Closing the Golden Door: The Restrictionist Mirage of the 1924 Johnson-Reed Act

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Abstract

The 1924 Johnson-Reed Act promised American workers occupational gains by curtailing Southern and Eastern European immigration. We test this claim using IPUMS linked census panels of 10.1 million native-born men across 3,039 counties. Exploiting cross-county variation in pre-quota immigrant concentration, we find a precisely estimated null effect on occupational upgrading ($\hat{\beta} = -1.35$, SE = 0.95). The data are inconsistent with the promised labor market benefits. They do, however, reveal a hidden cost: young workers (aged 18–35) in high-exposure counties were significantly less likely to become homeowners ($\hat{\beta} = -0.27$, $p < 0.01$), with similar effects for urban residents. A placebo analysis of 1910–1920 reveals pre-restriction complementarity (full specification: $\hat{\beta} = +17.9$, $p < 0.01$), suggesting restriction disrupted productive immigrant–native relationships. The restrictionist promise was a mirage.

JEL Codes: J15, J61, N32, J62, R23

Keywords: Immigration restriction, Johnson-Reed Act, occupational mobility, homeownership, complementarity, age of mass migration

*APEP is maintained at https://github.com/SocialCatalystLab/ape-papers. This paper is a revision of APEP-0626. See https://github.com/SocialCatalystLab/ape-papers/tree/main/apep_0626_v1 for the original. Correspondence: scl@econ.uzh.chmailto:apep@socialcatalystlab.orgapep@socialcatalystlab.org

1 Introduction

"The day of unalloyed welcome to all peoples, the day of indiscriminate acceptance of all races, has definitely ended," declared Representative Albert Johnson upon passage of the 1924 Immigration Act that bears his name.[1] The Johnson-Reed Act imposed severe national-origin quotas that reduced immigration from Southern and Eastern Europe by over 80 percent virtually overnight. Its architects made an explicit economic promise: by stemming the tide of low-skill immigrants, American workers would ascend the occupational ladder. Wages would rise. Native livelihoods would improve. Nearly a century later, this paper asks whether that promise was kept.

The answer is no—and the truth is worse than a broken promise. Using linked census panels from IPUMS covering 10.1 million native-born men across 3,039 counties, we find that the 1924 restriction delivered no measurable occupational gains to native workers. Counties that lost the most immigrants to the quotas saw no greater occupational upgrading than counties that were barely affected. The estimated effect on occupational income score change is -1.35 points (SE = 0.95), statistically indistinguishable from zero and, if anything, slightly negative. This null persists across specifications that progressively add state fixed effects, individual controls, and 1920 occupation fixed effects.

But the restriction was not merely ineffective—it was costly. This paper's central contribution is documenting what we call the *restrictionist mirage*: the gap between the promised benefits and the actual consequences of immigration restriction. Beyond the occupational null, we uncover a significant negative effect on homeownership transitions. Native-born men in high-exposure counties were significantly less likely to become homeowners between 1920 and 1930. The effect concentrates separately among young workers aged 18–35 ($\hat{\beta} = -0.271$, $p < 0.01$) and among urban residents ($\hat{\beta} = -0.270$, $p < 0.01$)—precisely the populations that restrictionists claimed to be protecting. Rural counties show no effect whatsoever.

Why would removing immigrants reduce native homeownership? Our leading explanation

[1] Congressional Record, 68th Congress, 1st Session, April 1924.

draws on complementarity. Immigrants in this era disproportionately filled manual and construction labor roles, supporting housing supply and local economic activity that facilitated native asset accumulation. When the quotas abruptly curtailed this supply, the complementary benefits evaporated. Suggestive evidence supports this channel: a placebo analysis of 1910–1920—the decade *before* restriction—reveals that counties with greater immigrant presence experienced significantly *more* native occupational upgrading ($\hat{\beta} = +17.9$, $p < 0.01$). The positive pre-period relationship is the mirror image of the null-to-negative post-period effect, consistent with a story in which restriction disrupted productive immigrant–native complementarities rather than removing competitive pressures.

This paper contributes to three literatures. First, it advances the economic history of immigration restriction. Goldin (1994) analyzes the political economy that produced the 1924 Act but does not estimate its labor market consequences. Tabellini (2020) studies the effects of immigration inflows on natives during the age of mass migration, finding that political backlash coexisted with modest economic benefits. We study the reverse experiment—an abrupt reduction in immigration—and find that the promised occupational gains never materialized. Sequeira et al. (2020) document long-run benefits of historical immigration at the county level; we show that even in the short run, the case for restriction was empirically hollow.

Second, the paper engages the modern debate on immigration and native labor markets. Borjas (2003) emphasizes substitution effects, while Ottaviano and Peri (2012) and Card (2009) stress complementarity and heterogeneous impacts across skill groups. Dustmann et al. (2016) survey why studies reach different conclusions. Our historical setting offers a rare opportunity: a large, sudden, policy-driven reduction in immigration that generates sharp cross-sectional variation. The null occupational effect and the positive pre-period relationship are more consistent with the complementarity view of Ottaviano and Peri (2012) and Peri (2016) than with the substitution framework.

Third, we contribute to the literature on local labor market adjustment. Moretti (2011) emphasizes that local shocks propagate through housing markets, migration, and occupational reallocation. We document that the immigration shock operated primarily through the housing channel—not through the occupational channel that restrictionists invoked. Boustan (2010) and Hornbeck (2012) study native mobility responses to local demographic shocks; we find limited geographic sorting in response to restriction, suggesting that the effects we estimate reflect in-situ adjustment rather than selective migration.

Our identification strategy exploits cross-county variation in the pre-quota share of residents born in countries subject to severe restrictions under the 1924 Act—primarily Italy, Russia, Poland, Austria-Hungary, and Czechoslovakia. This *quota exposure* measure captures the local intensity of the immigration shock: counties with larger pre-existing immigrant communities from restricted origins experienced proportionally greater declines in immigrant inflows. The first-stage relationship is strong ($\hat{\beta} = -0.266$, $t > 80$). We estimate individual-level regressions with state and 1920 occupation fixed effects, two-way clustered standard errors (state and county), and a battery of demographic controls. Robustness checks include leave-one-origin-out analysis, state-level clustering, restriction to non-movers, and an alternative exposure measure using 1910 immigrant shares.

The paper proceeds as follows. Section 2 describes the historical context and the explicit economic promises of immigration restriction. Section 3 details the linked census data and variable construction. Section 4 presents the empirical strategy. Section 5 tests whether restriction delivered occupational gains. Section 6 documents the hidden homeownership cost. Section 7 presents the pre-restriction complementarity evidence. Section 8 provides robustness checks. Section 9 discusses mechanisms and implications. Section 10 concludes.

2 Historical Context and the Restrictionist Promise

The decades preceding the Johnson-Reed Act witnessed the largest wave of immigration in American history. Between 1880 and 1920, over 23 million immigrants arrived in the

United States, increasingly from Southern and Eastern Europe—Italy, Russia, Poland, Austria-Hungary, and the Balkans (Abramitzky and Boustan, 2017). These "new immigrants" differed visibly from earlier Northern European arrivals in language, religion, and, in the eyes of nativists, racial fitness (Higham, 1955; King, 2000).

The economic case for restriction rested on a simple labor-market argument: immigrants and natives competed for the same jobs, and the influx of low-skill foreigners depressed wages and blocked occupational advancement for American workers. The Dillingham Commission's 41-volume report, published in 1911, codified this view, arguing that new immigrants were less skilled, less assimilable, and more economically harmful than their predecessors (United States Immigration Commission, 1911). Though subsequent scholarship has challenged nearly every empirical claim of the Commission (Abramitzky et al., 2014; Abramitzky and Boustan, 2022), its framework shaped the political debate for a generation.

The legislative response built gradually. The Literacy Test Act of 1917 required immigrants to demonstrate reading ability, but proved insufficient to stem Southern and Eastern European flows. The Emergency Quota Act of 1921 imposed the first numerical limits, pegged to 3 percent of each nationality's 1910 population. The 1924 Johnson-Reed Act tightened these dramatically: quotas dropped to 2 percent of the 1890 population base, a deliberate choice that virtually eliminated immigration from Southern and Eastern Europe while preserving flows from Britain, Germany, and Scandinavia (Ngai, 2004; Hatton, 2011).

The Act's supporters made their economic logic explicit. Senator David Reed argued that restriction would "protect the American standard of living" and "preserve job opportunities for American citizens." The American Federation of Labor endorsed the quotas precisely because it expected them to improve native workers' bargaining position (Goldin, 1994). The promise was concrete and testable: with fewer immigrant competitors, American workers should have experienced occupational upgrading.

The 1924 Act provides an unusually clean natural experiment. First, the quotas created dramatic cross-national variation in restriction intensity. Italian immigration fell by over

90 percent; British immigration was barely affected. Second, the policy was implemented rapidly—quotas took effect on July 1, 1924—leaving little time for anticipatory adjustment. Third, the quotas were binding for the entire 1920–1930 intercensal period, generating a sustained treatment. Fourth, because immigrants had settled unevenly across counties during the pre-quota era, the national-origin quotas generated sharp cross-county variation in local exposure to the immigration shock.

