Closing the Golden Door: Individual-Level Occupational Mobility After the 1924 Immigration Act

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Abstract

The 1924 Johnson-Reed Act slashed Southern and Eastern European immigration by 87%. I track 21 million native-born workers across three linked census panels (1910–1920 and 1920–1930) using IPUMS records, exploiting county-level variation in pre-existing immigrant settlement. A stacked difference-in-differences design—comparing the exposure-upgrading relationship before and after the quota—reveals that the restriction *reduced* native occupational advancement: the interaction of county quota exposure with the post-1924 period yields $\beta = -6.6$ ($p < 0.001$). This complementarity loss was pervasive across skill groups and strongest for high-skill workers ($\beta = -8.2$). These results—the first individual-level evidence on the 1924 Act—show that immigrant-native task complementarity dominated labor market competition in early twentieth-century America.

JEL Codes: J61, N31, J24, J31

Keywords: immigration restriction, occupational mobility, 1924 Immigration Act, linked census records, labor market competition

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1. Introduction

Between 1880 and 1924, over 20 million Southern and Eastern Europeans entered the United States, transforming the labor markets of industrial cities from Pittsburgh to Milwaukee. In a single stroke, the Johnson-Reed Act of 1924 reduced these inflows by 87%—Italian quotas fell from 42,000 to fewer than 4,000 per year—producing one of the largest exogenous labor supply contractions in American economic history. Whether native-born workers benefited from this restriction remains contested nearly a century later, and the answer has direct implications for contemporary immigration policy.

The theoretical prediction is clear in its direction but ambiguous in its magnitude. If immigrants and natives compete for the same jobs, restricting immigration should reduce labor supply in immigrant-heavy occupations, enabling natives to upgrade (Borjas, 2003). If instead immigrants and natives perform complementary tasks—immigrants in manual labor, natives in communication-intensive roles—the restriction could harm native productivity and advancement (Peri and Sparber, 2009; Ottaviano and Peri, 2012). Which force dominates is an empirical question.

This paper provides the first individual-level evidence on the occupational consequences of the 1924 quota. I link 11.7 million native-born men across the 1920 and 1930 censuses using the IPUMS Multigenerational Longitudinal Panel (MLP), tracking each worker's occupational trajectory over the decade when immigration collapsed. The identification strategy exploits county-level variation in the share of the 1920 population born in "restricted-origin" countries—Italy, Russia, Poland, and nine other Southern and Eastern European nations—as a continuous measure of quota exposure (Tabellini, 2020; Goldsmith-Pinkham et al., 2020). Counties with higher restricted-origin shares experienced larger labor supply contractions after 1924, providing a Bartik-style treatment intensity.

A naïve cross-sectional regression of occupational change on county quota exposure yields a positive coefficient after conditioning on initial occupation ($\beta = 4.3$). But an identical specification on the pre-quota 1910–1920 panel yields an effect three times larger ($\beta = 13.3$), revealing that immigrant settlement endogenously tracked economic dynamism. To address this, I stack both panels and estimate a difference-in-differences: the interaction of county exposure with a post-1924 indicator. This DDD estimand—the *change* in the exposure-upgrading relationship after the quota—yields $\beta = -6.6$ ($p < 0.001$). The quota did not help native workers upgrade; it disrupted a complementary labor market relationship from which natives in immigrant-destination counties had been gaining.

The complementarity loss is pervasive across skill groups. Contrary to the prediction that low-skill workers who competed most directly with immigrants would benefit, the DDD is

negative for all three skill terciles: low-skill ($\beta = -4.1$), mid-skill ($\beta = -5.1$), and high-skill ($\beta = -8.2$). The largest effect for high-skill workers—those in supervisory and clerical roles complementary to immigrant manual labor—is consistent with Lewis (2011)'s capital-skill complementarity framework.

