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# Immigration Restrictions and Natives' Intergenerational Mobility: Evidence from the 1920s US Quota Acts*

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**Abstract**

We study the effects of immigration restrictions on the intergenerational mobility of US-born men in the United States. We link US-born sons observed in 1900, 1920, and 1940 full-count Censuses to their fathers, and construct a measure of county-level exposure to the 1920s immigration acts, which sharply curtailed immigration from Southern and Eastern Europe. Exploiting this policy-induced variation, we find that the quotas reduced intergenerational mobility among US-born white men, but had no adverse effect for Black men. Among whites, losses were smaller for sons of richer fathers, who were more likely to migrate away from highly exposed areas. Evidence from the 1940 Census indicates that exposed white men were less likely to be employed and earned lower wages in adulthood, consistent with both occupational downgrading and reduced productivity within occupations. We show that these effects operated through both reduced immigrant–native complementarities and incomplete substitution from unrestricted migration, while human capital investment can explain at most only a modest part of the total effect.

**JEL Codes:** J15; J62; K37; N32.
**Keywords:** Immigration; immigration restrictions; intergenerational mobility.

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# 1 Introduction

Immigration policy has emerged as a focal point of political debate in many advanced economies. Proposals to curtail immigration are frequently justified on both cultural and economic grounds. Advocates of restrictions often express concerns about national identity, social cohesion, and cultural compatibility. But economic arguments are also prominent: immigrants are said to increase competition in local labor markets, depress wages for native workers, and strain public resources. A growing literature has sought to evaluate the consequences of immigration restrictions on economic growth, employment, and wages (Clemens et al., 2018; Clemens and Lewis, 2022; Long et al., 2022; Abramitzky et al., 2023). While these studies have deepened our understanding of the short- and medium-run labor market effects of immigration policy, we know less about its implications for the next generation of native-born individuals. This question is especially important because immigration restrictions are often persistent: once enacted, they tend to remain in place for decades.

In this paper, we provide new, rigorous empirical evidence on this issue, studying the impact of the 1921 and 1924 US immigration laws on the intergenerational mobility of US-born men. These laws ended the Age of Mass Migration (1850–1920), a period during which over 30 million Europeans migrated to the United States (Abramitzky and Boustan, 2017), introducing a restrictive quota system that remained in place for four decades.[1] The quotas sharply reduced immigration from Southern and Eastern Europe while preserving higher allowances for Northern and Western European countries. This policy shift generated a sharp and geographically uneven change in both the scale and composition of immigration across the country, providing a quasi-natural experiment for identifying the causal effects of immigration restrictions.

Following previous work (Abramitzky et al., 2023; Ager et al., 2023), we construct a county-level measure of exposure to the quotas, based on pre-existing immigrant settlement patterns and predicted inflows in the absence of the law. We combine this measure with individual-level data on intergenerational mobility, linking US-born sons observed in childhood to their adult outcomes using the full-count US censuses from 1900, 1920, and 1940 and the Census Tree Project (Price et al., 2023). These

[1] The United States had already banned most Chinese immigration with the Chinese Exclusion Act of 1882 and had effectively barred Japanese immigrants through the Gentlemen's Agreement of 1907 (Abramitzky and Boustan, 2017).

rich and detailed data allow us to track family background, geographic location, and occupational status over time.

We focus on absolute occupational mobility: the probability that sons attain higher-status occupations than their fathers, using harmonized occupation scores, which we adjust for cohort, race, region, and nativity (Song and Xie, 2020; Ward, 2023). While the literature also considers relative mobility (Abramitzky et al., 2025), measured as the strength of the association between children's and parents' ranks, absolute mobility is particularly well suited to our historical setting. This is because it captures economically meaningful improvements in living standards that are not mechanically tied to changes in the cross-sectional distribution of occupations. In periods of structural transformation—such as the early 20th century US (Eckert and Peters, 2025)—relative mobility can remain stable even when broad-based gains or losses affect entire cohorts. By contrast, absolute mobility directly reflects whether sons were able to move into higher-status occupations than their fathers. For completeness, we also present results for relative mobility.