3 Data and Measurement

3.1 Linked Census Panels

We construct individual-level linked panels using the IPUMS Multigenerational Longitudinal Panel (MLP) version 2 (Ruggles et al., 2024). The MLP links individuals across decennial census waves using a combination of name, age, birthplace, and household characteristics, applying the ABE linking methodology refined in Abramitzky et al. (2021).

Our primary sample links native-born men aged 18–55 in 1920 to their records in the 1930 census, yielding 10,096,489 linked observations.[2] For the placebo analysis, we construct a parallel panel linking the 1910 census to 1920, producing 8,152,133 linked observations. The MLP's probabilistic matching is conservative, prioritizing match precision over recall.

For each linked individual, we observe county of residence, occupation, and demographic characteristics in both census years. The key outcome variable is the change in OCCSCORE—the IPUMS occupational income score that assigns the median total income of workers in each occupation based on the 1950 Census—between censuses. A positive change indicates occupational upgrading.

[2] We restrict to men because female labor force participation was low and occupational coding less reliable in this era. Native-born status is defined by birthplace in the United States. One observation is dropped in some specifications due to a singleton county fixed-effect cell, yielding $N = 10{,}096{,}488$ in most regression tables.

3.2 Quota Exposure Measure

Our treatment variable, *quota exposure*, measures the county-level share of the 1920 population born in countries subject to severe quotas under the 1924 Act:

$$\text{Exposure}_c = \frac{\sum_{o \in \mathcal{R}} \text{Pop}_{c,o,1920}}{\text{Pop}_{c,1920}} \tag{1}$$

where $\mathcal{R}$ denotes restricted-origin countries: Italy, Russia, Poland, Austria, Hungary, and Czechoslovakia—the six nationalities that accounted for the vast majority of the quota-constrained decline. We also construct a broader measure that adds Greece, Romania, Yugoslavia, and the Baltic states.

The exposure measure varies substantially across the 3,039 counties in our sample, with a mean of 4.4 percent and a standard deviation of 4.3 percentage points.[3] High-exposure counties are concentrated in the industrial Northeast and Upper Midwest—the historical destinations of Southern and Eastern European immigrants. Low-exposure counties are predominantly rural and Southern.

3.3 Additional Outcome Variables

Beyond OCCSCORE changes, we construct several additional outcomes to capture different dimensions of labor market and housing adjustment:

- **Upgraded**: Indicator for moving to a higher-OCCSCORE occupation between census waves.
- **Farm Exit**: Among 1920 farm workers, indicator for transitioning to a non-farm occupation.
- **Moved**: Indicator for residing in a different county in the two census years.

[3] Table 1 reports means by quartile. The county-level first-stage regression (Table 7) uses 3,036 counties; three small counties are dropped due to missing change data in the county-level aggregation.

- **Δ SEI**: Change in Duncan Socioeconomic Index, an alternative occupational ranking.
- **Became Owner**: Indicator for transitioning from renter in the base year to homeowner in the follow-up year.
- **Ladder Up**: Categorical indicator for upward movement along a coarse occupational ladder (unskilled $\rightarrow$ semi-skilled $\rightarrow$ skilled $\rightarrow$ white collar $\rightarrow$ professional).
- **Self-Employment**: Indicator for transition to self-employment.

Table 1 presents summary statistics by quartile of quota exposure. Several patterns are worth noting. Higher-exposure counties are more urban, more literate, and have higher baseline OCCSCORE—reflecting the fact that immigrants settled in economically dynamic areas. Raw OCCSCORE gains are actually *smaller* in higher-exposure quartiles (+1.17 points in Q4 vs. +2.07 in Q1), a pattern driven by composition (urban workers have less room to upgrade) rather than a causal effect. The econometric strategy below addresses this directly.

4 Empirical Strategy

4.1 Baseline Specification

We estimate individual-level regressions of the form:

$$\Delta Y_{i,c,s} = \beta \cdot \text{Exposure}_c + \mathbf{X}_i' \gamma + \alpha_s + \delta_{o(i,t_0)} + \varepsilon_{i,c,s} \tag{2}$$

where $\Delta Y_{i,c,s}$ is the change in outcome for individual i in county c and state s between census years; Exposure_c is defined in equation (1); $\mathbf{X}_i$ is a vector of individual controls (age, age squared, literacy, urban status, log county population); α_s are state fixed effects; and $\delta_{o(i,t_0)}$ are 1920 occupation fixed effects. Standard errors are two-way clustered by state and county.

Table 1: Summary Statistics by Quota Exposure Quartile

	Exposure Quartile				
	Q1 (Low)	Q2	Q3	Q4 (High)	Overall
N	2,528,647	2,521,851	2,542,711	2,503,280	10,096,489
Mean Quota Exposure	0.002	0.019	0.048	0.107	0.044
OCCSCORE (1920)	18.8	22.6	24.7	27.5	23.4
OCCSCORE (1930)	20.8	24.4	26.2	28.6	25.0
Δ OCCSCORE	2.07	1.81	1.52	1.17	1.64
Share Upgraded	0.329	0.341	0.338	0.334	0.335
Share Farm (1920)	0.609	0.365	0.261	0.113	0.337
Farm Exit Rate	0.159	0.113	0.080	0.034	0.097
Share Moved	0.117	0.127	0.115	0.097	0.114
Share Literate	0.916	0.985	0.995	0.996	0.973
Mean Age (1920)	34.1	34.1	33.7	32.5	33.6

Note:
Sample: Native-born men aged 18–55 in 1920 with valid occupations in both census years,
linked across the 1920 and 1930 censuses via IPUMS MLP v2. Quota exposure is the county-level
share of 1920 population born in restricted-origin countries (Italy, Russia, Poland, Austria,
Hungary, Czechoslovakia). OCCSCORE is the IPUMS occupational income score.

The coefficient β captures the differential change in outcomes for natives in counties with greater pre-quota immigrant presence. Under the restrictionist hypothesis, β should be positive and significant: native workers in counties that lost the most immigrants should have experienced the greatest occupational gains.

4.2 Identification

Our design exploits cross-county variation in exposure to the 1924 quotas, driven by historical immigrant settlement patterns. The key assumption is that exposure is as-if-randomly assigned conditional on state fixed effects and initial occupation—that is, counties' pre-quota immigrant composition reflects historical settlement decisions rather than 1920–1930 economic conditions.

First, the quotas were determined at the national level based on 1890 population shares, not on the economic conditions of any particular county. Local exposure is driven by historical settlement patterns of immigrants who arrived decades before the restriction, plausibly exogenous to county-specific 1920–1930 trends.[4]

Second, we examine the 1910–1920 pre-period. As we show in Section 7, the pre-period relationship between exposure and native occupational gains is positive and significant. The standard parallel-trends condition does not hold: high-exposure counties were already experiencing faster occupational upgrading before 1924, consistent with immigrant–native complementarity. Crucially, this positive pre-trend *biases against* our main finding. Under the restrictionist hypothesis, the pre-existing advantage of high-exposure counties should have persisted or grown after restriction removed competition; instead, it vanishes. Our post-period estimates are thus conservative: any omitted trending confounder that produced the pre-period pattern would, if anything, push the post-period coefficient *upward*, making our null-to-negative finding harder to explain without restriction having disrupted the complementarity.

Third, we verify the first stage: counties with greater quota exposure experienced proportionally larger declines in the share of restricted-origin foreign-born residents (Table 7). The relationship is tight, with an R^2 of 0.80 in the county-level regression.

4.3 Threats to Identification

The primary concern is that quota exposure correlates with other county-level trends that independently affected native outcomes. We address this in several ways. State fixed effects absorb state-level shocks (e.g., differential industrialization across regions). Occupation fixed effects in the 1920 base year ensure we compare workers starting from the same occupational position. Leave-one-origin-out analysis (Section 8) verifies that results are not driven by any single nationality group. Restriction to non-movers rules out compositional changes from

[4]This is the standard shift-share logic applied in immigration research (Card, 2001). The "share" is pre-determined by historical settlement, and the "shift" comes from the nationally imposed quota.

selective migration.

5 Did Restriction Deliver Occupational Gains?

5.1 Main Results

Table 2 presents the central finding. Column (1) reports the unconditional relationship between quota exposure and OCCSCORE change: counties with greater immigrant presence saw less occupational upgrading ($\hat{\beta} = -7.52$, $p < 0.01$). This raw correlation, however, reflects composition—urban, high-exposure counties had higher baseline OCCSCORE and less room for gains—not a causal effect.

Adding state fixed effects in column (2) barely changes the estimate ($\hat{\beta} = -8.26$). Individual controls in column (3) reduce the coefficient substantially to -1.48 (SE = 0.91), statistically insignificant. The full specification in column (4) adds 1920 occupation fixed effects, yielding our preferred estimate: $\hat{\beta} = -1.35$ (SE = 0.95). The coefficient is negative, small, and statistically indistinguishable from zero. Column (5) uses the broader exposure measure with identical results ($\hat{\beta} = -0.59$, SE = 0.66).

