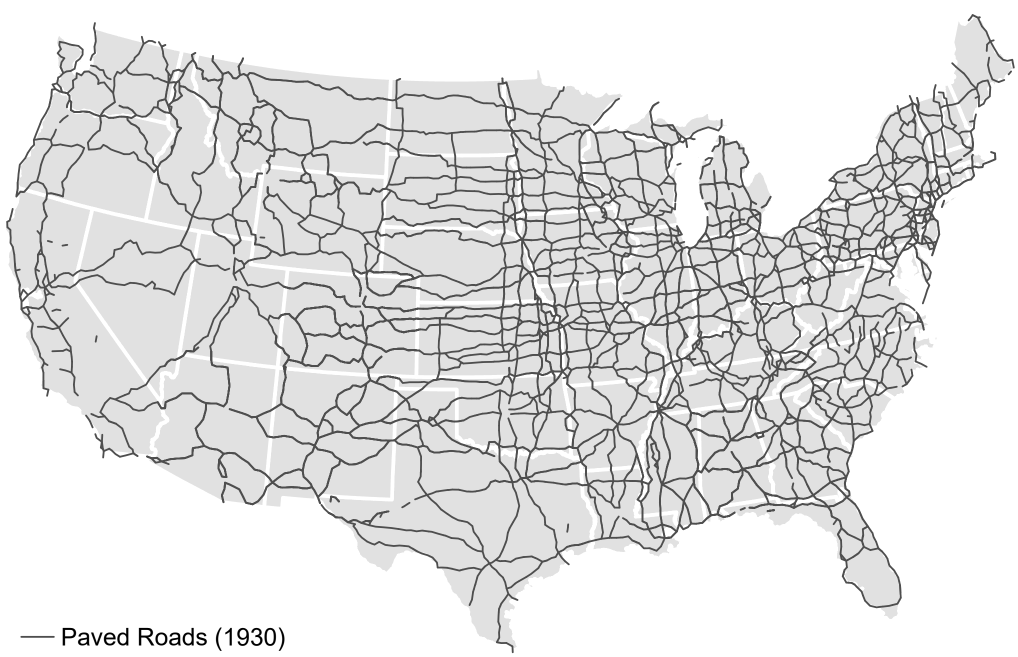


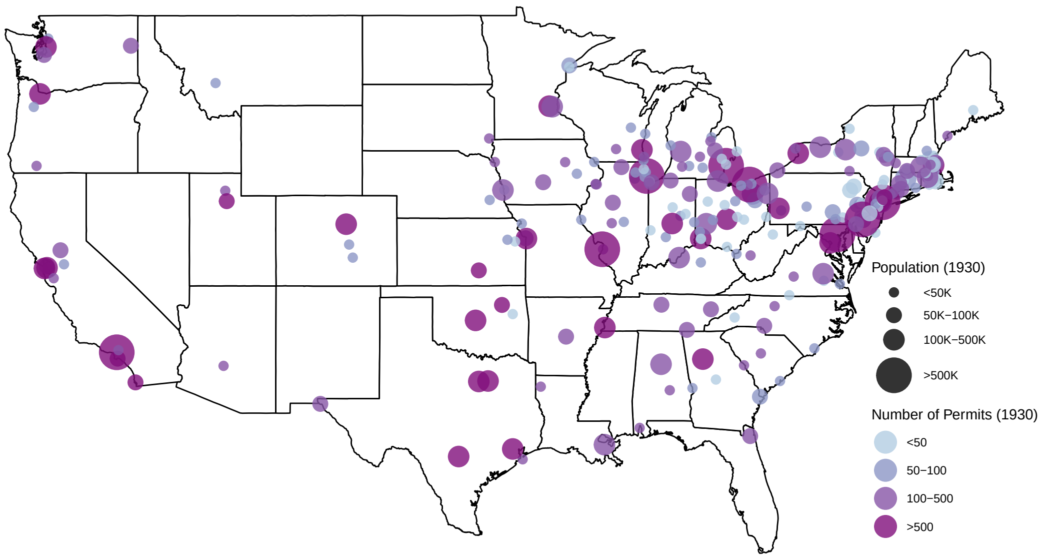
Figure B.1. Border crossing stations and the 1930 U.S. paved road network. Panel A shows the geographic location of the chief border crossing stations on the U.S.–Mexico border, overlaid on the 1930 paved road network (shown in blue) from Morin and Swisher (2016). Panel B presents the broader 1930 U.S. paved road network from the same source. Morin and Swisher (2016) use this road network to compute county-to-county travel times; we use the inverse of this measure to capture the cost associated with engaging in repatriation activities.

Alt text: Two maps: Panel A the U.S.–Mexico border region with crossing stations starred over the 1930 paved roads; Panel B the full 1930 continental U.S. road network.

Table B.2. Mexican and U.S.-born workers distribution by sector and occupation in 1930. This table shows the distribution of Mexican and U.S.-born workers in the U.S. according to the full-count 1930 U.S. Census. We report the number of workers in each category and their respective share relative to the total number of workers from each nationality. Occupations and sectors are defined by the U.S. Census Bureau. We consider as Mexicans anyone who declared Mexico as their birthplace. Similarly, U.S.-born are individuals who reported the United States as their birthplace and that did not have Mexican parents. Taking a closer look at the descriptive statistics for industries and occupations, Panel A shows that Mexican immigrants in 1930 were primarily employed in agriculture (37.5%), transportation (14.1%), and durable manufacturing (9.8%). In terms of occupations, Panel B shows that most Mexican immigrants in 1930 worked as laborers (34.7%), farm laborers (26%), and operatives (10.7%). For comparison, the table also presents the occupational and sectoral distributions of U.S. native-born workers; it shows a more even distribution of U.S.-born workers across these dimensions.

	Mexican Workers		U.S.-Born Workers	
Panel A. Sectors	Number	Share (%)	Number	Share (%)
Agriculture, Forestry, and Fishing	95,500	37.5	7,809,241	27.5
Transportation	35,859	14.1	1,777,318	6.3
Manufacturing (durables)	25,009	9.8	2,404,762	8.5
Personal services	21,976	8.6	2,566,983	9.0
Retail Trade	18,986	7.5	3,229,471	11.4
Manufacturing (nondurables)	16,520	6.5	2,508,287	8.8
Construction	15,605	6.1	1,799,683	6.3
Mining	10,762	4.2	706,384	2.5
Business and Repair Services	3,055	1.2	864,862	3.0
Professional and Related Services	2,575	1.0	1,826,147	6.4
Wholesale Trade	2,179	0.9	428,511	1.5
Utilities and Sanitary Services	2,123	0.8	259,709	0.9
Entertainment and Recreation Services	1,540	0.6	184,758	0.7
Finance, Insurance, and Real Estate	1,537	0.6	1,045,997	3.7
Public Administration	884	0.3	614,400	2.2
Telecommunications	473	0.2	369,838	1.3
Not declared	254,552		32,036,748	
	Mexican Workers		U.S.-Born Workers	
Panel B. Occupations	Number	Share (%)	Number	Share (%)
Laborers	96,588	34.7	3,228,242	10.5
Farm Laborers	72,178	26.0	2,676,618	8.7
Operatives	29,881	10.7	4,027,515	13.0
Farmers	19,906	7.2	4,891,850	15.8
Craftsmen	17,232	6.2	3,838,690	12.4
Service Workers (private household)	11,537	4.1	1,410,824	4.6
Service Workers (not household)	10,378	3.7	1,482,138	4.8
Sales workers	6,738	2.4	2,219,888	7.2
Managers, Officials, and Proprietors	6,606	2.4	1,952,912	6.3
Clerical workers	3,600	1.3	3,021,014	9.8
Professional, Technical	3,383	1.2	2,132,953	6.9
Non-occupational response	231,108		29,550,455	