We estimate individual-level regressions that include county fixed effects and Census region-by-period fixed effects, and interact the quota exposure measure with a post-1920 (post-quota) indicator. This specification absorbs all time-invariant county characteristics and allows for flexible mobility trends across regions over time. To account for differences in the intensity of the quota shock across locations, we also control for the interaction between the size of the foreign-born population in 1900 and period dummies. This ensures that we isolate variation in quota-exposure arising from the composition of the immigrant population—as determined by national origin quotas—while holding constant the initial scale of foreign-born population.

We find that the quotas slowed intergenerational mobility among US-born whites. In counties more exposed to the restrictions, sons were significantly less likely to surpass their fathers in occupational status. A 5 percentage point increase in quota exposure—equivalent to moving from a low- to a highly affected county—reduces the probability of upward mobility by approximately 1.9 percentage points, or about 3.7% relative to the sample mean. This effect is about half as large as the mobility gains experienced by the first generation of Black migrants who moved to the North between 1940 and 1970, as estimated by Derenoncourt (2022). It is also comparable in magnitude to within-race regional differences in absolute mobility documented by Ward (2023), and to localized geographic variation in upward mobility across US counties in more recent data (Chetty et al., 2014).

The effects of immigration restrictions differ sharply by race. For Black Americans, quota exposure is associated with a positive but imprecisely estimated increase in intergenerational mobility. While these results are not statistically significant, the contrast with the negative effects for white men is consistent with the literature: Black workers and European immigrants—particularly those from Southern and Eastern Europe—were closer substitutes in the labor market (Collins, 1997). By limiting the inflow of immigrant labor, the quotas may have reduced competition in lower-skilled urban jobs, modestly improving occupational prospects for Black men. These findings underscore the distributional consequences of immigration policy: while restrictions hindered upward mobility for US-born white men, they had no negative or even beneficial effects for historically disadvantaged groups, such as Black Americans.

The results are robust to a range of alternative specifications that strengthen the credibility of our identification. First, we find no evidence of pre-trends: the quota shock does not predict intergenerational mobility in the period before 1920. Second, we show that our baseline results—which rely on cross-county variation in exposure derived from the 1900 distribution of immigrants by country of origin—hold when we construct an alternative measure of exposure at the industry level. This measure assigns quota shocks to three-digit industries based on the 1900 immigrant composition of each sector and links it to sons based on their fathers' industry of employment. The estimated effects are very similar. This reduces concerns that results reflect diverging pre-existing county trends or geographic sorting, since the industry-based exposure measure does not rely on cross-county variation. Third, our findings are robust to restricting the sample to urban counties—where most European immigrants settled—and excluding the South—where mobility dynamics and immigrant exposure differed markedly.

Results are also robust to using alternative clustering structures to address potential spatial correlation in the error term, and to controlling for quota exposure in neighboring counties to account for spillovers. Our findings are also unchanged when using alternative measures of intergenerational mobility, including a continuous measure of absolute mobility—defined as the difference between sons' and fathers' occupational scores—and relative mobility. The results continue to hold when restricting comparisons within states, dropping counties with extreme exposure or mobility values, controlling for local Great Depression severity as well as a wide range of

1900 county-level characteristics, and replacing the linked samples from Price et al. (2023) with those from Abramitzky et al. (2022). Finally, we address the concern that our results might reflect correlations between where quota-exposed immigrant groups were settled in 1900 and the underlying characteristics of those counties (Goldsmith-Pinkham et al., 2020). To this end, we interact period dummies with the 1900 county shares of each of the 15 largest immigrant groups (scaled by 1900 national totals), and show that the estimates remain unchanged.

Our results indicate that the effects of the quotas varied by race—negative for whites and positive, though imprecise, for Black Americans. But even within groups, these effects were not homogeneous. Parental resources appear to have played an important role. For whites, the negative effects of the quotas were smaller for sons of richer fathers. Higher income may have attenuated the negative effects of the quotas for white families by facilitating the transmission of tacit knowledge and networks or by enabling migration away from declining local economies. In the context of the Great Depression, Feigenbaum (2015) shows that the sons of richer fathers were more likely to move to areas less exposed to the downturn. We find a similar pattern in our setting: sons of richer white fathers in more exposed counties were more likely to move to counties with stronger economic conditions and higher mobility prospects. Consistent with migration mitigating the shock, the impacts are substantially smaller for movers, whereas the negative effects of the quotas are concentrated among stayers.