The progression from columns (1) through (4) is informative. The large negative raw correlation disappears entirely once we account for the compositional differences between high- and low-exposure counties. Workers starting from the same occupation in the same state showed no differential change in occupational income regardless of how many immigrants their county lost. The occupational gains that restrictionists promised simply did not materialize.

5.2 Occupational Transitions and Alternative Measures

Table 3 extends the null result across multiple alternative measures of occupational mobility. The probability of upgrading to a higher-OCCSCORE occupation is not significantly affected by quota exposure ($\hat{\beta} = -0.067$, SE = 0.045). Farm exit rates show no response ($\hat{\beta} = 0.009$, SE = 0.128). Geographic mobility is unaffected ($\hat{\beta} = -0.019$, SE = 0.101). The change in

Table 2: Effect of Quota Exposure on Occupational Mobility, 1920–1930

Dependent Variable:	delta_occscore				
	(1)	(2)	(3)	(4)	(5)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Constant	1.972***				
	(0.0621)				
Quota Exposure	-7.516***	-8.262***	-1.476	-1.352	
	(1.419)	(1.608)	(0.9097)	(0.9471)	
Age			-0.4317***	-0.0093	-0.0092
			(0.0183)	(0.0096)	(0.0096)
I(I(age_1920$\hat{2}$))			0.0044***	-0.0005***	-0.0005***
			(0.0002)	(0.0001)	(0.0001)
Literate			0.9856***	2.405***	2.405***
			(0.0553)	(0.0699)	(0.0699)
Urban			-2.556***	2.430***	2.431***
			(0.0713)	(0.0598)	(0.0599)
Log(Population)			-0.0384	0.2628***	0.2530***
			(0.0254)	(0.0195)	(0.0210)
Quota Exposure (Broad)					-0.5875
					(0.6598)
Fixed-effects					
statefip_1920		Yes	Yes	Yes	Yes
occ1950_1920				Yes	Yes
Fit statistics					
Observations	10,096,489	10,096,489	10,096,489	10,096,488	10,096,488
R^2	0.00125	0.00276	0.03709	0.24530	0.24529
Within R^2		0.00081	0.03521	0.01159	0.01158

Clustered (statefip_1920 & countyicp_1920) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variable is change in OCCSCORE between 1920 and 1930.
Quota exposure is the county-level share of 1920 population born in countries subject to severe quota restrictions under the 1924 Johnson-Reed Act. Column (5) uses a broader definition including Greece, Romania, Yugoslavia, and Baltic states. Standard errors two-way clustered by state and county in parentheses.

Duncan Socioeconomic Index confirms the OCCSCORE null ($\hat{\beta} = -0.174$, SE $= 1.291$).

The occupational null is not an artifact of a particular outcome measure. Across continuous indices, categorical indicators, and sector-specific transitions, we consistently find that immigration restriction had no detectable effect on native occupational advancement. Appendix Table 10 reports additional categorical outcomes. The ladder-up indicator—capturing movement across coarse occupational categories from unskilled through professional—yields a small but significant negative effect ($\hat{\beta} = -0.095$, SE $= 0.027$), suggesting that restriction may have marginally *impeded* rather than facilitated categorical occupational upgrading. Self-employment transitions are unaffected ($\hat{\beta} = -0.011$, SE $= 0.033$).

Figure 1 displays the distribution of OCCSCORE changes by exposure quartile, showing extensive overlap and no systematic rightward shift for high-exposure counties.

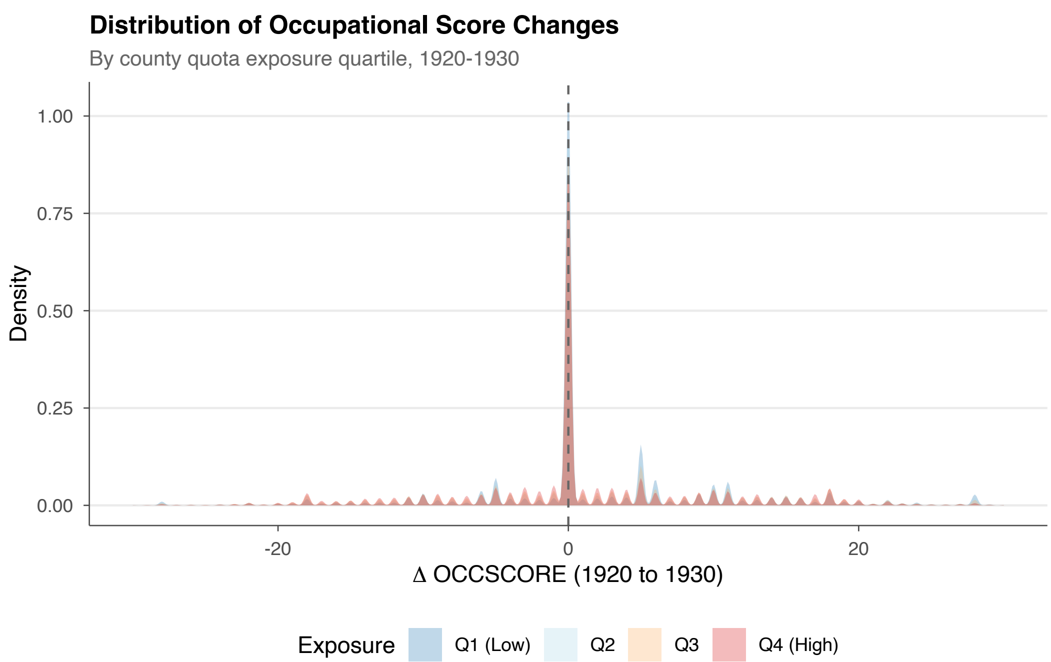

Figure 1: Distribution of Δ OCCSCORE by Quota Exposure Quartile
Notes: Kernel density estimates of the change in occupational income score between 1920 and 1930, by quartile of county-level quota exposure. Vertical lines indicate quartile means. The distributions overlap substantially, consistent with the null treatment effect in Table 2.

Table 3: Alternative Outcomes: Occupational Mobility, Migration, and Homeownership

Dependent Variables:	upgraded	farm_exit	moved	delta_sei	became_owner
	Upgraded	Farm Exit	Moved	Δ SEI	Became Owner
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Quota Exposure	-0.0667	0.0088	-0.0191	-0.1741	-0.2226***
	(0.0450)	(0.1276)	(0.1013)	(1.291)	(0.0689)
age_1920	-0.0088***	-0.0256***	-0.0019***	-0.0028	0.0200***
	(0.0003)	(0.0013)	(0.0007)	(0.0199)	(0.0010)
I(I(age_1920$\hat{2}$))	$6.08 \times 10^{-5***}$	0.0003***	-6.54×10^{-6}	-0.0011***	-0.0003***
	(4.17×10^{-6})	(1.55×10^{-5})	(7.18×10^{-6})	(0.0003)	(1.29×10^{-5})
literate	0.1106***	0.0442***	0.0106***	4.947***	0.0136***
	(0.0045)	(0.0044)	(0.0027)	(0.1471)	(0.0028)
urban	0.0806***		0.0312***	2.991***	0.0795***
	(0.0028)		(0.0028)	(0.0894)	(0.0051)
log_pop	0.0087***	0.0231***	-0.0031*	0.5897***	0.0113***
	(0.0008)	(0.0038)	(0.0016)	(0.0386)	(0.0018)
Fixed-effects					
statefip_1920	Yes	Yes	Yes	Yes	Yes
occ1950_1920	Yes		Yes	Yes	Yes
Fit statistics					
Observations	10,096,488	3,406,565	10,096,488	10,096,488	10,096,488
R^2	0.23437	0.04678	0.05804	0.19005	0.02316
Within R^2	0.01452	0.02979	0.00563	0.00792	0.00920

Clustered (statefip_1920 & countyicp_1920) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Each column estimates a separate regression of the indicated outcome on quota exposure. All specifications include state FE, individual controls, and standard errors two-way clustered by state and county. 1920 occupation FE included in all columns except Farm Exit (Col 2), which omits them because farm workers share a single 1920 occupation code. Farm Exit restricted to 1920 farm workers.
Became Owner indicates transition from renter to owner between 1920 and 1930.

5.3 Heterogeneous Effects

Table 4 asks whether the null masks heterogeneous responses across subgroups. It does not. The effect is insignificant for white workers ($\hat{\beta} = -1.47$, SE $= 0.93$), Black workers ($\hat{\beta} = -2.94$, SE $= 2.04$), urban workers ($\hat{\beta} = -1.54$, SE $= 1.10$), and rural workers ($\hat{\beta} = 0.34$, SE $= 1.31$). If anything, the point estimates are uniformly negative, suggesting a tendency toward harm rather than help. But no subgroup experienced statistically significant occupational gains from restriction.