(A) Number of Building Permits



(B) Value of Building Permits

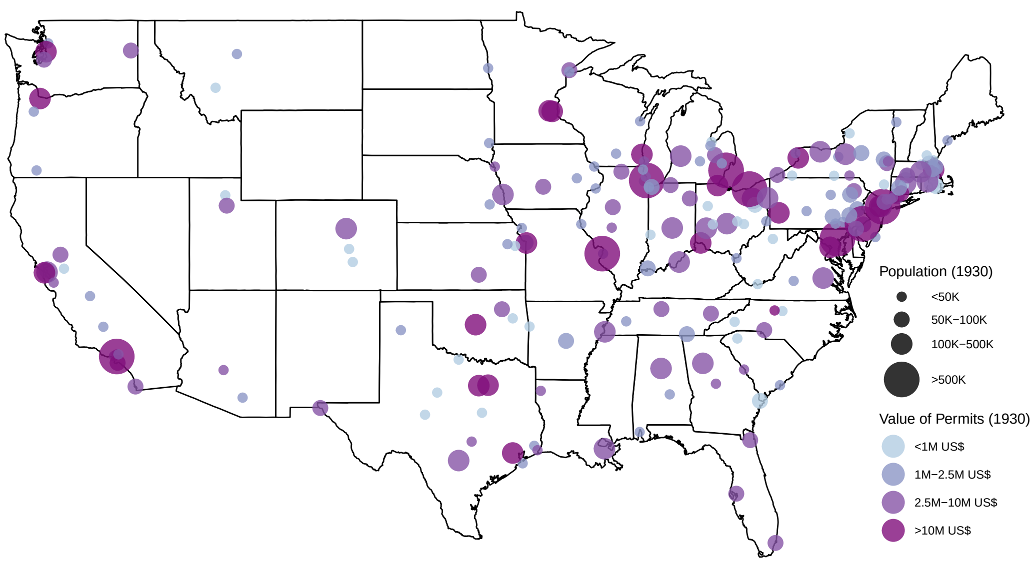
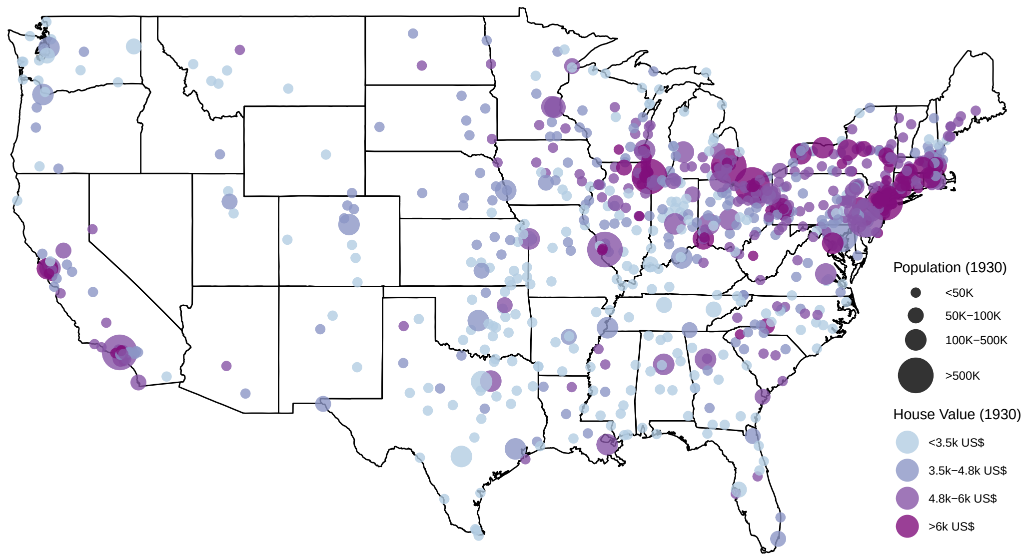


Figure B.2. Geographic distribution of housing-market outcomes across U.S. cities, 1930. This figure displays the geographic distribution of the four city-level housing-market outcomes used in our analysis. Panel A shows the number of building permits by city, collected by Snowden (2006) from several issues of the *Bulletin of the Bureau of Labor Statistics*. Panel B shows the total value of building permits (in millions of dollars), collected from several issues of the *Dun & Bradstreet's Review*, a business and financial publication from the 1920s and 1930s. Panel C (on the following page) shows the median house value (in thousands of dollars) from the 1930 full-count U.S. Census (Ruggles et al., 2020). Panel D (on the following page) shows the median rent (in dollars) from the same source. In every panel, the colors distinguish cities by their value of the respective outcome in 1930, and the size of each bubble is proportional to the city's working-age population.

Alt text: Four U.S. bubble maps of 1930 city housing outcomes: number of building permits (A), value of building permits (B), median house value (C), and median rent (D).

(C) Median House Value



(D) Median Rent

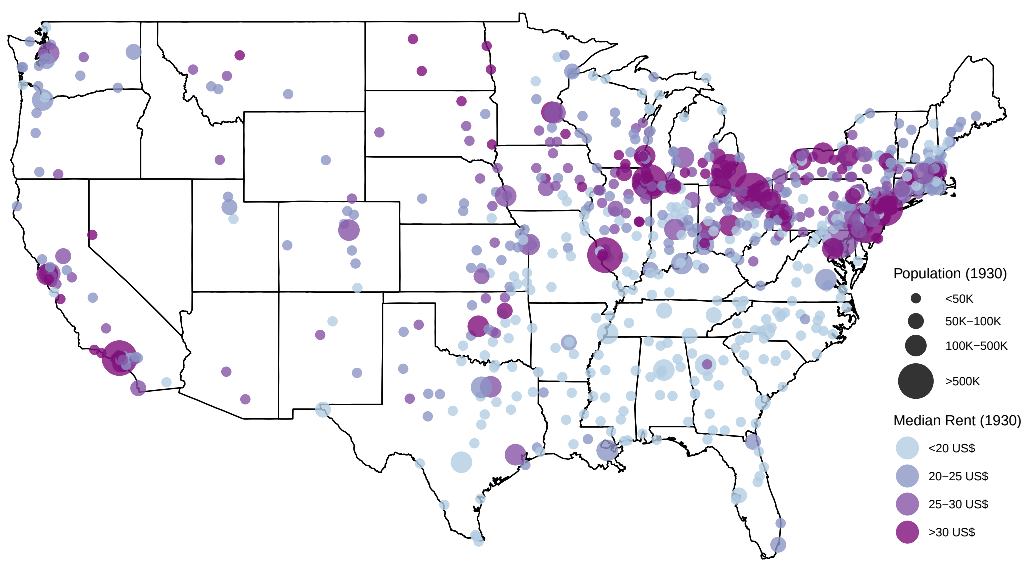


Figure B.2. Geographic distribution of housing-market outcomes across U.S. cities, 1930 (Continued). This figure displays the geographic distribution of the four city-level housing-market outcomes used in our analysis. Panel A (on the preceding page) shows the number of building permits by city, collected by Snowden (2006) from several issues of the *Bulletin of the Bureau of Labor Statistics*. Panel B (on the preceding page) shows the total value of building permits (in millions of dollars), collected from several issues of the *Dun & Bradstreet's Review*, a business and financial publication from the 1920s and 1930s. Panel C shows the median house value (in thousands of dollars) from the 1930 full-count U.S. Census (Ruggles et al., 2020). Panel D shows the median rent (in dollars) from the same source. In every panel, the colors distinguish cities by their value of the respective outcome in 1930, and the size of each bubble is proportional to the city's working-age population.

Alt text: Four U.S. bubble maps of 1930 city housing outcomes: number of building permits (A), value of building permits (B), median house value (C), and median rent (D).

B.1 Housing Supply Effects: Employment in the Construction Industry

In 1930, a significant portion of Mexican immigrant workers was employed in the construction industry. Table B.2 shows that approximately 6% of the Mexican workers were employed in the construction and related sectors. Therefore, the repatriation might also have affected the housing supply through employment in these sectors. For instance, when studying Mexican immigration in the 1990s, Monras (2020) finds that low-skilled Mexican workers entering the construction sector in the U.S. after the Peso crisis led to lower wages and, consequently, lower construction costs.

Table B.3 presents estimates of how the repatriation affected changes in employment in the construction industry to Mexicans (columns 1–2), U.S.-born (columns 3–4), and total employment (columns 5–6), all relative to the total working-age population in each city. We consider two complementary definitions of the construction sector: a *narrow* definition restricted to the construction industry itself, and a *broad* definition that additionally includes construction-related durable-manufacturing industries, such as logging, wood products, furniture, glass, cement, structural clay, pottery, primary iron and steel, and fabricated metal products.