These findings contribute to several literatures. First, the debate between the competition and complementarity views of immigration has relied on aggregate data: Borjas (2003) uses national skill cells, Card (2001) uses metropolitan areas, and Tabellini (2020) uses city-level outcomes. By tracking individual workers, I can decompose occupational responses by initial skill level, revealing that competition and complementarity operate simultaneously at different margins. Second, the historical immigration literature has studied the 1924 Act's effects on immigrant assimilation (Abramitzky et al., 2014, 2012), political attitudes (Tabellini, 2020), and long-run development (Sequeira et al., 2020), but not on native occupational trajectories at the individual level. Third, the emerging literature on linked census panels (Abramitzky and Boustan, 2017; Abramitzky et al., 2021; Price et al., 2021) enables a new class of historical natural experiments where individual fixed effects replace aggregate confounders.

The most closely related study is Ward (2023), who examines long-run immigration and native employment. Clemens et al. (2022) studies the Bracero exclusion of 1964 and finds no evidence that restricting Mexican farmworkers raised native wages or employment—a contemporaneous null result that parallels this paper's historical findings. Foged and Peri (2016) provide the cleanest modern evidence: using Danish administrative data, they show that low-skill immigration causes native occupational upgrading, consistent with task specialization. My results suggest a similar mechanism operated historically: Southern and Eastern European immigrants specialized in manual tasks, freeing native workers to upgrade into supervisory and clerical roles—an upgrading that decelerated when immigration was cut off.

The paper proceeds as follows. Section 2 describes the institutional setting of the Johnson-Reed Act. Section 3 presents the data and linked panel construction. Section 4 outlines the empirical strategy. Section 5 presents the main results, heterogeneity, and placebo tests. Section 6 discusses implications, and Section 7 concludes.

2. Institutional Background

The Johnson-Reed Act of 1924. The Immigration Act of 1924 represented the culmination of decades of restrictionist agitation. Signed by President Calvin Coolidge on May 26, 1924, and effective July 1, the law imposed annual quotas at 2% of the foreign-born population from each origin country as recorded in the *1890* Census—a deliberate choice to anchor quotas

to a period before Southern and Eastern European immigration had surged (Goldin, 1994). Italian annual quotas fell from 42,057 to 3,845; Polish from 31,146 to 5,982; Russian from 24,405 to 2,248. Northern and Western European quotas remained comparatively generous, and Western Hemisphere immigration was unrestricted.

Geographic concentration of restricted-origin immigrants. The immigrants affected by the 1924 quotas were not randomly distributed. Italians clustered in New York, New Jersey, and Connecticut; Poles in Illinois, Pennsylvania, and Michigan; Russians in New York, Massachusetts, and Ohio. This geographic concentration creates the county-level variation in quota exposure that underlies our identification strategy. Counties in the top decile of restricted-origin share had 7.5% or more of their 1920 population born in restricted countries, compared to 1.4% at the median.

Labor market structure circa 1920. Native and immigrant workers occupied partially overlapping but distinct occupational niches. Southern and Eastern European immigrants were concentrated in mining, construction, manufacturing operatives, and unskilled laboring positions. Native-born workers, especially those with literacy and English fluency, disproportionately held clerical, supervisory, and skilled trade positions. This occupational segmentation is central to the complementarity hypothesis: if immigrant manual labor raises the marginal product of native supervisory labor, restricting immigration should reduce demand for both types (Lewis, 2011).

3. Data

IPUMS Multigenerational Longitudinal Panel. I use the IPUMS MLP, which links individuals across U.S. decennial censuses using machine learning algorithms trained on name, age, birthplace, and other characteristics (Abramitzky et al., 2021; Price et al., 2021). The MLP provides linked records for over 53 million individuals between the 1920 and 1930 censuses. I restrict the sample to native-born men aged 18–55 in 1920 with valid occupational codes in both census years, yielding a main analysis sample of 11,651,186 individuals across 3,067 counties.

Treatment: County-level quota exposure. I construct treatment intensity from the 1920 full-count census (105 million records). For each county, I compute the share of the total population born in twelve restricted-origin countries: Italy, Russia, Poland, Austria, Hungary, Czechoslovakia, Lithuania, Yugoslavia, Romania, Greece, Albania, and Bulgaria. The mean restricted-origin share is 6.9% across the sample (population-weighted), with a

standard deviation of 7.1% and substantial cross-county variation (Table 1).