6 The Hidden Cost: Homeownership and Local Dynamism

While restriction failed to deliver occupational benefits, the final column of Table 3 reveals an unexpected cost. Using the full sample (where the outcome is coded as 1 for any individual who owned their home in 1930 but not in 1920, and 0 otherwise), the probability of transitioning to homeownership was significantly lower in high-exposure counties ($\hat{\beta} = -0.223$, SE $= 0.069$, $p < 0.01$). This is the only outcome in Table 3 that achieves statistical significance, and it is negative—the opposite of what restriction advocates would have predicted.

6.1 Decomposing the Homeownership Effect

Table 5 decomposes this finding along several dimensions. Columns (1)–(3) distinguish between transitions into and out of homeownership. Among 1920 renters, the probability of becoming a homeowner by 1930 was substantially lower in high-exposure counties ($\hat{\beta} = -0.835$, SE $= 0.328$). Among 1920 homeowners, there was no significant increase in the probability of losing one's home ($\hat{\beta} = 0.041$, SE $= 0.151$).[5] The net ownership transition is negative but imprecisely estimated ($\hat{\beta} = -0.169$, SE $= 0.147$). The homeownership effect thus operates

[5] Coefficients in text are rounded from the precise table values: $0.0408 \to 0.041$, $-0.2226 \to -0.223$, etc.

Table 4: Heterogeneous Effects by Race and Location

Dependent Variable:	delta_occscore			
	White	Black	Urban	Rural
Model:	(1)	(2)	(3)	(4)
Variables				
Quota Exposure	-1.470	-2.937	-1.536	0.3430
	(0.9297)	(2.039)	(1.104)	(1.308)
Age	-0.0006	0.0101	0.0593***	-0.1645***
	(0.0103)	(0.0125)	(0.0064)	(0.0121)
I(I(age_1920$\hat{2}$))	-0.0006***	-0.0006***	-0.0013***	0.0013***
	(0.0001)	(0.0002)	(8.96×10^{-5})	(0.0001)
Literate	2.565***	0.7745***	3.295***	1.883***
	(0.0662)	(0.0498)	(0.0828)	(0.0524)
Urban	2.556***	1.423***		
	(0.0563)	(0.0739)		
Log(Population)	0.2913***	0.4489***	0.2556***	0.2714***
	(0.0198)	(0.0427)	(0.0228)	(0.0455)
Fixed-effects				
statefip_1920	Yes	Yes	Yes	Yes
occ1950_1920	Yes	Yes	Yes	Yes
Fit statistics				
Observations	9,517,281	565,708	6,689,923	3,406,555
R^2	0.24863	0.29053	0.25881	0.16970
Within R^2	0.01164	0.01316	0.00488	0.01250

Clustered (statefip_1920 & countyicp_1920) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variable is change in OCCSCORE (1920–1930).
All columns include state and 1920 occupation fixed effects, and individual controls.
Standard errors two-way clustered by state and county.

through a *blocked entry* channel: restriction made it harder for renters to become owners, rather than causing existing owners to lose their homes.

Table 5: Homeownership Mechanism Decomposition

Dependent Variables:	became_owner	lost_home	net_owner	became_owner			
	Became Owner (Renters)	Lost Home (Owners)	Net Ownership	Young (18-35)	Old (36-55)	Urban	Rural
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
Quota Exposure	-0.8345**	0.0408	-0.1687	-0.2706***	-0.1340*	-0.2700***	0.0035
	(0.3284)	(0.1508)	(0.1472)	(0.0750)	(0.0758)	(0.0615)	(0.1026)
age_1920	0.0163***	-0.0326***	0.0421***	0.0473***	-0.0091***	0.0225***	0.0135***
	(0.0008)	(0.0006)	(0.0016)	(0.0016)	(0.0007)	(0.0008)	(0.0011)
I(I(age_1920$\hat{2}$))	-0.0002***	0.0003***	-0.0005***	-0.0008***	5.88×10^{-5}***	-0.0003***	-0.0002***
	(1.1×10^{-5})	(7.83×10^{-6})	(2.01×10^{-5})	(2.96×10^{-5})	(8.49×10^{-6})	(1.03×10^{-5})	(1.6×10^{-5})
literate	0.1151***	-0.0558***	-0.0331***	0.0275***	0.0034	0.0260***	0.0038
	(0.0044)	(0.0056)	(0.0045)	(0.0037)	(0.0026)	(0.0031)	(0.0036)
urban	0.0100	0.0069***	0.1532***	0.0837***	0.0581***		
	(0.0069)	(0.0023)	(0.0076)	(0.0056)	(0.0046)		
log_pop	-0.0036	-0.0023	0.0222***	0.0098***	0.0131***	0.0126***	0.0092***
	(0.0046)	(0.0024)	(0.0031)	(0.0018)	(0.0019)	(0.0016)	(0.0030)
Fixed-effects							
statefip_1920	Yes	Yes	Yes	Yes	Yes	Yes	Yes
occ1950_1920	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	4,959,955	4,988,075	10,096,488	5,971,824	4,124,659	6,689,923	3,406,555
R^2	0.04590	0.09438	0.04064	0.02706	0.02242	0.01156	0.01727
Within R^2	0.00954	0.05551	0.01982	0.01088	0.00547	0.00640	0.00691

Clustered (statefip_1920 & countyicp_1920) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Cols 1-3 decompose net homeownership transitions. Col 1: among 1920 renters, probability of becoming owner by 1930. Col 2: among 1920 owners, probability of losing ownership by 1930. Col 3: net transition (became minus lost).
Cols 4-7 estimate homeownership transitions by age and location.
All specifications include state and occupation FE, individual controls, and standard errors two-way clustered by state and county.

6.2 Who Was Harmed?

Columns (4)–(7) of Table 5 reveal sharp heterogeneity by age and urbanization. The homeownership effect is driven almost entirely by young workers aged 18–35 ($\hat{\beta} = -0.271$, SE $= 0.075$, $p < 0.01$) and urban residents ($\hat{\beta} = -0.270$, SE $= 0.062$, $p < 0.01$). Older workers aged 36–55 show a smaller, marginally significant effect ($\hat{\beta} = -0.134$, SE $= 0.076$). Rural areas show no effect whatsoever ($\hat{\beta} = 0.004$, SE $= 0.103$).

This pattern is consistent with a complementarity mechanism operating through local economic dynamism. Young urban workers were at the margin of homeownership entry, dependent on local labor demand and housing supply conditions. Immigrants in this era were heavily concentrated in construction, manufacturing, and service occupations that sustained urban economies (Abramitzky and Boustan, 2017). When the quotas removed these workers, the resulting reduction in economic activity and housing construction fell disproportionately on the young urbanites who were most sensitive to local conditions.

Figure 2 plots residualized homeownership transition rates by quintile of quota exposure, after partialing out state and 1920 occupation fixed effects, demographic controls, and urban status.

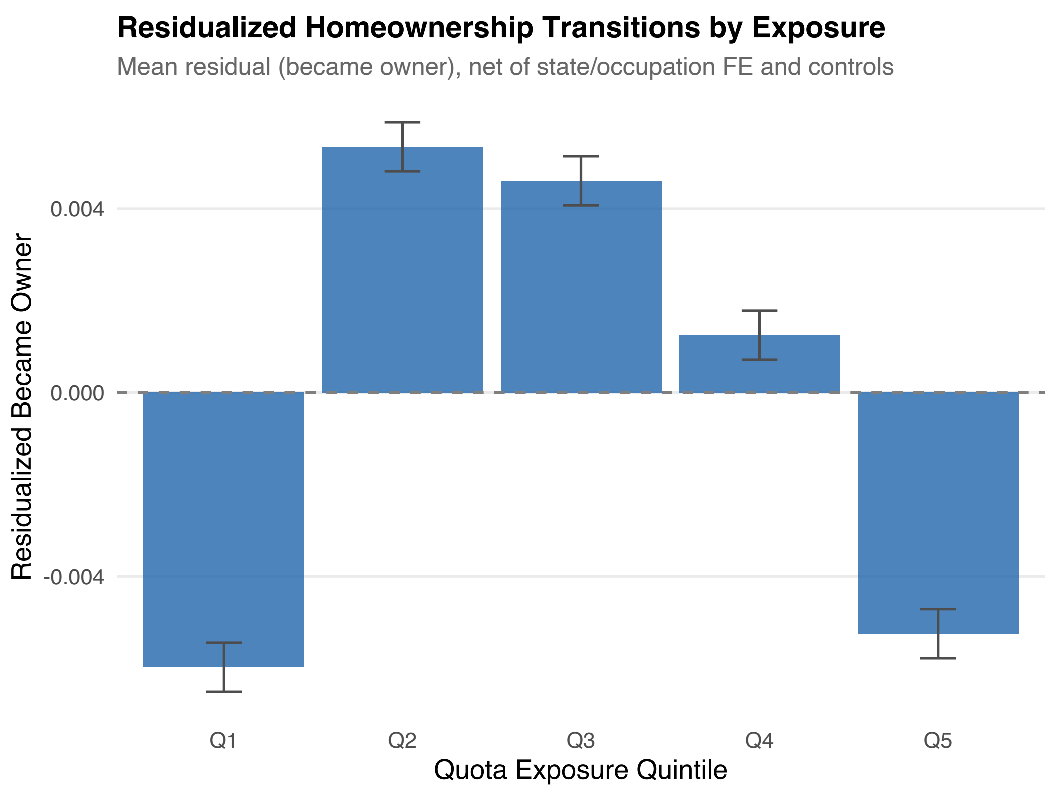

Figure 2: Residualized Homeownership Transition Rate by Quota Exposure Quintile
Notes: Mean residualized homeownership transition (became owner) by quintile of county-level quota exposure, after partialing out state and 1920 occupation fixed effects, age, literacy, urban status, and log county population. The highest-exposure quintile (Q5) shows the lowest residualized transition rate. The non-monotonic pattern across middle quintiles reflects heterogeneity in county composition that the regression specification absorbs through its full set of controls and fixed effects.