The first set of results (columns 1–2) is not surprising. It shows a negative effect of the repatriation on the share of employment of Mexicans in the construction sector. The second set of regressions (columns 3–4) shows that cities more affected by repatriation had an increase in the employment of U.S.-born in construction. However, once we consider the total employment in the construction sector (columns 5–6), we find a small and not statistically significant effect. Taken together, these results suggest that the repatriation induced the substitution of Mexicans for U.S.-born workers in the construction and construction-related sectors, but had no impact on the overall employment growth in these sectors. This is in line with the employment results spanning all sectors estimated by Lee, Peri, and Yasenov (2022).

B.2 Distribution Effects: Robustness to Different Percentiles of Values and Rents

Taking advantage of the full-count census data available, we expand the analysis to investigate the sensitivity of the impact of Mexican repatriation across the distribution of house values and rents.[23] We examine the effects across different percentiles within cities by estimating the repatriation effect at different percentiles of the house value and rent distributions. The equation we estimate is analogous to Eq. (8) and is given by:

$$\underset{1930:40}{\Delta} Y_c(\tau) = \alpha + \beta \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c, \tag{B.1}$$

where $\Delta_{1930:40} Y_c(\tau)$ is a city's 1930–40 growth rate in reported *House Value* or *Rent* at percentile τ. We obtain, for each city, the house value or rent at several points of the house value or rent distribution. Specifically, to avoid the influence of outliers, we consider the interquartile range (i.e., from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile), breaking it down into 10 percent intervals.

The estimated coefficients and 95% confidence intervals of the two-stage least squares (2SLS) estimates are presented in Figure B.3. The median (50$^{\text{th}}$ percentile) effect is the coefficient reported in Table 5. The results show that the Mexican repatriation had statistically significant effects across different percentiles of the house value and rent distributions. Although we do observe some variation at the magnitudes of the coefficients, their differences are not statistically significant.

[23] We cannot conduct a similar analysis for building permits because they are aggregate city-level measures.

Table B.3. Effects on employment in the construction industries, 1930–1940. This table presents the results from estimating:

$$\underset{1930:40}{\Delta} Emp_c^{Mex|US|Tot} = \alpha + \beta \cdot \tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right) + \lambda_s + \mathbf{X}_{c,1930} + \epsilon_c,$$

where $\Delta_{1930:40} Emp_c^{Mex|US|Tot}$ is a city's employment change in the construction industries between 1930 and 1940 for each group of workers: Mexican (*Mex*) employment, U.S.-born (*U.S.*), and total (*Tot*) employment. $\tilde{O}^{MEX}_{1930:40,c}\left(IV_{cty}\right)$ is the Mexican outflow between 1930 and 1940 in the city c instrumented by IV_{cty}, which is the instrument defined in Eq. (6); λ_s represents state fixed effects to capture state-specific, unobserved heterogeneity; and $\mathbf{X}_{c,1930}$ is a set of 1930 city-level controls, which include the population *Average Age*, average *School Attendance*, share of *Unemployed* workers, and county-level controls *Drought Exposure*, which is the interaction on the number of severe drought months faced by a county and the county's share of farming land; the *Retail Sales Growth* rate between 1929 and 1935; the log of the city-level median house value in 1930; the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. The table presents the estimates of the effect of repatriation on the growth of the employment of Mexicans (columns 1 and 2), U.S.-born (columns 3 and 4), and total employment (columns 5 and 6) in the construction industries. For each group, the first column adopts a *narrow* definition of the construction sector, restricted to the construction industry itself, while the second column adopts a *broad* definition that additionally includes construction-related durable manufacturing sectors: logging; miscellaneous wood products; furniture and fixtures; glass and glass products; cement, concrete, gypsum, and plaster products; structural clay products; pottery and related products; miscellaneous nonmetallic mineral and stone products; blast furnaces, steel works, and rolling mills; other primary iron and steel industries; fabricated steel products; fabricated nonferrous metal products; and not specified metal industries. All columns present 2SLS estimates with controls, state fixed effects, and the employment sector shares. All regressions are weighted by the total working-age population in 1930. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Employment Change (Mexicans)		Employment Change (Natives)		Employment Change (Total)	
Construction Sector Definition	Narrow (1)	Broad (2)	Narrow (3)	Broad (4)	Narrow (5)	Broad (6)
Mexican Outflow	–2.094***	–2.430***	2.653***	2.300***	0.993	0.006
	(0.043)	(0.092)	(0.826)	(0.730)	(0.922)	(0.807)
Observations	866	866	866	866	866	866
R-squared	0.73	0.85	0.39	0.27	0.40	0.28
Kleibergen–Paap *F*-statistic	238.556	238.556	238.556	238.556	238.556	238.556
Controls (City)	✓	✓	✓	✓	✓	✓
Sector Shares (16)	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓

B.3 Sensitivity to the Exclusion of Cities not Directly Exposed

One of the concerns with our baseline analysis is that our sample includes many cities that were not directly exposed to the Mexican repatriation. In this section, we investigate the sensitivity of our baseline estimates to different strategies to account for these non-affected cities. We re-estimate our baseline specifications, performing various sample restrictions to exclude the cities that were not directly affected by the Mexican repatriation. The goal

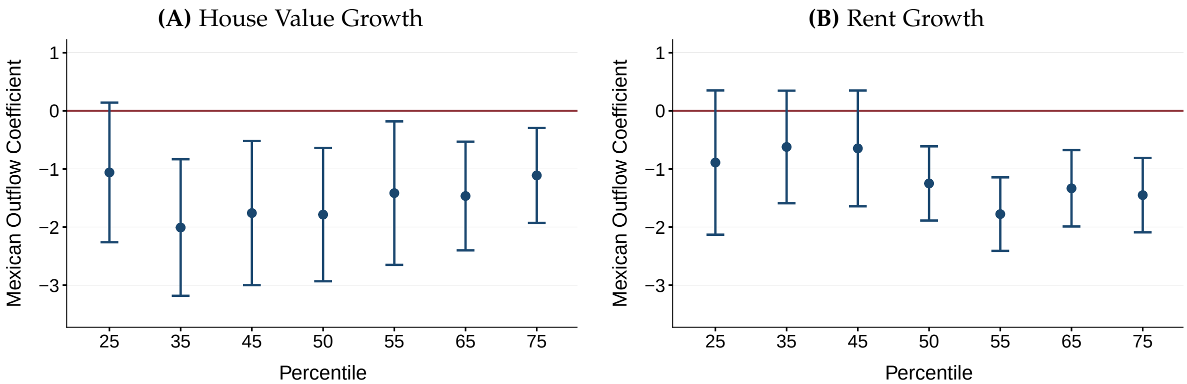


Figure B.3. Heterogeneous effects at different percentiles of the within-city house value and rent distribution. This figure re-estimates the *House Value Growth* and the *Rent Growth* equations from our baseline specification in Columns 9 and 12 of Table 5 but uses the growth rates at different percentiles of each city's *House Value* and *Rent* distributions as the dependent variable. The figure shows the coefficients and 95% confidence intervals for the two-stage least squares (2SLS) estimates of the Mexican outflow on house value growth (Panel A) and rent growth (Panel B). All specifications include the control variables, sector employment shares, and state-fixed effects.

Alt text: Two coefficient plots, House Value Growth (A) and Rent Growth (B): point estimates with confidence intervals for selected percentiles of the dependent variable: 25, 35, 45, 50, 55, 65, and 75.

is to study the sensitivity of the coefficients to the exclusion of cities and regions with plausibly no exposure to the Mexican repatriation.[24]

Table B.4 presents the estimated coefficients for *Number of Building Permits* (Column 1); *Value of Building Permits* (Column 2); median *House Value* (Column 3); and median *Rent* (Column 4); estimated by two-stage least squares (2SLS) using our IV, which is defined in Eq. (6). All specifications include the same set of control variables and state fixed effects, as discussed in Section 5.3. In Panel A, we restrict the sample to the states that had more than 0.25% of their working age population composed of Mexican workers in 1930 (Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, and Indiana). In Panel B, we restrict the sample to the states that contained the top 25 cities with the largest Mexican outflow rates (Texas, California, Indiana, Arizona, Colorado, Ohio, and New Mexico). All regressions are weighted by the total working-age population in 1930. We do not include the sector employment shares as controls due to the small sample sizes.