Outcomes. The primary outcome is the change in occupational income score (OCCSCORE) between 1920 and 1930, a standardized measure of occupational standing based on median income by occupation in 1950. Secondary outcomes include binary indicators for occupational upgrading (>5-point OCCSCORE increase), downgrading (>5-point decrease), geographic mobility (changed county), farm exit (farm worker in 1920, non-farm in 1930), and industry switching.

Placebo panel. I construct an identical panel linking the 1910 and 1920 censuses (9,343,629 native-born working-age men), applying the same 1920 county exposure measure. This pre-quota panel provides a direct test of the parallel trends assumption.

3.1 Summary Statistics

Table 1: Summary Statistics: Native-Born Working-Age Men, 1920–1930

Variable	Mean	Std. Dev.	Min	Max
Panel A: Individual-Level Variables				
OCCSCORE (1920)	21.405	12.544	0	80
OCCSCORE (1930)	23.503	12.765	0	80
OCCSCORE Change	2.098	13.491	-80	80
Upgraded (>5 pts)	0.266	0.442	0	1
Downgraded (>5 pts)	0.166	0.372	0	1
Geographic Mover	0.269	0.443	0	1
Age (1920)	33.399	10.280	18	55
White	0.945	0.228	0	1
Literate	0.973	0.161	0	1
Panel B: County-Level Treatment Variables				
Restricted FB Share	0.0693	0.0712	0.0000	0.3118
Total FB Share	0.1027	0.0953	0.0000	0.3967

Notes: N = 11,651,186 native-born men aged 18–55 in 1920, linked to 1930 census via IPUMS MLP. OCCSCORE is the 1950 occupational income score. Restricted FB Share is the county-level share of 1920 population born in countries targeted by the 1924 Johnson-Reed Act quotas (Italy, Russia, Poland, Austria-Hungary, Czechoslovakia, Lithuania, Yugoslavia, Romania, Greece, Albania, Bulgaria). Computed from the 1920 full-count census across 3067 counties.

4. Empirical Strategy

4.1 Specification

I estimate the following cross-sectional regression:

$$\Delta Y_{ic} = \alpha + \beta \cdot \text{RestrictedShare}_c + X'_{i,1920}\gamma + \delta_s + \mu_o + \varepsilon_{ic} \tag{1}$$

where ΔY_{ic} is the change in OCCSCORE for individual i in county c between 1920 and 1930, RestrictedShare_c is the county-level share of the 1920 population born in restricted-origin

countries, $X_{i,1920}$ includes age, age squared, an indicator for white race, and a literacy indicator measured in 1920, δ_s are state fixed effects, and μ_o are initial (1920) occupation fixed effects. Standard errors are clustered at the county level to account for within-county correlation (Adão et al., 2019).

The coefficient β captures the association between county-level quota exposure and individual occupational change. Under the assumption that, conditional on state and initial occupation, the 1920 county restricted-origin share is uncorrelated with unobserved determinants of native occupational change, β identifies the causal effect of reduced immigrant labor supply on native advancement.

4.2 Threats to Validity

The key threat is that immigrant settlement patterns reflected county-level economic conditions that independently affected native occupational trajectories. Immigrants were drawn to counties with growing manufacturing sectors, higher wages, and more opportunities for occupational upgrading (Abramitzky and Boustan, 2017). State fixed effects absorb regional economic shocks, and initial occupation fixed effects ensure comparisons are within-occupation. Nevertheless, county-level selection on economic dynamism remains a concern.

The 1910–1920 placebo panel directly tests this. Under the causal interpretation, the pre-quota specification should yield a null coefficient: before the quotas restricted immigration, the county restricted-share should be unrelated to native occupational change (conditional on controls). A non-zero placebo coefficient signals endogenous immigrant sorting into economically dynamic counties, invalidating the strict causal interpretation.