6.3 Selective Mobility

One concern is that the homeownership result could reflect selective migration: perhaps upwardly mobile natives left high-exposure counties, leaving behind a less dynamic population. We address this directly. First, the geographic mobility outcome in Table 3 is insignificant ($\hat{\beta} = -0.019$, SE = 0.101), indicating no differential out-migration from high-exposure counties. Second, when we interact the mobility indicator with a proxy for skill level (baseline OCCSCORE), we find that high-skill workers in high-exposure counties were *more* likely to stay ($\hat{\beta}_{\text{interaction}} = 0.131$, SE = 0.032, $p < 0.01$; Appendix Table 10, Column 3), the opposite of what a brain-drain story would predict. The homeownership effect appears to reflect genuine in-situ harm, not compositional change.

7 Pre-Restriction Complementarity

The homeownership results suggest that immigrants and natives were complements, not substitutes, in local labor markets. This section provides suggestive evidence by examining the pre-restriction period.

7.1 The Placebo as a Mirror

Table 6 estimates the relationship between quota exposure and native occupational changes between 1910 and 1920—the decade *before* the quotas took effect. The primary specification uses 1920 exposure, which reflects immigrant settlement patterns accumulated over decades of pre-restriction migration; column (2) verifies that the more historically predetermined 1910 exposure measure yields qualitatively similar results ($\hat{\beta} = +1.39$, SE = 0.43, $p < 0.01$). If restriction simply removed competitors, the pre-period relationship should be null or negative (more immigrants $\rightarrow$ worse native outcomes). Instead, we find the opposite.

Column (1) reports the baseline relationship without occupation fixed effects ($\hat{\beta} = +10.41$, SE = 2.46). Adding base-year (1910) occupation fixed effects in column (3) sharpens the

Table 6: Placebo Test: 1910–1920 (Pre-Quota Period)

Dependent Variables:	Δ OCCSCORE			Upgraded
	(1920 Exp.)	(1910 Exp.)	(Full Spec.)	(1920 Exp.)
Model:	(1)	(2)	(3)	(4)
Variables				
Quota Exposure (1920)	10.41***		17.90***	0.2828*
	(2.463)		(3.147)	(0.1431)
Age	-0.4982***	-0.5032***	-0.0497***	-0.0434***
	(0.0183)	(0.0186)	(0.0093)	(0.0023)
I(I(age_1910$\hat{2}$))	0.0053***	0.0053***	0.0002	0.0005***
	(0.0003)	(0.0003)	(0.0001)	(2.92×10^{-5})
Literate	0.3692***	0.3988***	2.008***	0.0142***
	(0.0409)	(0.0405)	(0.0856)	(0.0027)
Urban	-2.121***	-1.993***	2.430***	0.0176***
	(0.0643)	(0.0634)	(0.0896)	(0.0056)
Quota Exposure (1910)		1.390***		
		(0.4343)		
Fixed-effects				
statefip_1910	Yes	Yes	Yes	Yes
occ1950_1910			Yes	
Fit statistics				
Observations	8,152,133	8,152,133	8,152,133	8,152,133
R^2	0.03504	0.03385	0.24569	0.06580
Within R^2	0.03441	0.03322	0.01552	0.06404

Clustered (statefip_1910 & countyicp_1910) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Columns (1)–(3): dependent variable is the change in OCCSCORE between 1910 and 1920 (the decade before the 1924 quota). Column (4): binary upgraded indicator.
The large positive coefficients indicate that counties with greater immigrant presence experienced more native occupational upgrading before restriction—consistent with complementarity and opposite to the restrictionist substitution hypothesis. All columns include state FE and individual controls.
Standard errors two-way clustered by state and county.

estimate to +17.90 (SE = 3.15, $p < 0.01$), because within-occupation comparisons remove the compositional differences between high- and low-exposure counties that attenuate the raw correlation.[6] In standardized terms, a one-standard-deviation increase in quota exposure (4.3 percentage points) is associated with $17.9 \times 0.043 \approx 0.77$ additional OCCSCORE points—roughly half of the sample mean OCCSCORE change of 1.64 points (Table 1). Native workers in counties with more restricted-origin immigrants experienced substantially *greater* occupational upgrading in the decade before restriction. This large positive coefficient stands in stark contrast to the null-to-negative post-restriction effect of −1.35.

The juxtaposition is striking. During 1910–1920, when Southern and Eastern European immigrants were arriving freely, their presence was associated with native occupational gains. After 1924, when the quotas dramatically curtailed these flows, the gains disappeared. The simplest interpretation is that restriction destroyed a productive complementarity: immigrants filled manual and industrial roles that generated demand for native supervisory, clerical, and skilled positions, consistent with the task-specialization framework of Peri (2016) and the historical evidence in Sequeira et al. (2020).

7.2 Complementarity in Detail

Figure 3 presents the pre-period relationship as a binned scatterplot, showing a clear positive gradient between quota exposure and native OCCSCORE gains in 1910–1920.

Column (4) of Table 6 shows that the upgraded indicator also exhibits a positive pre-period relationship ($\hat{\beta} = 0.283$, SE = 0.143), though this is only marginally significant. Column (2) verifies that the 1910 distribution of immigrants—an even more historically predetermined measure—also predicts positive native outcomes.

Figure 4 presents these findings as a multi-wave event study, plotting the relationship between quota exposure and native occupational gains separately for 1910–1920 and 1920–

[6] The larger coefficient in column (3) reflects the well-known omitted variable bias formula: conditioning on 1910 occupation removes the negative correlation between exposure and within-occupation OCCSCORE ceilings, revealing the full extent of pre-period complementarity.

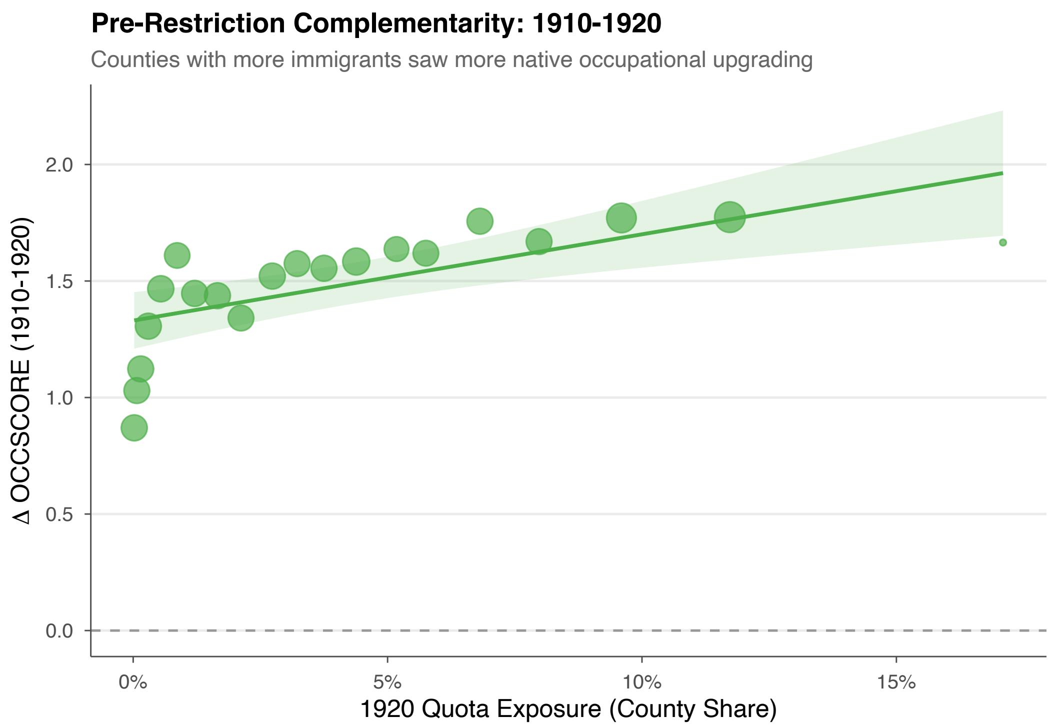

Figure 3: Pre-Restriction Complementarity: Quota Exposure and Native Occupational Gains, 1910–1920

Notes: Binned scatterplot of county-level mean Δ OCCSCORE (1910–1920) against quota exposure (1920), residualized on state fixed effects. Each point represents approximately equal numbers of observations. The positive slope reflects the complementarity between immigrants and native occupational upgrading documented in Table 6.