In summary, the results are qualitatively and quantitatively similar to the baseline results on Table 5. It is worth mentioning that the outflow coefficients for house value growth remain consistent with the baseline estimate, at around –1.8 in both panels, while the coefficient for rent growth is slightly smaller (in absolute terms) than the baseline. Our findings suggest that the results are robust to different sample restrictions to cities with a reasonable number of Mexican immigrants.

B.4 Robustness to the Exclusion of Multi-Family Addresses

Our linked address sample includes both single-family and multi-family dwellings. As discussed in Section 4.2, partial move-outs in multi-family buildings can mechanically shift the median

[24] In this section, we limit the sample to cities that are above some threshold in terms of the pre-existing Mexican-immigrant population. In doing so, some points are worth emphasizing. First, our measure of the Mexican population is limited to the working-age population. Therefore, the number of working-age Mexican immigrants may represent a much larger number of people after including other household members who are not of working age. Second, because our specifications include a rich set of control variables, some sample restrictions do not provide a sufficiently large sample to provide enough power to estimate our coefficients of interest.

Table B.4. Robustness to the exclusion of regions with few or no Mexican workers in 1930: effects on the housing market, 1930–40. This table presents the estimates of the baseline regressions but performs various sample restrictions to account for the cities that were not exposed to the Mexican repatriation. The goal is to study the sensitivity of our city-level estimates to the exclusion of the cities not directly affected by the Mexican repatriation. The regressions are estimated as given by Eq. (8). We exclude the employment sector shares in these specifications due to the small sample sizes. In Panel A, we restrict the sample to the states that had more than 0.25% of their working age population composed of Mexican workers in 1930 (Arizona, Texas, California, New Mexico, Kansas, Colorado, Wyoming, Utah, Idaho, Nebraska, and Indiana). In Panel B, we restrict the sample to the states that contained the top 25 cities with the largest Mexican outflow rates (Texas, California, Indiana, Arizona, Colorado, Ohio, and New Mexico). Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A. States with more than 0.25% share of Mexican workers

	Building Permit Change (Number)	*Building Permit Change (Value)*	*House Value Change*	*Rent Change*
	(1)	(2)	(3)	(4)
Mexican Outflow	–0.286	–3.844***	–1.793**	–0.924***
	(8.074)	(0.340)	(0.748)	(0.188)
Observations	39	47	158	158
R-squared	0.22	0.49	0.20	0.22
Kleibergen–Paap F-statistic	409.47	382.74	199.47	199.47
Controls (City)	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Panel B. States with the 25 cities with highest Mexican outflow

	Building Permit Change (Number)	*Building Permit Change (Value)*	*House Value Change*	*Rent Change*
	(1)	(2)	(3)	(4)
Mexican Outflow	2.859	–5.050***	–1.808***	–0.799***
	(12.053)	(0.902)	(0.436)	(0.065)
Observations	51	51	186	186
R-squared	0.29	0.30	0.34	0.22
Kleibergen–Paap F-statistic	236.02	299.26	139.82	139.82
Controls (City)	✓	✓	✓	✓
State FE	✓	✓	✓	✓

observed rent among remaining tenants, potentially biasing our rent estimates. To address this concern directly, we re-estimate our main house-level specifications from Table 3, restricting the sample to single-household addresses—properties with exactly one household per address—where we rule out selection via partial move-outs within a building.

Table B.5 presents the results. The house-value coefficient on single-family Mexican-occupied addresses is -14.56 (column 1), virtually identical to the full-sample estimate of -14.47 in Table 3. The rent coefficient on single-family Mexican-occupied addresses is -10.87 (column 2), which is larger in absolute value than the full-sample estimate of -8.13. Both estimates are statistically significant, and the first-stage F-statistics remain well above conventional thresholds (806 and 430, respectively). These results confirm that multi-family compositional effects do not drive our findings. If anything, the slightly larger rent coefficient on single-family ad-

Table B.5. Single-family robustness: Effects on Mexican-occupied house values and rents, 1930–1940. This table re-estimates the house-level specifications from Table 3, restricting the sample to single-household addresses—properties with exactly one household per address. Column 1 presents the 2SLS estimate for the percent change in house values among owner-occupied Mexican-occupied single-family addresses. Column 2 presents the corresponding estimate for the percent change in rents among renter-occupied Mexican-occupied single-family addresses. Both specifications instrument the *Mexican Outflow* with IV_{cty} (defined in Eq. (6)), include the baseline set of house- and city-level controls, sector employment shares, and state fixed effects. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Sample: Mexican-Occupied Single-Family Addresses	*House Value Change (%)*	*Rent Change (%)*
	(1)	(2)
Mexican Outflow	–14.559***	–10.871**
	(2.143)	(4.180)
Observations	3,507	7,767
Kleibergen–Paap F-statistic	806.1	429.8
Controls (House & City)	✓	✓
Sector Shares (16)	✓	✓
State FE	✓	✓

dresses suggests that the full-sample estimates are conservative, consistent with the attenuation bias from partial vacancies discussed in Section 4.2.

B.5 Interaction Specification for Neighbor Spillovers

The neighbor-spillover analysis in Table 4 partitions the U.S.-born owner-occupied and renter-occupied samples by Mexican-neighbor status and estimates Eq. (7) separately on each subsample. While the partitioned design is an easy and intuitive way to report the effect of the repatriation within each group, it does not allow us to test whether the two point estimates are statistically distinguishable. To deliver a formal test of differential spillovers and the contrast between the Mexican-neighbor and non-Mexican-neighbor coefficients, we consider a fully-interacted specification estimated on the pooled U.S.-born sample. This specification nests the two subgroup coefficients in a single regression and therefore allows direct, cluster-robust inference on their difference ($\hat{\beta}_{\text{Mex}} - \hat{\beta}_{\text{NoMex}}$). Pooling also yields a joint Kleibergen–Paap statistic that summarizes first-stage strength across both endogenous regressors and properly accounts for within-state correlation across neighbor groups—an inferential coherence the partitioned design cannot deliver. Every covariate, the state fixed effect, and both the endogenous Mexican outflow and its instrument are separately interacted with a Mexican-neighbor indicator:

$$\underset{1930:40}{\Delta} Y_h = \beta_{\text{Mex}} \cdot \left[\widetilde{O}^{MEX}_{1930:40,c} \times \mathbb{1}\{\textit{Mex Neighbor}_h\}\right] + \beta_{\text{NoMex}} \cdot \left[\widetilde{O}^{MEX}_{1930:40,c} \times \mathbb{1}\{\textit{No Mex Neighbor}_h\}\right] + \mathbf{X}'_{h,c,1930}\gamma_{g(h)} + \lambda_{s\cdot g(h)} + \epsilon_h, \tag{B.2}$$

where $\mathbb{1}\{\textit{Mex Neighbor}_h\}$ equals one if dwelling h had at least one Mexican-occupied next-door neighbor in 1930, $\mathbb{1}\{\textit{No Mex Neighbor}_h\}$ equals one if both neighbors were confirmed non-Mexican, and $g(h) \in \{\text{Mex, NoMex}\}$ indexes the dwelling's neighbor group. The control vector $\mathbf{X}_{h,c,1930}$ and the state fixed effect λ_s enter with group-specific coefficients ($\gamma_{g(h)}$ and $\lambda_{s\cdot g(h)}$), so the model is fully saturated in the neighbor group. The two endogenous outflow interactions are instrumented using IV_{cty} (defined in Eq. (6)) and interacted with each of the two neighbor-group indicators. Under this specification, $\hat{\beta}_{\text{Mex}}$ and $\hat{\beta}_{\text{NoMex}}$ report the total effect of the repatriation