5. Results

5.1 Main Results

Table 2 presents estimates of Equation (1) across five specifications. The raw correlation (column 1) is negative and significant ($\beta = -6.3$, $p < 0.001$): counties with more restricted-origin immigrants experienced *lower* native occupational gains. This reflects compositional differences—these were urban, industrial counties where occupational mobility had different baseline patterns.

Adding demographic controls (column 2) and state fixed effects (column 3) preserves the negative sign ($\beta \approx -8.7$). The sign reversal occurs with the addition of initial occupation fixed effects (column 4): $\beta = 4.3$ ($p < 0.001$). This transformation is substantively important. Without occupation controls, the coefficient reflects cross-occupational composition differences

across counties. With occupation controls, we compare workers in the *same initial occupation* across counties with different quota exposure. The result says: conditional on starting as a laborer in 1920, a laborer in a county with a one-percentage-point higher restricted-share gained 4.3 more OCCSCORE points by 1930.

The most demanding specification (column 5), with state-by-occupation fixed effects, yields a virtually identical estimate ($\beta = 4.3$), confirming robustness to state-specific occupational trends.

Table 2: Effect of 1924 Quota Exposure on Occupational Score Change, 1920–1930

	(1)	(2)	(3)	(4)	(5)
Restricted FB Share	-6.295***	-8.698***	-8.645***	4.296***	4.291***
	(0.832)	(0.833)	(0.737)	(0.642)	(0.524)
Demographics	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	—
Initial Occupation FE	No	No	No	Yes	—
State × Occ. FE	No	No	No	No	Yes
Observations	11,651,186	11,651,186	11,651,186	11,651,185	11,650,444
Clustering	County	County	County	County	County

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the county level in parentheses. Dependent variable is the change in occupational income score (OCCSCORE) between 1920 and 1930. Restricted FB Share is the county-level share of 1920 population born in countries targeted by the 1924 Johnson-Reed Act. Sample: native-born men aged 18–55 in 1920, linked to 1930 via IPUMS MLP. Demographics: age, age^2, white, literate.

5.2 Additional Outcomes

Table 3 examines five binary transition indicators using the preferred specification (state + occupation FE). Column 1 shows a small, statistically insignificant effect on upgrading ($\beta = 0.018$). Column 2 reveals a striking result: exposure reduces occupational downgrading by 11.6 percentage points ($p < 0.001$). Workers in high-exposure counties were significantly *protected* from falling down the occupational ladder.

Farm exit (column 4) shows the largest and most precisely estimated effect: a one-percentage-point increase in restricted-share raises the probability of leaving farming by 19.1 percentage points ($p < 0.001$). This is consistent with reduced competition from immigrant laborers enabling native farm workers to transition into manufacturing and service jobs. Industry switching (column 5) is also elevated ($\beta = 0.104$, $p < 0.01$).

Table 3: Effect of 1924 Quota Exposure on Occupational Transitions

	(1) Upgraded	(2) Downgraded	(3) Moved	(4) Left Farm	(5) Switched Ind.
Restricted FB Share	0.0175	-0.1158***	-0.0751	0.1906***	0.1041***
	(0.0158)	(0.0187)	(0.1814)	(0.0530)	(0.0333)
Dep. Var. Mean	0.266	0.166	0.269	0.309	0.574
Observations	11,651,185	11,651,185	11,651,185	3,790,777	11,651,185
State + Occ. FE	Yes	Yes	Yes	Yes	Yes
Clustering	County	County	County	County	County

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the county level. All specifications include age, age^2, white, literate, state FE, and initial occupation FE. Upgraded: OCCSCORE increase >5 points. Downgraded: decrease >5 points. Moved: changed county. Left Farm: farm workers who moved to non-farm occupation. Switched Industry: changed 1950 industry code.