1930. The contrast between the positive pre-period and null post-period relationships is consistent with a complementarity interpretation.

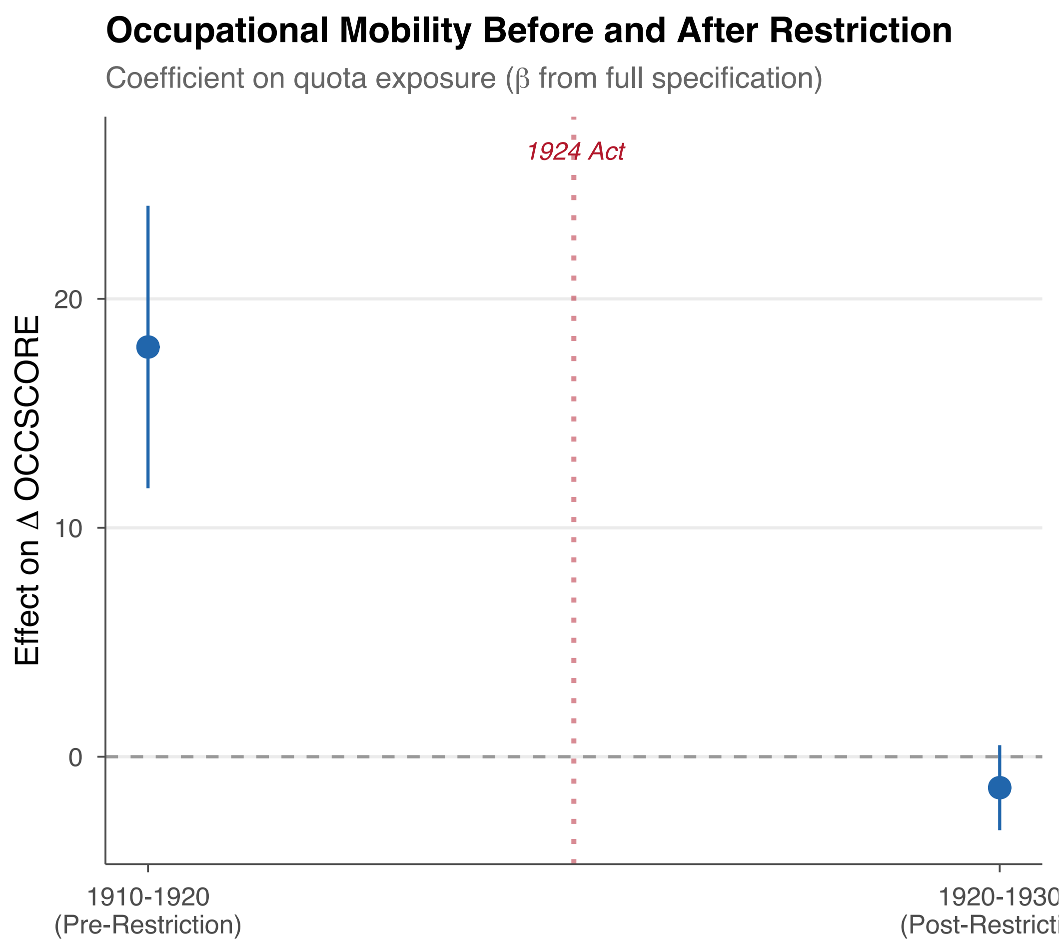

Figure 4: Multi-Wave Event Study: Quota Exposure and Native Occupational Gains
Notes: Coefficients from separate regressions of Δ OCCSCORE on quota exposure for two periods: 1910–1920 (pre-restriction) and 1920–1930 (post-restriction). Bars represent 95% confidence intervals. The positive pre-period coefficient and null post-period coefficient together constitute the "restrictionist mirage."

8 Robustness

8.1 First-Stage Evidence

Table 7 confirms the first stage of the natural experiment. In a county-level regression weighted by the number of linked individuals, a one-percentage-point increase in quota exposure is associated with a 0.266 percentage-point decline in the restricted-origin foreign-born share ($t > 80$, $R^2 = 0.80$). Column (2) shows that the total foreign-born share also declined significantly ($\hat{\beta} = -0.204$, $p < 0.01$), indicating limited substitution from unrestricted origins. Figure 5 plots this relationship.

Table 7: First Stage: Did Restriction Reduce Immigrant Presence?

Dependent Variables:	delta_restricted_share	delta_fb_share
	Δ Restricted Share	Δ Total FB Share
Model:	(1)	(2)
Variables		
Quota Exposure (1920)	-0.2658***	-0.2043***
	(0.0032)	(0.0077)
Fixed-effects		
statefip_1920	Yes	Yes
Fit statistics		
Observations	3,036	3,036
R^2	0.79556	0.52724
Within R^2	0.69811	0.18932

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

County-level regressions weighted by number of linked individuals.
Dependent variable is the change in county share of foreign-born from
restricted-origin countries (Col 1) or all foreign-born (Col 2)
between the 1920 and 1930 censuses. Includes state fixed effects.

8.2 Leave-One-Origin-Out

A key concern is that results could be driven by a single nationality group. Table 8, Panel A reports the main coefficient when each origin country is dropped in turn from the exposure measure. Estimates range from -1.93 (dropping Poland) to $+0.23$ (dropping Russia). The coefficient excluding Poland is marginally significant ($t = -2.05$), reflecting the importance of Polish immigrants in defining exposure variation, but the qualitative conclusion is unchanged: no single origin group reverses the overall null. Figure 6 displays these coefficients graphically.

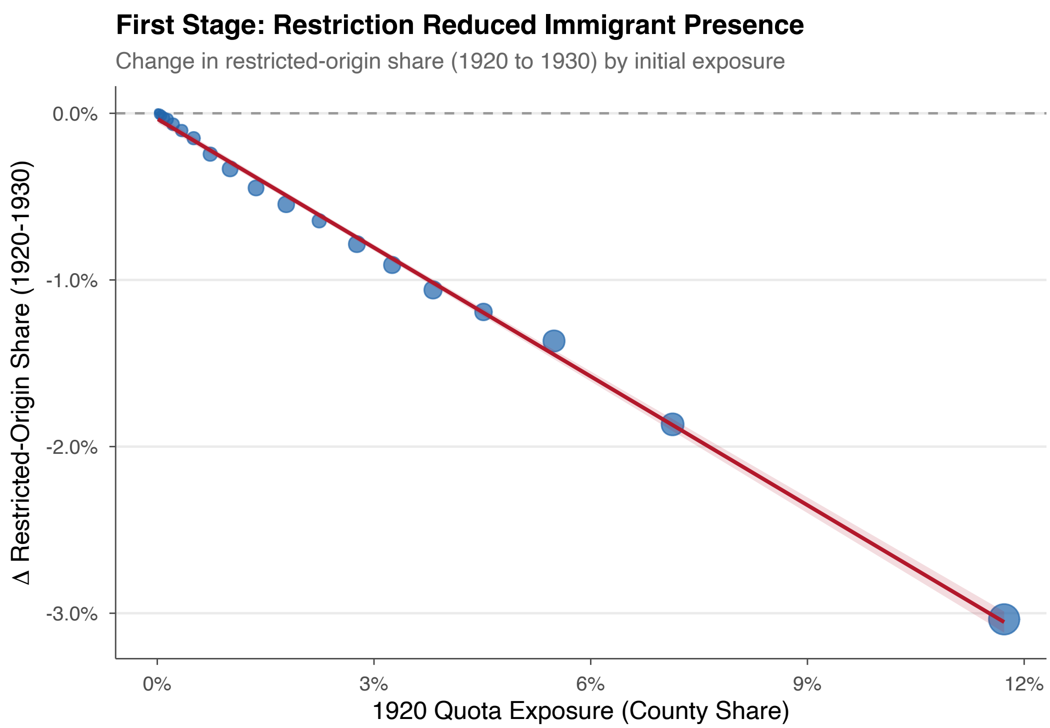

Figure 5: First Stage: Change in Restricted-Origin Share vs. Quota Exposure
Notes: County-level scatterplot of the change in restricted-origin foreign-born share (1920–1930) against 1920 quota exposure. Each point represents a county, weighted by the number of linked individuals. The tight negative relationship ($R^2 = 0.80$) confirms that the 1924 quotas effectively reduced the presence of restricted-origin immigrants in high-exposure counties.