Table B.6. Fully-interacted specification for neighbor spillovers, 1930–1940. This table presents 2SLS estimates of the fully-interacted specification in Eq. (B.2). Column 1 reports the percent change in house values for U.S.-born owner-occupied dwellings; column 2 reports the percent change in rents for U.S.-born renter-occupied dwellings. The two reported coefficients, $\hat{\beta}_{\text{Mex}}$ and $\hat{\beta}_{\text{NoMex}}$, measure the total effect of the *Mexican Outflow* on U.S.-born dwellings with and without at least one Mexican-occupied next-door neighbor in 1930, respectively, and are directly comparable to the subsample estimates in Table 4. Under the fully-interacted design, every baseline house- and city-level control, the sector employment shares, the state fixed effect, and both the endogenous outflow and its instrument are separately interacted with the Mexican-neighbor indicator. The two endogenous outflow interactions are instrumented with IV_{cty} (Eq. (6)) interacted with the same neighbor-status indicators. The reported Kleibergen–Paap F-statistic is the joint cluster-robust first-stage statistic across the two endogenous regressors. The sample consists of all U.S.-born-occupied addresses in 1930. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	House Value Change (%) (1)	Rent Change (%) (2)
Mexican Outflow × *Mexican Neighbor*	–5.119***	–10.600
	(1.512)	(6.796)
Mexican Outflow × *Non-Mexican Neighbor*	–1.452	–9.576
	(1.185)	(5.697)
Observations	124,691	113,359
R-squared	0.001	0.001
Kleibergen–Paap F-statistic (joint)	202.7	156.7
Controls × Neighbor Group	✓	✓
Sector Shares (16) × Neighbor Group	✓	✓
State FE × Neighbor Group	✓	✓

on each neighbor-status subgroup and are directly comparable to the subsample estimates in Table 4. The sample consists of all U.S.-born-occupied dwellings.

Table B.6 reports the estimates. For house values (column 1), the repatriation significantly reduced prices on U.S.-born dwellings adjacent to a Mexican-occupied property: $\hat{\beta}_{\text{Mex}} = -5.119$ ($p < 0.01$). The effect on U.S.-born dwellings without a Mexican neighbor is substantially smaller in absolute value and not statistically significant ($\hat{\beta}_{\text{NoMex}} = -1.452$). Conditional on the same city-level outflow and state fixed effect, the house-value decline is concentrated on dwellings that were next to a Mexican-occupied property. The magnitude of $\hat{\beta}_{\text{Mex}}$ is consistent with the subsample estimate for U.S.-born dwellings with a Mexican neighbor in Table 4. For rents (column 2), the two point estimates are negative and of similar magnitude, but neither is statistically significant, consistent with the rent null in the subsample estimates of Table 4.

B.6 Quasi-Counterfactual Exercise Details

In this section, we use a quasi-counterfactual exercise to illustrate the quantitative implications of our findings. This empirical exercise, similar to Burchardi, Chaney, and Hassan (2018), is not meant as a formal counterfactual; rather, it offers a reduced-form visualization of the effects of the Mexican repatriation on aggregate outcomes. How different would the growth rates of the housing market in U.S. cities have been had the Mexican repatriation not happened?

We first assume that the general equilibrium effects from the repatriation are either negligible or non-existent. In this case, our reduced-form results could be used to estimate the predicted changes in the outcome variables under the scenario without the Mexican repatriation. We also require an estimate of the outflow of Mexican immigrants in the absence of the Mexican repatriation. We use the data and the estimated coefficients from the first stage to derive a rough estimate. We start by assuming that the repatriation effect is completely captured by our instrumental variable, conditional on the control variables. The underlying assumption is that, if the repatriation had not happened, we would not have observed any effect of the instrumental variable on the Mexican outflow. The results in Table 7 support this idea.

The results show that the instrumental variable is relevant to explaining Mexican immigrants' outflow and that it does not have a statistically significant association with the outflow of immigrants from other nationalities. We use the estimates from the first stage regressions to obtain the hypothetical Mexican outflow in the absence of the repatriation. We calculate the hypothetical Mexican outflow in the case of no repatriation as $\ddot{O}^{MEX}_{1930:40,c} \equiv O^{MEX}_{1930:40,c} - \hat{\xi} \cdot IV_{cty}$, where $O^{MEX}_{1930:40,c}$ is the observed Mexican outflow and $\hat{\xi}$ is the estimated coefficient of the instrumental variable IV_{cty} in the first stage. We rearrange this expression to represent the predicted change in Mexican outflow as $dO^{MEX}_{1930:40,c} \equiv \hat{\xi} \cdot IV_{cty}$.

Given this predicted change in the Mexican outflow, we use the estimates from Table 5 to calculate the hypothetical change in the growth rates of our housing market outcome variables. The predicted change in the outcomes is specified by $d\left[\underset{1930:40}{\Delta} Y_c\right] \equiv \hat{\gamma} \cdot dO^{MEX}_{1930:40,c}$. Figure 7 in the main text visualizes the results, with maps of the predicted city-level changes and bar graphs for the ten cities with the largest observed Mexican outflow between 1930 and 1940.

Appendix C Instrument Validation and Pre-trends Assessment

C.1 First-Stage Regressions

Table C.1 reports the first-stage regressions of the Mexican outflow on our instrument $IV_{cty} = MexShare_{cty,1900} \times Proximity_{cty,1930}$, at both the house level (columns 1–4) and the city level (columns 5–6). The house-level estimates are reported separately for owner-occupied dwellings (columns 1–2) and renter-occupied dwellings (columns 3–4). Columns 1, 3, and 5 present the unconditional first stage. Columns 2, 4, and 6 add the full set of controls, state fixed effects, and employment sector shares, and weight the city-level observations in column 6 by the 1930 working-age population (weighted least squares). Across all specifications, the instrument predicts the Mexican outflow with a large positive coefficient that is significant at the 1% level and Kleibergen–Paap F-statistics well above the Stock and Yogo (2005) threshold, indicating that weak-instrument bias is not a concern in our setting.

Table C.1. First-stage regressions. This table presents the first-stage regressions of the instrumental variable approach. The first-stage regressions are given by:

$$O^{MEX}_{1930:40,c} = \alpha + \xi \cdot IV_{cty} + \epsilon_c,$$

where $O^{MEX}_{1930:40,c}$ is the Mexican outflow between 1930 and 1940 in any given city c. IV_{cty} combines the proximity to the Mexican border interacted with the share of Mexican immigrants in 1900. Columns 1–4 report the first-stage regression on the house-level sample, separately for owner-occupied dwellings (columns 1 and 2) and renter-occupied dwellings (columns 3 and 4), while columns 5 and 6 report the estimates at the city level. Columns 1, 3, and 5 estimate the equation above without additional controls. Columns 2 and 4 add state fixed effects, sector employment shares, and the set of controls which include: (i) house-level characteristics *Average Age of Residents*, the *Dwelling Size*, the *Share of Black Residents*, the *Share of Female Residents*, and the house value or rent reported in 1930; (ii) city-level variables, the *Average Age*, average *School Attendance*, the share of *Unemployed* workers; (iii) county-level variables, *Retail Sales Growth*, *Drought Exposure*, and the fraction of each county exposed to *Medium* and *High* permanent soil erosion due to the American Dust Bowl in the 1930s. Controls are described in more detail in Table 2. Column 6 includes the city- and county-level controls listed above, state fixed effects, sector employment shares, and weights observations by the 1930 working-age city population. The table also includes the Kleibergen–Paap F-statistics on the excluded instrument for each IV specification to test for weak identification. Standard errors are clustered by state for all specifications. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Mexican Outflow					
	Owner-Occupied		*Renter-Occupied*		*City-Level*	
	(1)	(2)	(3)	(4)	(5)	(6)
IV_{cty} [1900 Settlement × Proximity to Mexico]	1.361***	1.506***	1.311***	1.552***	1.194***	1.341***
	(0.056)	(0.187)	(0.055)	(0.198)	(0.014)	(0.087)
Kleibergen–Paap F-statistic	592.7	64.6	562.9	61.2	7,278.2	239.2
Observations	237,700	237,700	223,934	223,934	868	868
R-squared	0.68	0.77	0.67	0.79	0.67	0.68
Weighted						✓
Controls (House)		✓		✓		
Controls (City)		✓		✓		✓
Sector Shares (16)		✓		✓		✓
State FE		✓		✓		✓