To shed further light on the long-run consequences of the quotas, we turn to the 1940 Census—the first census to systematically record individual labor earnings and educational attainment—for the same cohort of men who were children in 1920. Although this analysis is cross-sectional and should therefore be interpreted with caution, it offers additional evidence on the economic impacts of the restrictions. We find that US-born white men from more exposed counties earned substantially lower wages in adulthood: a 5 percentage point increase in quota exposure reduced weekly and hourly wages by 2.6% and 2.3%, respectively. Given that real wages for white men rose by roughly 10–20% between 1920 and 1940, these losses account for about one-tenth to one-fifth of overall wage growth. The effects remain sizeable when controlling for three-digit industry and occupation fixed effects, indicating that the quotas depressed both occupational attainment and productivity within jobs. Employment rates follow a similar pattern: white men from more exposed counties were less likely to be employed in 1940, suggesting that the restrictions reduced labor-market attachment as well as earnings conditional on work.

For Black men, the estimated effects of quota exposure on weekly wages are positive but imprecisely estimated. The corresponding effects on employment and hourly wages are also statistically indistinguishable from zero and quantitatively small. Taken together, the 1940 evidence aligns with our mobility results: restrictions reduced earnings, productivity, and employment for white men, while Black men saw no comparable losses and may have benefited modestly from improved access to jobs previously contested by European immigrants.

In the second part of the paper, we examine the mechanisms. A first possibility is that restricting European immigration induced substitution from unrestricted sources, including from countries with permissive quotas or from domestic migrants. Abramitzky et al. (2023) document such substitution across state economic areas. After confirming that the quotas substantially reduced the populations of restricted immigrant groups, we find only partial substitution at the county level: increases in Canadians and Mexicans, and imprecise increases in US-born migrants, concentrated in urban areas.[2] These patterns indicate that substitution, while present, cannot explain the full magnitude of our results.

A second mechanism is that immigrants complement native workers by raising their productivity and enabling them to specialize in higher-quality tasks (Peri and Sparber, 2009; Ottaviano and Peri, 2012; Foged and Peri, 2016). If so, restricting immigration should have had larger effects in counties more reliant on complementary immigrant groups. To test this hypothesis, we decompose exposure into English- and non-English-speaking immigrant groups and find that the negative mobility effects for whites are nearly twice as large for the latter. A similar pattern appears when comparing Southern and Eastern to Northern and Western European origins: the former—who were less educated, less English-proficient, and concentrated in different industries (Medici, 2024)—generated stronger complementarities and thus larger losses when excluded. For Black men, the signs reverse: exposure to Southern and Eastern and non-English-speaking immigrant restrictions is associated with (impre-

[2] Abramitzky et al. (2023) show that the 1920s quota acts sharply reduced arrivals from Southern and Eastern Europe, but that local labor markets adjusted in heterogeneous ways: in urban areas, lost European immigrants were largely replaced by internal migrants and by immigrants from Mexico and Canada, whereas rural areas shifted toward more capital-intensive agriculture and mining communities contracted due to the absence of substitutable capital. Our results are broadly consistent with these patterns, and we likewise observe substitution—some increases in Canadians and Mexicans and modest inflows of US-born workers in urban counties—but we also find that such substitution is far from complete at the county level, likely reflecting the finer geographic resolution of our data (460 state economic areas in Abramitzky et al., 2023, as opposed to around 2,800 counties in our context).

cise) increases in mobility, consistent with greater labor-market competition between Black workers and these groups in northern cities.

The wage and employment evidence from the 1940 Census reinforces this interpretation. The decline in both weekly and hourly earnings for white men is driven almost entirely by exposure to missing non–English-speaking and Southern and Eastern European immigrants—groups most likely to have provided productivity-enhancing complementarities. Exposure to missing English-speaking or Northern and Western European immigrants, by contrast, yields much smaller or even positive wage coefficients. Employment outcomes follow a similar pattern: white men in counties more exposed to missing non–English-speaking and Southern and Eastern Europeans were significantly less likely to be employed in 1940, although employment also declined following quota exposure to other groups. Taken together, the mobility, wage, and employment results consistently point to immigrant–native complementarities—and their removal under the quotas—as an important mechanism behind the effects of immigration restrictions.