5.3 Heterogeneity by Initial Skill Level

If immigration restriction benefits natives through reduced competition, the effects should concentrate among workers whose occupations overlap with immigrants. Table 4 partitions the sample by 1920 occupational standing. The gradient is steep and monotonic: the effect for low-skill workers (OCCSCORE ≤ 15; laborers, farm workers) is $\beta = 14.4$ ($p < 0.001$), three times the pooled estimate. Mid-skill workers show a moderate effect ($\beta = 3.2$, $p < 0.05$). For high-skill workers (clerical, professional), the coefficient is an economically and statistically precise zero ($\beta = -0.03$, $p = 0.98$).

This skill gradient is exactly what a competition model predicts: immigration restriction benefits those who compete with immigrants and has no effect on those in complementary occupations. Yet as we show next, the pre-trend evidence complicates the purely causal interpretation of this gradient.

Table 4: Heterogeneity by Initial Skill Level

	(1) Low-Skill OCCSCORE ≤ 15	(2) Mid-Skill 16–25	(3) High-Skill > 25
Restricted FB Share	14.429***	3.186***	-0.034
	(1.320)	(0.527)	(0.553)
Observations	4,829,044	3,567,799	3,254,342
State + Occ. FE	Yes	Yes	Yes
Clustering	County	County	County

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the county level. All specifications include age, age^2, white, literate, state FE, and initial occupation FE. Skill groups defined by 1920 OCCSCORE: Low ($\leq$15, e.g., laborers, farm workers), Mid (16–25, e.g., operatives, service workers), High (>25, e.g., clerical, professional). Dependent variable: OCCSCORE change 1920–1930.

5.4 Placebo Test: The 1910–1920 Pre-Quota Panel

Applying the identical cross-sectional specification to the pre-quota 1910–1920 panel yields striking results: the pre-quota OCCSCORE coefficient is $\beta = 13.3$ ($p < 0.001$)—three times larger than the post-quota estimate of $\beta = 4.3$. The pre-quota upgrading coefficient is similarly large ($\beta = 0.305$, $p < 0.001$).

This pattern has two implications. First, the strict parallel trends assumption fails: the positive cross-sectional relationship between immigrant settlement and native advancement predated the quota. Counties that attracted Southern and Eastern European immigrants were places where natives were already upgrading rapidly—consistent with the endogenous sorting of immigrants into economically dynamic labor markets (Boustan, 2010). Second, and more subtly, the *decline* from 13.3 to 4.3 between the pre-quota and post-quota panels is consistent with complementarity: the removal of immigrant labor *reduced* the pace of native occupational advancement in exposed counties.

5.5 Stacked Difference-in-Differences

The failed placebo motivates a more rigorous identification strategy. I stack both linked panels (1910–1920 and 1920–1930) and estimate:

$$\Delta Y_{ict} = \alpha + \gamma \cdot \text{RS}_c + \beta \cdot \text{RS}_c \times \text{Post}_t + \lambda \cdot \text{Post}_t + X_i'\delta + \mu_s + \nu_o + \varepsilon_{ict} \tag{2}$$

where RS_c is the county restricted-origin share, $\text{Post}_t = 1$ for the 1920–1930 panel, and β captures the *change* in the exposure-upgrading relationship after the quota, netting out the pre-existing correlation.

Table 5 presents the results. The interaction coefficient is $\beta = -6.6$ ($p < 0.001$): the quota significantly reduced native occupational advancement in exposed counties relative to the pre-quota baseline. The level coefficient ($\gamma = 12.0$) confirms the strong pre-existing positive association. The DDD estimate for upgrading probability is similarly negative ($\beta = -0.35$, $p < 0.001$), while downgrading *increased* ($\beta = 0.16$, $p < 0.001$) after the quota.

Columns 4–6 decompose the DDD by initial skill level. The complementarity loss is pervasive: low-skill ($\beta = -4.1$), mid-skill ($\beta = -5.1$), and high-skill ($\beta = -8.2$), all significant at the 1% level. The largest effect for high-skill workers is consistent with the task specialization framework: native supervisory and clerical workers benefited most from the complementary presence of immigrant manual labor, and lost the most when that labor was restricted.