Table C.2. Overidentification test: 2SLS with two excluded instruments. This table re-estimates the baseline city-level 2SLS specifications of Table 5 using two excluded instruments for the Mexican outflow: the 1900 county share of Mexicans (Mex_{cty}^{1900}) and the 1930 inverse travel time to Mexico ($\text{Proximity}_{cty}^{1930}$), rather than their interaction IV_{cty}. This yields one overidentifying restriction. All specifications include the baseline controls, sector employment shares, and state fixed effects, and are weighted by 1930 city population. Standard errors in parentheses are clustered by state. "Hansen J $\chi^2(1)$" is the cluster-robust Hansen J; under the null of valid overidentifying restrictions it is distributed $\chi^2(1)$. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	Building Permit Change (Number)	*Building Permit Change (Value)*	*House Value Change*	*Rents Change*
	(1)	(2)	(3)	(4)
Mexican Outflow	-17.158^{*}	-4.648^{**}	-2.036^{**}	-1.266^{***}
	(8.645)	(2.016)	(0.758)	(0.328)
Observations	236	192	868	868
Within R-squared	0.27	0.14	0.49	0.23
Kleibergen–Paap F-statistic	10,668.37	159.90	757.91	757.91
Hansen J $\chi^2(1)$ $[p]$	1.26 [0.26]	0.11 [0.74]	1.97 [0.16]	1.04 [0.31]
Controls + Sector Shares + State FE	✓	✓	✓	✓
Two Excluded Instruments	✓	✓	✓	✓

C.2 Overidentification Test

Our benchmark specification employs a single interacted instrument, $IV_{cty} = Mex_{cty}^{1900} \times Proximity_{cty}^{1930}$, which delivers an exactly identified 2SLS system. As an additional check on instrument validity, we re-estimate the city-level specifications of Table 5 while treating the two underlying components—the 1900 share of Mexicans in the county (Mex_{cty}^{1900}) and the inverse travel time to the U.S.-Mexico border ($Proximity_{cty}^{1930}$)—as separate excluded instruments. This yields one overidentifying restriction, which we evaluate using the cluster-robust Hansen J statistic—the appropriate choice given that our standard errors are clustered at the state level.

Table C.2 reports the results. Across all four outcomes, the coefficients on the Mexican outflow remain virtually unchanged relative to our single-instrument baseline—a pattern that suggests the multiplicative form of IV_{cty} is not driving our findings. First-stage Kleibergen–Paap F-statistics remain well above conventional thresholds across all four outcomes, so the two-instrument specification remains strongly identified. The Hansen J statistic—distributed $\chi^2(1)$ under the null of valid overidentifying restrictions—fails to reject at conventional levels for all four outcomes, with p-values ranging from 0.16 to 0.74.

C.3 Source of Identifying Variation

Our instrument IV_{cty} is the product of the 1900 Mexican settlement share ($MexShare_{cty,1900}$) and city-level proximity to the U.S.–Mexico border ($Proximity_{cty,1930}$). A natural concern is whether identification comes from the interaction or from one of its components alone. In our sample, $\text{corr}(IV_{cty}, MexShare_{cty,1900}) = 0.988$ and $\text{corr}(IV_{cty}, Proximity_{cty,1930}) = 0.180$: the instrument is nearly collinear with the historical settlement share and only weakly correlated with proximity. This has a direct implication for robustness: controlling linearly for the settlement share absorbs nearly all of the instrument's identifying variation, mechanically weakening the first stage (see the discussion of shares in shift-share designs in Goldsmith-Pinkham, Sorkin, and Swift, 2020).

Table C.3. Robustness to controlling for the main effects of the instrument. This table investigates the robustness of our baseline aggregate results when explicitly controlling for the components of our shift-share instrument. The dependent variables are the city-level median house value growth rate (odd columns) and median rent growth rate (even columns) between 1930 and 1940. Columns 1–2 reproduce the baseline specification. Columns 3–4 control linearly for proximity to Mexico (the shift component). Columns 5–6 introduce a flexible control for the 1900 Mexican population share (the share component) by adding dummy variables for a non-zero Mexican population and for having an above-median Mexican population share in 1900. Columns 7–8 show the benchmark where we control linearly for both main components simultaneously; due to severe multicollinearity, the instrument loses its power (F-statistic drops to 47.5), rendering the estimates statistically insignificant, although the point estimates remain negative and larger in absolute value. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

	A. Baseline		B. Add Proximity		C. Add Prox. & Mex. Dummies		D. Add Both (Linear)	
	(1) Median House Value Growth	(2) Median Rent Growth	(3) Median House Value Growth	(4) Median Rent Growth	(5) Median House Value Growth	(6) Median Rent Growth	(7) Median House Value Growth	(8) Median Rent Growth
Mexican Outflow	–1.786***	–1.250***	–2.136***	–1.298***	–2.014***	–1.238***	–4.734	–2.392
	(0.585)	(0.326)	(0.648)	(0.340)	(0.647)	(0.345)	(3.456)	(3.376)
Observations	868	868	868	868	868	868	868	868
First-Stage F-Stat	239.2	239.2	308.0	308.0	300.6	300.6	47.5	47.5

To illustrate this, Table C.3 reports the city-level 2SLS estimates of Eq. (8) for median house value growth and median rent growth across four specifications that progressively absorb the main effects of the instrument. Panel A reproduces the baseline with no main-effect controls (Kleibergen–Paap $F = 239$). Panel B adds proximity linearly ($F = 308$)—since proximity is only weakly correlated with IV_{cty}, this control absorbs geographic sorting without depleting the instrument. Panel C adds proximity together with flexible dummies for non-zero and above-median 1900 Mexican settlement share ($F = 301$), absorbing extensive-margin settlement patterns while preserving the continuous variation that drives the interaction. Panel D adds both main components linearly ($F = 47.5$); as expected from the near-perfect collinearity with the settlement share, the first stage weakens substantially.

Across Panels A–C, where the instrument retains its identifying variation, the 2SLS coefficients are stable and significant at the 1% level. Panel D illustrates the collinearity bound: the point estimates remain negative and larger in absolute value, but the standard errors inflate as the first stage collapses. We read the four panels together as evidence that the identifying variation in IV_{cty} comes through the cross-sectional variation in the 1900 settlement share, mediated by proximity to the border, consistent with the standard pattern in shift-share designs documented by Goldsmith-Pinkham, Sorkin, and Swift (2020).

C.4 Robustness to Controlling for Other Migration Shocks and Racial Segregation

Despite our instrumental variable approach and the validation tests presented so far, some concerns may persist about the potential association between our instrument and other important migration trends and residential segregation of the time. To address these concerns, we include three additional control variables in our baseline specifications. First, we include the measure of the 1920s quota intensity from Abramitzky et al. (2023). The idea is to control for the potential influence of the 1924 Immigration Act, which barred immigrants, mostly from Asian, Eastern, and Southern European countries (Abramitzky et al., 2023). Second, we include the percent change in the Black American population in the county between 1900 and 1930. The idea with this variable is to capture the first wave of the Great Migration when Black families migrated in

Table C.4. Robustness to the inclusion of additional controls for other local migrations shocks. This table presents the results from estimating our baseline specifications with the inclusion of additional control variables for local exposure to other migration shocks. Panel A estimates a similar specification to Table 3 at the house level, Panel B uses the specification of Table 4, and Panel C of Table 5. All specifications include the controls and fixed effects from the baseline analysis. In addition, we include the quota intensity exposure measure from Abramitzky et al. (2023), the neighbor-based measure of residential segregation from Logan and Parman (2017), and the percent change in black U.S.-born population between 1900 and 1930 in the county. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A. Effects on house-level prices

	House value change (%)		*Rent change (%)*	
	Mexican-Occupied	*U.S.-born-Occupied*	*Mexican-Occupied*	*U.S.-born-Occupied*
	(1)	(2)	(3)	(4)
Mexican Outflow	−14.796***	−1.518	−21.146***	−10.514**
	(3.001)	(1.123)	(3.803)	(4.542)
Observations	3,580	178,354	9,521	170,028
R-squared	0.021	0.001	0.011	0.001
Kleibergen–Paap *F*-statistic	220.42	171.36	4465.14	111.70
Controls + Sector Shares + State FE	✓	✓	✓	✓
Additional Controls	✓	✓	✓	✓