In addition to productivity and occupational channels, a complementary explanation involves human capital investment. If immigrants raise the returns to skills—for example, by complementing educated native workers or by altering the task composition of employment—then restricting immigration could weaken incentives to acquire education. To evaluate this channel, we examine educational attainment in 1940. For white men, we find a negative and statistically significant effect: a 5 percentage point increase in quota exposure reduces years of schooling by approximately 0.16 years—about 1.5% of the sample mean. To assess whether such changes could plausibly account for the wage declines we observe, we combine causal estimates of the Mincerian return to schooling in 1940 from the literature (Feigenbaum and Tan, 2020; Clay et al., 2021) with our estimated effect of the quotas on education to calculate the implied impact on wages. Our calculations imply that reduced schooling can explain at most 22-49 percent of the weekly wage decline, indicating that this channel, while present, is too small to be the main driver of the quotas' overall effects. For Black men, the effect of quota exposure on educational attainment is also negative, but imprecisely estimated and not robust across specifications.

Our paper contributes to a large literature studying how immigration affects the labor market outcomes of native-born workers. This literature has found mixed evidence on whether immigration depresses native wages or displaces native workers, with results often varying by skill level, geography, and time period (Card, 2001; Borjas, 2003; Peri and Sparber, 2009; Ottaviano and Peri, 2012; Lewis and Peri, 2015; Foged and Peri, 2016; Dustmann et al., 2017; Monras, 2020; Dustmann et al., 2025). We complement this literature by examining the consequences of immigration restrictions for the next generation of native-born individuals. The difference in results between white and Black Americans paints a nuanced picture, and underscores the importance of considering distributional consequences when evaluating the long-run impact of immigration policy. While immigration restrictions reduced mobility and earnings for US-born whites, they appear to have had modest positive effects for Black Americans, consistent with reduced labor market competition in segments where immigrants and Black workers were likely to be substitutes. This finding aligns with the "immigrant-as-deterrent" hypothesis proposed by Brinley Thomas and empirically confirmed by Collins (1997), who shows that mass European immigration prior to the 1920s limited Black access to northern labor markets and delayed the Great Migration.

Our paper is also related to recent work that examines how immigration affects intergenerational mobility and long-run outcomes of US-born children after 1980. In line with our results, Borgschulte et al. (2024) find that immigrant inflows during childhood improve educational attainment and income ranks for US-born individuals, particularly in more advantaged areas. Andirin (2025) documents that post-1980 immigration had no economically meaningful effects on the intergenerational mobility of either white or Black children. The null effects for Black children are broadly consistent with our findings; the absence of an effect for white children contrasts with the negative impact of restrictions we estimate in the 1920s setting, underscoring that the relationship between immigration and many important economic outcomes, including intergenerational mobility, possibly varies depending on the context. We complement these papers in two ways. First, we study a different episode and exploit a sharp, policy-induced reduction in immigration—rather than a gradual rise—to identify the effects of immigration restrictions. Second, we show that in the early 20th century, immigration policy had meaningful and heterogeneous consequences for US-born men, generating large mobility losses for white men and modest gains for Black men.

Finally, our paper contributes to the vast literature on intergenerational mobility in the United States. Chetty et al. (2014) document large spatial and temporal differences in mobility, while Chetty et al. (2017) and Chetty et al. (2020) show that absolute mobility has declined sharply since the mid-20th century and that racial disparities remain stark and persistent.[3] We complement these insights by turning to the historical period of the 1920s and examining how a major immigration policy shock shaped the trajectories of US-born Americans. In doing so, we connect contemporary work on the geography of opportunity with historical analyses that use linked census records to trace mobility across groups and cohorts (Feigenbaum, 2015; Abramitzky et al., 2021a; Tan, 2023; Ward, 2023; Abramitzky et al., 2025; Althoff et al., 2025). This research has revealed large racial and regional gaps in economic opportunities and documented how major migration episodes (Derenoncourt, 2022) and wealth shocks (Ager et al., 2021) reshaped intergenerational mobility. We expand on these contributions by showing that immigration policy had long-lasting effects on the mobility of natives, which varied by race.