Table 8: Robustness Checks

Dropped Origin	Coefficient	Std. Error	t-stat	N
Panel A: Leave-One-Origin-Out				
Italy	-1.8391	1.2672	-1.45	10,096,488
Russia	0.2281	0.8656	0.26	10,096,488
Poland	-1.9294	0.9406	-2.05	10,096,488
Austria	-1.4207	1.0889	-1.30	10,096,488
Hungary	-1.7793	0.9857	-1.81	10,096,488
Czech	-1.3966	1.0959	-1.27	10,096,488
Specification	Coefficient	Std. Error		N
Panel B: Alternative Specifications				
Baseline (two-way cluster)	-1.3519	0.9471		10,096,488
State cluster only	-1.3519	0.9605		10,096,488
Non-movers only	-1.7938	1.1314		8,944,291

Notes: Panel A drops each origin country in turn from the quota exposure measure (recalculating county-level exposure excluding that group) and re-estimates the main specification on the same sample. The individual sample is unchanged; only the treatment variable varies. Panel B varies the clustering level and sample. All specifications include state and 1920 occupation fixed effects and individual controls. Standard errors two-way clustered by state and county unless otherwise noted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

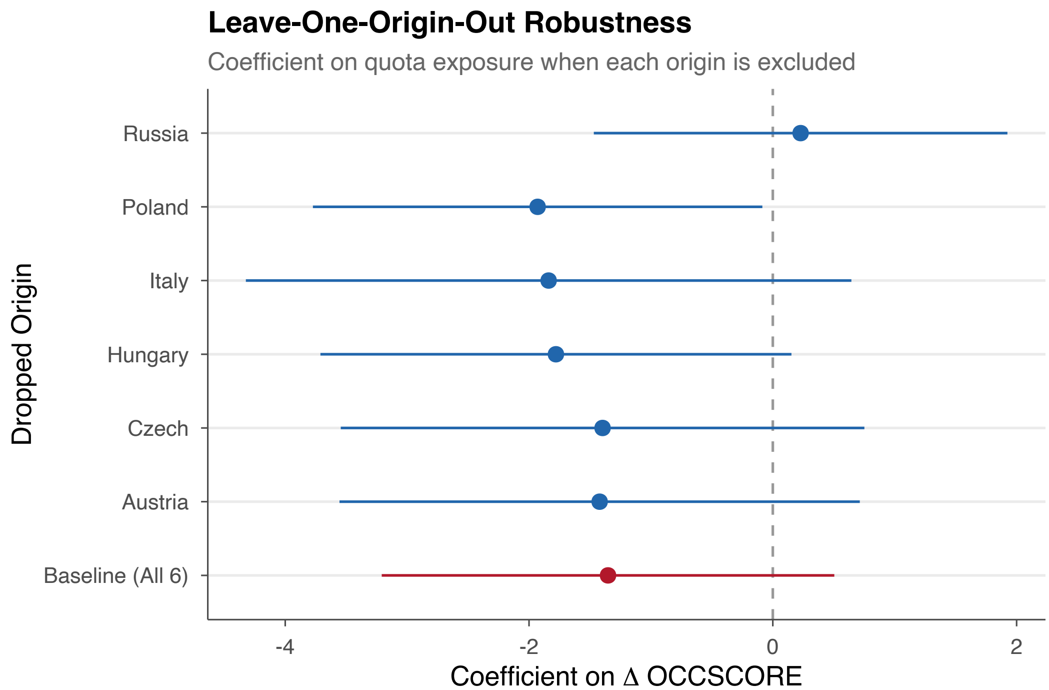

Figure 6: Leave-One-Origin-Out Robustness

Notes: Point estimates and 95% confidence intervals for the main specification when each restricted-origin country is dropped from the exposure measure. The baseline estimate (-1.35) is shown as a dashed line. Most estimates are statistically insignificant. The specification excluding Poland is marginally significant ($t = -2.05$), but no single origin group reverses the overall null.

8.3 Alternative Clustering and Sample Restrictions

Panel B of Table 8 reports additional robustness checks. Clustering standard errors at the state level only (48 clusters) yields nearly identical inference (SE = 0.961 vs. 0.947 with two-way state-and-county clustering). Restricting the sample to non-movers—individuals who remained in the same county between 1920 and 1930—produces a larger point estimate ($\hat{\beta} = -1.79$, SE = 1.13) that remains statistically insignificant. The non-mover result is slightly more negative, consistent with the selective-mobility finding that high-skill workers in high-exposure counties were more likely to stay.

8.4 Alternative Exposure Measure

As an additional robustness check, Appendix Table 11 replaces the 1920-based quota exposure measure with 1910 restricted-origin immigrant shares—a more historically predetermined instrument. The OCCSCORE result is unchanged ($\hat{\beta} = -0.43$, SE = 0.49, $p = 0.39$). The homeownership result using 1910 exposure is also negative and significant ($\hat{\beta} = -0.15$, SE = 0.056, $p < 0.01$), confirming that the homeownership finding is not an artifact of the particular exposure measurement year.

8.5 Occupational Transition Patterns

Figure 7 presents the overall occupational transition matrix, showing the probability of moving between broad occupational categories between 1920 and 1930. The matrix reveals substantial occupational churning—workers moved both up and down the occupational ladder. Diagonal dominance confirms strong occupational persistence, while the off-diagonal mass quantifies the scope of mobility against which the treatment effects are estimated.

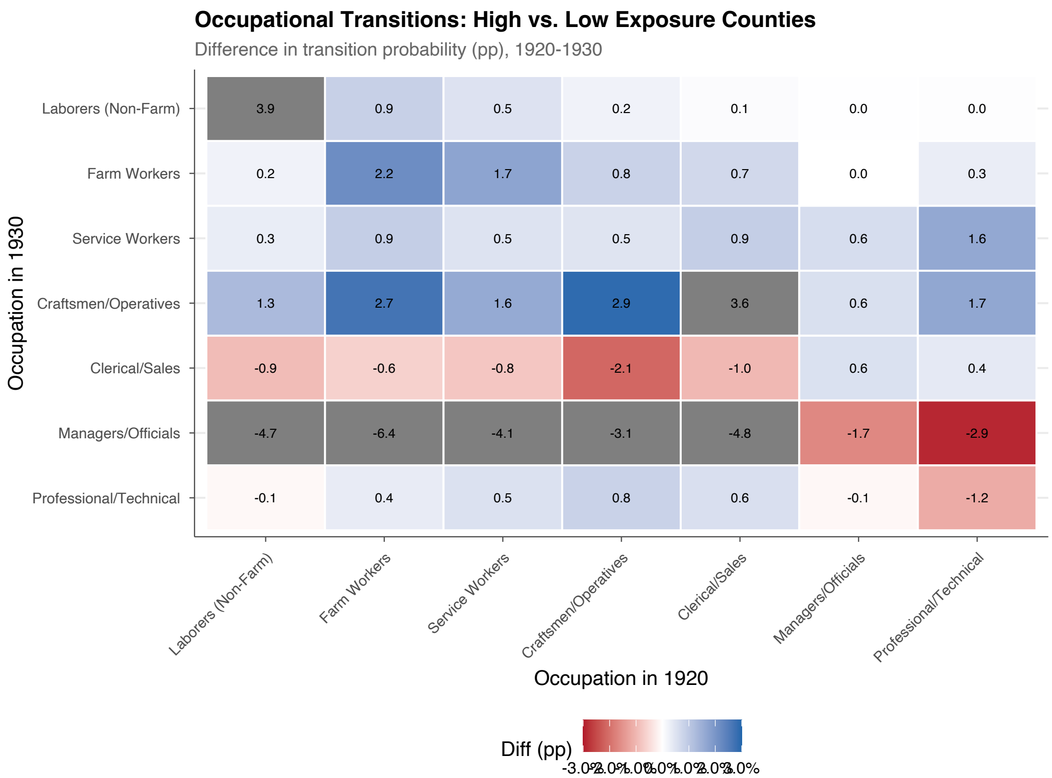

Figure 7: Occupational Transition Matrix, 1920–1930

Notes: Heatmap showing the probability of transitioning between broad occupational categories from 1920 to 1930. Rows indicate 1920 occupation; columns indicate 1930 occupation. Darker cells represent higher transition probabilities. The diagonal dominance reflects occupational persistence, while off-diagonal mass captures mobility.

8.6 Exposure Distribution

Figure 8 displays the distribution of the quota exposure measure across the 3,039 counties in our sample. The distribution is right-skewed, with most counties having low exposure and a substantial tail of industrial counties with exposure exceeding 10 percent.