Panel B. Effects on neighbors

	House value change (%)		*Rent change (%)*	
	Mexican-Neighboring	*U.S.-born-Neighboring*	*Mexican-Neighboring*	*U.S.-born-Neighboring*
	(1)	(2)	(3)	(4)
Mexican Outflow	−7.324***	−1.376	−16.547*	−11.084*
	(1.197)	(1.096)	(8.403)	(5.412)
Observations	2,571	120,518	3,789	108,611
R-squared	0.023	0.001	0.004	0.001
Kleibergen–Paap *F*-statistic	470.42	200.70	4117.44	114.35
Controls + Sector Shares + State FE	✓	✓	✓	✓
Additional Controls	✓	✓	✓	✓

Panel C. Effects on city-level prices

	Building Permit Change (Number)	*Building Permit Change (Value)*	*House Value Change*	*Rents Change*
	(1)	(2)	(3)	(4)
Mexican Outflow	−16.709*	−4.047**	−1.904***	−1.367***
	(8.786)	(1.982)	(0.578)	(0.339)
Observations	231	188	830	830
R-squared	0.29	0.17	0.51	0.24
Kleibergen–Paap *F*-statistic	1,203.531	689.590	246.867	246.867
Controls + Sector Shares + State FE	✓	✓	✓	✓
Additional Controls	✓	✓	✓	✓

large numbers to the North of the country (Boustan, 2010; Derenoncourt, 2022). It is important to highlight that the moving rate of Black Americans from the South was significantly slower during the 1930s and that its first wave between 1910 and 1930 was much smaller compared to the second wave between 1940 and 1970 (Derenoncourt, 2022). Third, we include the neighbor-based measure of residential racial segregation from Logan and Parman (2017). The idea is to control for the influence of racial segregation on the local housing market.

C.5 Pre-trends Assessment

We conduct two complementary pre-trend assessments. First, we use annual city-level building-permit data over 1921–1940 to test for pre-1929 trends in housing-market activity. Second,

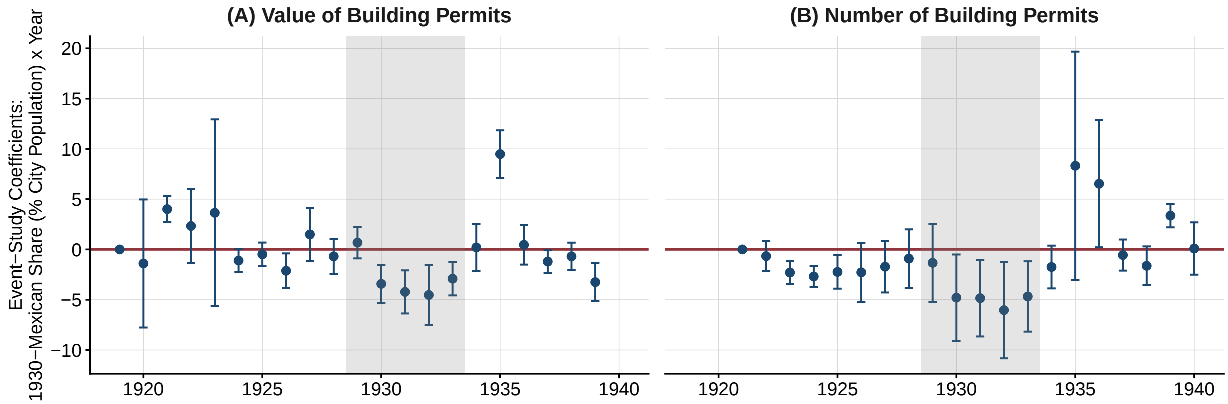


Figure C.1. Share of Mexicans in 1930 and annual building permit growth, 1920–1940. This figure shows coefficients of the share of Mexican workers in 1930 using the event study specification on the annual growth rate of the value of building permits (Panel A) and the growth rate of the number of building permits (Panel B). The lines are the point estimates from the event study specification with 95% confidence intervals. The shaded areas represent the Great Depression period dated by the NBER. Standard errors are clustered by state.

Alt text: Two event-study plots of yearly coefficients, 1920–1940, for the value of building permits (A) and number of building permits (B), with a shaded area representing the Great Depression period.

we re-estimate our baseline specifications while controlling for decennial non-Mexican population growth in the three decades preceding 1930, to assess whether our estimates could be driven by longer-run demographic trends.

C.5.1 Building Permits

We test for pre-trends in annual city-level building-permit growth associated with the 1930 share of Mexican workers, using the event-study specification in Eq. (12). Figure C.1 shows the estimated coefficients β_t with 95% confidence intervals. The coefficients for years before 1929 show no systematic pattern: they are individually indistinguishable from zero and display no trend, supporting our identification strategy. The negative effect of the Mexican share on permit growth appears as the repatriation intensifies between 1930 and 1934 and is concentrated in those years. In 1935–1936, growth rates for the number and value of permits turn temporarily positive; one explanation for this rebound is New Deal housing support, including the Home Owners' Loan Corporation (Fishback, 2017; Fishback et al., 2011; Courtemanche and Snowden, 2011). If so, this bias works against our estimates, suggesting that our decennial coefficients are a lower bound on the repatriation's impact on local construction activity. Despite this short-term recovery, the overall effect of the repatriation along the decade remains negative, as shown in Table 5.

C.5.2 Population Growth

A complementary concern is that pre-repatriation trends in non-Mexican population growth could be correlated with both the intensity of the repatriation and local housing market outcomes. To assess this, we re-estimate our baseline specifications with two additional sets of controls: (i) the decennial percent change in the county's non-Mexican working-age population over the three decades preceding 1930, i.e., 1900–1910, 1910–1920, and 1920–1930; and (ii) the long-term percent change in the same population between 1900 and 1930. Table C.5 reports the results. Across all three levels of aggregation (house, neighbor, city) and both pre-trend specifications, our estimates remain consistent with the baseline and statistically significant. Most notably, the house-level

Table C.5. Robustness to the inclusion of population growth pre-trends as additional controls. This table presents the results from estimating our baseline specifications with the inclusion of additional control variables for the population growth of non-Mexicans in a county. Panel A estimates a similar specification to Table 3 at the house level, Panel B uses the specification of Table 4, and Panel C of Table 5. In columns 1, 3, 5, and 7, we include as additional control variables a set of decennial percent changes in the population of the county between the three preceding decades from the repatriation, that is, from 1900 to 1910, 1910 to 1920, and 1920 to 1930. Columns 2, 4, 6, and 8 include the long-term population trend, as measured by the percent change between 1900 and 1930. The population in each county and decade accounts for only working-age adults (as previously defined) who do not live in group quarters and are not born in Mexico. All specifications include the controls and fixed effects from the baseline analysis. Standard errors in parentheses are clustered by state. Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel A. Effects on house-level prices

	House value change (%)				*Rent change (%)*			
	Mexican-Occupied		*U.S.-born-Occupied*		*Mexican-Occupied*		*U.S.-born-Occupied*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mexican Outflow	−12.40***	−14.57***	−1.277	−1.137	−21.51***	−14.37***	−8.922*	−9.182
	(2.952)	(2.011)	(1.299)	(1.285)	(1.995)	(1.074)	(4.603)	(5.113)
Observations	3,592	3,592	177,027	177,027	9,532	9,532	167,910	167,910
R-squared	0.026	0.024	0.001	0.001	0.012	0.014	0.003	0.003
Kleibergen–Paap *F*-statistic	628.65	861.51	217.14	173.54	479.65	466.77	131.05	106.06
Controls + Sector Shares + State FE	✓	✓	✓	✓	✓	✓	✓	✓
Decennial trends (1910, 1920, 1930)	✓		✓		✓		✓	
Long trend (1900–1930)		✓		✓		✓		✓

Panel B. Effects on neighbors

	House value change (%)				*Rent change (%)*			
	Mexican-Neighboring		*U.S.-born-Neighboring*		*Mexican-Neighboring*		*U.S.-born-Neighboring*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mexican Outflow	−3.400**	−4.608***	−1.59	−1.45	−8.074	−9.139	−9.681	−9.822
	(1.470)	(1.262)	(1.255)	(1.213)	(7.360)	(6.698)	(5.950)	(6.082)
Observations	2,516	2,516	119,662	119,662	3,712	3,712	107,268	107,268
R-squared	0.023	0.022	0.001	0.001	0.005	0.005	0.003	0.003
Kleibergen–Paap *F*-statistic	302.36	397.05	265.3	217.2	261.13	270.83	135.74	114.5
Controls + Sector Shares + State FE	✓	✓	✓	✓	✓	✓	✓	✓
Decennial trends (1910, 1920, 1930)	✓		✓		✓		✓	
Long trend (1900–1930)		✓		✓		✓		✓