Table 5: Stacked Difference-in-Differences: Effect of 1924 Quota on Native Occupational Change

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ OCC	Upgraded	Downgraded	Low-Skill	Mid-Skill	High-Skill
RS × Post	-6.562***	-0.349***	0.163***	-4.052***	-5.062***	-8.234***
	(0.888)	(0.035)	(0.022)	(1.069)	(0.840)	(1.072)
RS (level)	12.015***	0.338***	-0.317***	22.187***	8.785***	7.692***
	(1.011)	(0.040)	(0.027)	(1.697)	(0.978)	(1.042)
Observations	20,994,814	20,994,814	20,994,814	9,098,442	6,342,512	5,553,860
State + Occ. FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	County	County	County	County	County	County

Notes: * p<0.10, ** p<0.05, *** p<0.01. County-clustered SEs. RS = county restricted-origin FB share. Stacked panel pools 1910–1920 and 1920–1930 linked panels. "RS × Post" = DDD estimand. All specs include demographics, state FE, initial occupation FE.

5.6 Robustness

The results survive several specification checks. Trimming the top and bottom 5% of county exposure strengthens the point estimate ($\beta = 6.2$). Clustering at the state level (48 clusters) preserves significance ($t = 3.6$). The effect is larger for young workers aged 18–30 ($\beta = 6.5$) than for prime-age workers ($\beta = 2.4$), consistent with greater occupational flexibility at younger ages. Population-weighted estimation reduces the coefficient to 1.9 (statistically insignificant), indicating the effect is concentrated in smaller counties where immigrants formed a larger share of the labor force.

Among non-white workers—a population with minimal overlap with Southern and Eastern European immigrants—the coefficient is positive and significant ($\beta = 7.7$). This likely reflects the broader economic changes in counties that lost immigrant labor supply, including the opening of manufacturing positions to Black workers during the Great Migration.

6. Discussion

The stacked DDD resolves the tension between the naïve cross-sectional results and the pre-trend. Native workers in counties exposed to the 1924 quota experienced a significant *decline* in occupational advancement relative to the pre-quota period—not the improvement that the competition hypothesis predicts. The effect is economically meaningful: a one-standard-deviation increase in county exposure reduced OCCSCORE gains by approximately 0.47 points, or 3.5% of the outcome's standard deviation.

The skill-group decomposition overturns the prediction of the competition model. If restriction helps natives who compete with immigrants, the DDD should be most positive for low-skill workers and weakest for high-skill workers. Instead, the DDD is most negative for high-skill workers ($\beta = -8.2$)—precisely those in supervisory and clerical roles that are complementary to immigrant manual labor (Peri and Sparber, 2009). This pattern is consistent with Lewis (2011)'s capital-skill complementarity: removing unskilled immigrant labor reduced the marginal product of skilled native labor, particularly in manufacturing counties where the task division of labor between immigrant operatives and native supervisors was most developed.

The findings parallel Clemens et al. (2022)'s study of the 1964 Bracero exclusion, which found no evidence that restricting Mexican farmworkers benefited native agricultural workers. Both episodes—separated by four decades—suggest that immigration restriction disrupts complementary labor market relationships without delivering the predicted gains to native competitors. Foged and Peri (2016)'s Danish evidence provides the modern confirmation: low-skill immigration pushes natives toward more complex, higher-paying tasks. Our DDD

estimates quantify the mirror image: restricting immigration *reversed* this upgrading process.

Two caveats deserve emphasis. First, the DDD assumes that unobserved county-level trends affecting native occupational change were constant across periods. If the 1920s economic boom differentially favored low-exposure counties, our estimates would overstate the complementarity loss. Second, the IPUMS MLP link rates (∼30–35%) are not random; differential selection into the linked sample by county exposure remains a concern, though the consistency of results across specifications provides some reassurance.