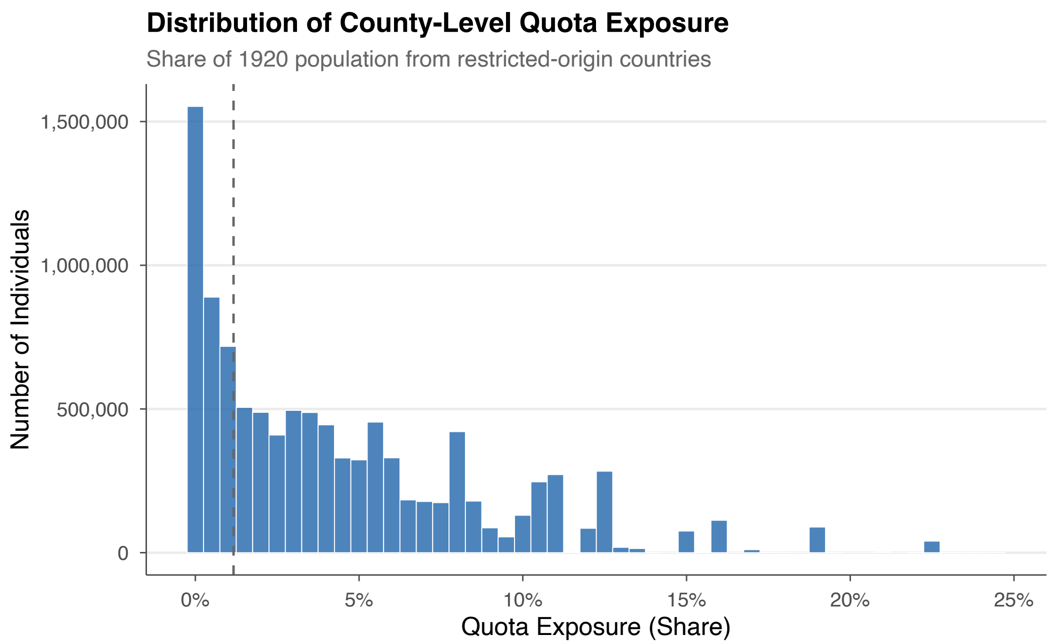

Figure 8: Distribution of Quota Exposure Across Counties
Notes: Histogram and kernel density of the county-level quota exposure measure (share of 1920 population born in restricted-origin countries). Mean exposure is 4.4 percent. The six restricted origins are Italy, Russia, Poland, Austria, Hungary, and Czechoslovakia.

9 Discussion and Implications

9.1 Why Did Restriction Fail to Deliver?

The null occupational effect is consistent with at least three mechanisms. First, complementarity: immigrants and natives occupied different positions in the task distribution, and removing immigrants eliminated the demand for complementary native skills rather than freeing up native opportunities (Ottaviano and Peri, 2012; Lewis, 2011). The strong positive pre-period relationship in Table 6 is consistent with this channel.

Second, general equilibrium adjustment: the reduction in immigrant labor may have reduced local economic activity and firm creation, offsetting any direct competitive relief. Moretti (2011) emphasizes the multiplier effects of labor demand shocks in local economies; if immigrant presence generated these multipliers, restriction could have simultaneously removed competitive pressure *and* reduced opportunity.

Third, imperfect substitution within native workers: the occupations vacated by immigrants may not have been attractive to natives. If immigrants were concentrated in arduous manual labor that natives were unwilling to perform—consistent with Foged and Peri (2016) finding that immigrants and natives sort into different tasks—then removing immigrants would not generate native occupational upgrading but could reduce the local economic base.

9.2 The Homeownership Channel

The homeownership finding is both novel and economically meaningful. Using the full-sample coefficient from Table 3, a one-standard-deviation increase in quota exposure (4.3 percentage points) is associated with a 0.96 percentage-point reduction in the probability of becoming a homeowner ($-0.223 \times 0.043 = -0.010$), against a base rate of approximately 33 percent. For young workers and urban residents—the groups most affected—the effect is approximately 1.2 percentage points (-0.271×0.043), a meaningful reduction in homeownership entry at the margin.

Why would restriction reduce homeownership? Immigrants in this era provided a disproportionate share of construction labor and service-sector employment that maintained urban housing markets. Ager and Brückner (2013) document the positive effects of immigrant diversity on local economic growth in this period. When quotas removed these workers, local housing construction likely slowed and service-sector employment contracted, making homeownership less attainable for marginal native buyers. The concentration of the effect among young and urban workers—those most dependent on local housing supply and economic conditions—supports this interpretation.

9.3 Implications for the Immigration Debate

The restrictionist mirage carries implications beyond the 1920s. The same economic logic—that reducing immigration will benefit native workers by removing competition—remains central to contemporary debates (Borjas, 2003; Dustmann et al., 2016). Our findings suggest that this logic failed dramatically in the case of the most aggressive immigration restriction in American history. Not only did restriction fail to deliver the promised occupational gains, it actively harmed native asset accumulation, plausibly by disrupting complementary economic relationships.

This does not mean that all immigration restrictions are counterproductive, or that the 1920s findings translate mechanically to the 21st century. Labor markets, immigration patterns, and institutional contexts differ profoundly. But the historical evidence offers a cautionary tale: the benefits of restriction are more easily promised than delivered, and the costs may emerge in unexpected channels. Eriksson and Ward (2020) and Abramitzky and Boustan (2022) provide additional evidence that the era of mass migration facilitated assimilation and economic dynamism in ways that restriction disrupted.

10 Conclusion

The Johnson-Reed Act of 1924 was the most consequential immigration restriction in American history, sold to the public as an economic policy that would protect native workers. The promise was specific: fewer immigrants would mean better jobs for Americans. A century later, linked census data covering 10.1 million workers and 3,039 counties allows us to adjudicate this claim.

The promise was hollow. Native-born men in counties most affected by the quotas experienced no greater occupational upgrading than those in counties barely touched. The null holds across continuous income scores, sector-specific transitions, and multiple identification strategies—though an auxiliary ladder-up indicator reveals a small negative effect, suggesting restriction marginally impeded categorical upgrading. Meanwhile, the restriction imposed a cost that its architects never anticipated: reduced homeownership entry among the young workers and urban residents it was supposed to help, plausibly driven by the destruction of immigrant–native complementarities that had fueled local economic dynamism.

The restrictionist mirage—the distance between what restriction promised and what it delivered—is a historical fact with contemporary resonance. It invites skepticism toward any policy that assumes native and immigrant workers are simple substitutes, and it underscores the importance of empirically testing, rather than rhetorically asserting, the economic consequences of closing the golden door.

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A Standardized Effect Sizes

Table 9: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Δ OCCSCORE	-1.3519	0.9471	9.216	-0.0064	0.0045	Small negative
Upgraded Occupation	-0.0667	0.0450	0.472	-0.0061	0.0041	Small negative
Became Homeowner	-0.2226	0.0689	0.392	-0.0247	0.0076	Small negative
Farm Exit	0.0088	0.1276	0.452	0.0008	0.0122	Null
Geographic Mobility	-0.0190	0.1013	0.318	-0.0026	0.0138	Null
Δ SEI	-0.1741	1.2906	18.600	-0.0004	0.0030	Null

Note:
Standardized effect sizes computed as SDE = *hatbetatimes* SD(X) / SD(Y) for continuous treatment. Treatment is the county-level share of restricted-origin foreign-born
in 1920 (SD =
0.043
). Classification refers to the magnitude of the
standardized effect. Data: IPUMS MLP v2 linked 1920–1930
panel, N = 10,096,489 native-born men.
Method: individual-level continuous-treatment DiD with county-level quota exposure.

B Auxiliary Regression Results

C Alternative Exposure Measure

Acknowledgements

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First Contributor: https://github.com/SocialCatalystLab

Project Repository: https://github.com/SocialCatalystLab/ape-papers

Table 10: Auxiliary Regression Results: Occupational Ladder, Self-Employment, and Selective Mobility

	Ladder Up (1)	Self-Employment (2)	Moved (3)
Quota Exposure	-0.0948***	-0.0108	-0.0722
	(0.0274)	(0.0330)	(0.1049)
Quota Exp. × High Skill			0.1309***
			(0.0318)
N	8,260,770	10,096,488	10,096,489
State + Occ FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
Skill Tercile Interactions	No	No	Yes

Notes: Col (1): binary indicator for upward movement along a coarse occupational ladder (unskilled to professional). Restricted to workers with non-missing categorical ladder codes in both census years, resulting in a smaller sample (N = 8,260,770) than the full panel. Col (2): transition to self-employment. Col (3): moved to different county; model includes baseline skill tercile (high/low based on 1920 OCCSCORE) and its interaction with quota exposure. The interaction coefficient captures differential mobility responses by skill level. Standard errors two-way clustered by state and county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Robustness: 1910 Exposure Instrument for 1920–1930 Outcomes

	Δ OCCSCORE (1)	Became Owner (2)
Quota Exposure (1910)	-0.4261	-0.1517***
	(0.4924)	(0.0562)
N	9,972,804	9,972,804
State + Occ FE	Yes	Yes
Individual Controls	Yes	Yes

Notes: Same specification as Tables 2 and 7 but using 1910 restricted-origin immigrant shares as the exposure measure instead of 1920 shares. The 1910 measure is more historically predetermined, reducing concerns about endogenous settlement between 1910 and 1920. Smaller N reflects counties matched across all three census years. Standard errors two-way clustered by state and county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.