Panel C. Effects on city-level prices

	Building Permit Change (Number)		*Building Permit Change (Value)*		House *Value Change*		*Rents Change*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mexican Outflow	−17.595*	−15.917	−5.254***	−4.368***	−1.780***	−1.794***	−1.258***	−1.188***
	(10.204)	(9.888)	(1.878)	(1.558)	(0.595)	(0.603)	(0.332)	(0.304)
Observations	236	236	192	192	868	868	868	868
R-squared	0.27	0.27	0.20	0.17	0.50	0.49	0.23	0.23
Kleibergen–Paap *F*-statistic	3243.1	2429.52	872.96	774.61	245.68	248.51	245.68	248.51
Controls + Sector Shares + State FE	✓	✓	✓	✓	✓	✓	✓	✓
Decennial trends (1910, 1920, 1930)	✓		✓		✓		✓	
Long trend (1900–1930)		✓		✓		✓		✓

coefficient on rents for Mexican-occupied addresses (Panel A) *increases* in absolute value after conditioning on pre-repatriation population trends, suggesting that the baseline estimates may have been conservative. We interpret this pattern as evidence that our results are not driven by differential pre-trends in non-Mexican population growth.

Appendix D Data Construction Details

D.1 Address Linking Approach: 1930–1940 Censuses

This section describes the procedure we adopted to construct a sample of matched houses between 1930 and 1940. Our goal is to match U.S. houses in 1930 to their records in the 1940 U.S. Census. Our source of U.S. houses and their addresses are from the IPUMS Restricted Complete Count Data (Ruggles et al., 2020). In addition to state and city, the main variables used in this linking approach are the street name and the house number of the household's street address, as written on the original census form.

Our approach to link addresses across censuses is similar in essence to previous approaches proposed in the literature of linking individuals across censuses (Abramitzky et al., 2021). The first study to match addresses across the 1930 and 1940 Censuses is Akbar et al. (2025). The authors perform the address matching for ten major northern U.S. cities, including New York, Philadelphia, Pittsburgh, Cleveland, Cincinnati, Columbus, Detroit, Chicago, Milwaukee, and Kansas City. Our procedure is similar to theirs in essence and follows four basic steps:

1. We remove observations with unidentifiable addresses, that are missing any component of the address: state, city, street name, or house number. We remove from the sample all housing units that are categorized as group quarters.

2. We standardize street names in the census, which are prone to typos and abbreviations. We standardize all the directional prefixes and street suffixes, convert ordinal street numbers to their cardinal text forms, remove special characters or punctuation, and remove any redundant information from street names. We also standardize house numbers by removing special characters or punctuation.

3. We restrict the sample to addresses that are unique by state, city, street name, and house number. We identified over 10 million unique U.S. addresses in 1930.

4. We match observations in 1930 to 1940 records using the following procedure. For each unique address in 1930, we search for an exact match by state, city, street name, and house number in 1940. If we find a unique match, we stop and consider the observations matched. If we find multiple matches for the same address, the observation is discarded to avoid a potential incorrect match. If there are no matches for an address from 1930 and the street name contains a suffix, we perform another search for an exact match in 1940, but excluding the street name's suffix. Observations that find multiple matches are discarded. If none of these attempts produces a unique match, the observation is also discarded.

The final sample contains only the addresses that can be successfully and uniquely matched between 1930 and 1940. This procedure generates a sample of over 4 million linked addresses, a matching rate of 41.7% of the identifiable addresses in 1930.

The approach we adopt has been shown to yield precise house links as seen from geo-coded blocks in northern U.S. cities by Akbar et al. (2025). For southern U.S. cities, Baerlocher, Cortes, and Sant'Anna (2025) validate our approach by comparing our house-level matches to modern street geo-locations using standard geo-coding sources for Los Angeles, CA.

We favor matching addresses over linking individuals across censuses (Feigenbaum, 2016; Abramitzky et al., 2021; Price et al., 2021) because our question concerns local out-migration shocks rather than individual trajectories. Because people are *mobile*, studying the changes in a given person's house prices or rents may be contaminated by selection into different regions, neighborhoods, or houses. By contrast, houses are *immobile*, allowing us to leverage the micro-level census data and avoid the selection issues that arise from the mobility of individuals.

D.2 Sample Restrictions for House-Level Analysis

Our full 1930–1940 matched address sample contains 4.03 million linked addresses spanning over 900 U.S. cities. We perform the following series of sample restrictions to obtain a clean sample for the house-level analysis.

In studying the effects on house values and rents, we exclude the addresses that report both an owner and a renter or that changed ownership status from 1930 to 1940. This is necessary because the Census only collected values for owner-occupied properties and only rents for those that were renter-occupied. We also exclude households with more than 10 members to avoid outliers and transcription errors. We obtain the percentage change in house value and rents from the self-reported values in the census between 1930 and 1940. In addresses with multiple households, we aggregate this information to the address level using the median house value and the median rent reported by the households living in the same address.

Finally, we restrict the house-level sub-sample to states with a sufficiently large Mexican population: states where Mexican immigrants constituted at least 0.25% of the urban workforce in 1930 (AZ, TX, CA, NM, KS, CO, WY, UT, ID, NE, and IN). This ensures geographic comparability and consistency with our state-level fixed effects and clustering. Our estimates are robust to using the complete sample without this restriction.

We use the IPUMS "City" variable to identify cities consistently across 1930 and 1940. This variable covers most, but not all, of the cities present in both censuses. Cities not identifiable in both census years are excluded from the sample, yielding a baseline sample of 868 cities for the city-level analysis.

D.3 Identifying Next-Door Neighbors

To study spillover effects on the neighbors of Mexican-occupied houses (Section 5.2), we must determine which houses are adjacent to one another using only the historical census information. We follow Logan and Parman (2017), Eriksson and Ward (2019), and Quincy (2022) and use the order in which households were registered in the original census records—combined with the technical variables of the 1930 Census—to define next-door neighbors. The strategy takes advantage of how Census takers were instructed to collect information at the time by walking down one side of a street at a time, recording households sequentially.

Since our main goal is to study housing rather than households, we adapt this approach to identify not the next-door household but the next-door *address*. Combining this with our sample of linked addresses, we construct two samples of houses: (1) houses with at least one Mexican-occupied immediate next-door neighbor in 1930, and (2) houses with no Mexican-occupied immediate next-door neighbors. We then estimate the specification in Eq. (7) for each sample separately.

D.4 Building Permit Data

We use two distinct series of building permits as measures of city-level housing and construction activity. Building permits must be filed with local authorities before any construction occurs; changes in permit activity therefore reflect both current construction trends and expectations about future housing demand (Cortes and Weidenmier, 2019; Cortes and LaPoint, 2024).

Value of building permits. The first series is the total value of building permits, taken from issues of *Dun & Bradstreet's Review*, a well-known business and financial publication in the 1920s and 1930s (see, e.g., Cortes and Weidenmier, 2019; Cortes and LaPoint, 2024). The value of building permits represents the cost of new commercial and residential buildings for 215 cities across the U.S. issued to prospective builders within the corporate limits of the cities. They include new residential and non-residential buildings and additions, alterations, and repairs, excluding land costs. The data are compiled from reports furnished monthly to *Dun & Bradstreet, Inc.* by the building departments of the various cities.

Number of building permits. The second series is the number of building permits collected by Snowden (2006) from issues of the *Bulletin of the Bureau of Labor Statistics*. These permits refer to single-family houses authorized for construction in 250 cities. Because the number series refers specifically to single-family residential units while the value series captures all commercial and residential construction, the two series reflect different aspects of the construction market and are not directly comparable. This fundamental difference precludes us from constructing a permit-based price index (e.g., dividing the value series by the number series); we therefore rely on Census median house values as our primary measure of house prices.

Both permit series are measured over the 1930–1940 decade, consistent with the Census endpoints of our main analysis and stopping before World War II and the Bracero Program introduced confounding shifts in construction activity.