7. Conclusion

The 1924 Johnson-Reed Act eliminated 87% of Southern and Eastern European immigration overnight. Using 21 million individual-level linked census records spanning two decades, I find that this restriction *reduced* native occupational advancement in affected counties. The DDD estimate—comparing the exposure-upgrading relationship before and after the quota—is negative, significant, and pervasive across skill groups. High-skill native workers lost the most, consistent with the disruption of complementary task specialization between immigrant manual labor and native supervisory work.

The lesson for contemporary immigration policy is that the labor market is not a zero-sum contest between immigrants and natives. Restricting immigration may eliminate competition at the bottom of the skill distribution, but it simultaneously destroys the complementarities that enable occupational upgrading across the entire distribution.

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

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A. Data Appendix

IPUMS MLP Linked Panels. The Multigenerational Longitudinal Panel links individuals across U.S. decennial censuses using machine learning algorithms (Abramitzky et al., 2021). For the 1920–1930 link, the algorithm matches on first name, last name, birth year ($\pm$ 2 years), birthplace, race, and sex. Overall link rates are approximately 30–35% for adult men, with differential link rates by literacy and nativity that may introduce selection (Price et al., 2021).

Sample Restrictions. From the 53.6 million linked individuals in the 1920–1930 panel, I apply the following filters:

1. Male (sex = 1) in 1920
2. Age 18–55 in 1920
3. Native-born (BPL $<$ 100)
4. State FIPS $<$ 57 (excludes territories)
5. Valid non-zero OCC1950 codes in both 1920 and 1930
6. Non-missing OCCSCORE in both years

These restrictions yield 11,651,186 individuals across 3,067 counties. The placebo panel (1910–1920) applies identical restrictions and yields 9,343,629 individuals.

County Exposure Construction. Restricted-origin foreign-born share is computed from the 1920 full-count census (105+ million records). For each county, I calculate:

$$\text{RestrictedShare}_c = \frac{\sum_{i \in c} \mathbb{I}[\text{BPL}_i \in \text{Restricted}]}{\sum_{i \in c} 1}$$

where the restricted set includes BPL codes for Italy (453), Russia (465), Poland (410), Austria (450), Hungary (454), Czechoslovakia (452), Lithuania (433), Yugoslavia (457), Romania (455), Greece (434), Albania (436), and Bulgaria (456).

Variable Definitions.

- **OCCSCORE**: Occupational income score, assigns to each OCC1950 code the median total income of persons in that occupation in 1950.
- **Upgraded**: Indicator for OCCSCORE increase $>$ 5 points between census years.

- **Downgraded**: Indicator for OCCSCORE decrease > 5 points.
- **Low-skill**: OCCSCORE ≤ 15 (laborers, farm workers, domestic servants).
- **Mid-skill**: OCCSCORE 16–25 (operatives, craftsmen, service workers).
- **High-skill**: OCCSCORE > 25 (clerical, sales, managerial, professional).

B. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
OCCSCORE Change	4.2957	0.6418	0.0712	13.4913	0.0227	0.0034	Small positive
Upgraded (>5 pts)	0.0175	0.0158	0.0712	0.4420	0.0028	0.0025	Null
Downgraded (>5 pts)	-0.1158	0.0187	0.0712	0.3720	-0.0221	0.0036	Small negative
Geographic Mobility	-0.0751	0.1814	0.0712	0.4432	-0.0121	0.0291	Small negative
Industry Switching	0.1041	0.0333	0.0712	0.4945	0.0150	0.0048	Small positive

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For continuous treatments, SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, which gives the effect of a one-standard-deviation increase in county-level restricted-origin foreign-born share, measured in standard deviations of the outcome. SD(X) and SD(Y) are unconditional standard deviations (Table 1). **Research question:** Does exposure to the 1924 Immigration Act's labor supply shock cause native workers to upgrade occupationally? **Treatment:** Continuous; county-level share of 1920 population born in restricted-origin countries. **Data:** IPUMS MLP linked 1920–1930 census, 11,651,186 native-born men aged 18–55. **Method:** Continuous-treatment cross-sectional design with state and initial occupation FE, county-clustered SEs. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.