# 2 Historical Background

## 2.1 The Age of Mass Migration

The Age of Mass Migration, spanning roughly from 1850 to 1920, marked a pivotal era in US demographic and economic history (Hatton and Williamson, 1998; Abramitzky and Boustan, 2017). During this period, more than 30 million Europeans migrated to the United States, transforming the country's labor force, cities, and social fabric. The foreign-born share of the US population rose steadily throughout this period, reaching over 14% by 1920 (Figure A1, Panel A). Initially, arrivals were predominantly from Northern and Western Europe, but over time, immigration shifted toward Southern and Eastern Europe—regions that were poorer and culturally more distant from the native-born US population. By 1920, immigrants from these "new" source regions accounted for nearly half of the foreign-born population (Figure A1, Panel B).

As the immigrant population grew larger and more diverse, nativist anxieties intensified (Goldin, 1994; Higham, 2002). Concerns over labor market competition and the preservation of American cultural identity became increasingly prominent in public discourse. These fears laid the groundwork for calls to limit immigration. The backlash was not only cultural: organized labor, rural constituencies, and some

[3] For a recent review of the intergenerational mobility literature, see Cholli and Durlauf (2022), Abramitzky et al. (2025), and Munoz and Van der Weide (2025).

economists argued that immigration depressed native wages and strained public services. This political climate gave rise to exclusionary measures, including the Chinese Exclusion Act (1882) and the Gentlemen's Agreement limiting Japanese immigration (1907).

In 1907, Congress created the Dillingham Commission—a bipartisan panel tasked with producing a comprehensive study of immigration to the United States. In its final report (1911), the commission drew a sharp distinction between "old" and "new" immigrants, praising the former as more likely to assimilate and warning that the latter posed economic and social risks. It recommended literacy tests and national quotas, laying the intellectual groundwork for later legislation. A central argument advanced in the report was that immigration—particularly of low-skilled workers from Southern and Eastern Europe—was contributing to wage suppression among US-born laborers. The commission concluded that limiting immigration would help raise wages and improve labor conditions for American workers, a view that would become a cornerstone of political support for restriction in the following decade.

The Immigration Act of 1917 imposed a literacy requirement for entry and created an "Asiatic Barred Zone," effectively prohibiting immigration from much of Asia. Although the literacy test had limited impact on overall inflows, it signaled a growing political consensus in favor of tighter restrictions (Goldin, 1994). World War I (WWI) brought transatlantic migration to a near halt, but it also hardened national borders and intensified existing concerns about assimilation, loyalty, and economic competition. When migration resumed after the war, it did so in a transformed institutional and ideological landscape—one that increasingly viewed immigrants as a threat to national cohesion and economic stability (Goldin, 1994; Higham, 2002).

## 2.2 The 1920s Immigration Acts

The restrictive turn in US immigration policy culminated in the early 1920s with a sequence of quota laws that dramatically curtailed entry from abroad and redefined the ethnic and national composition of future arrivals. These acts marked a sharp break with the prior era of largely open European immigration.

In 1921, the Emergency Quota Act capped annual immigration from each European country at 3% of the number of foreign-born individuals from that country recorded in the 1910 US Census. While this law significantly reduced overall inflows—especially from Southern and Eastern Europe—it was soon followed by even stricter legislation. The Immigration Act of 1924 (also known as the Johnson-Reed Act) lowered the quota to 2% and based it on the 1890 Census, further entrenching the advantage of earlier immigrant groups from Northern and Western Europe. The 1924 Act also imposed a total annual ceiling of 150,000 immigrants, effectively eliminated immigration from Asia, and formalized the national origins quota system that would remain in place until 1965 (Abramitzky and Boustan, 2017).

The quota laws achieved their intended effects. Immigration from Southern and Eastern Europe fell dramatically—from over 4 million between 1910 and 1914 to fewer than 800,000 between 1925 and 1929 (Ager et al., 2023). The composition of arrivals shifted sharply toward countries favored by the quota formula, and overall immigrant inflows remained at historically low levels until the Hart-Celler Act of 1965 repealed the national origins system. The foreign-born share of the US population declined steadily, falling to just 5% by 1970 (Figure A1, Panel A). The quota system not only reshaped the future trajectory of American demography; it also constituted one of the most ambitious efforts in US history to engineer the labor market and ethnic composition of the population through immigration policy.

## 2.3 Intergenerational Mobility in the Early 20th Century US

Early work using linked historical censuses suggests that white sons born to low-status fathers in the late 19th and early 20th centuries frequently experienced upward occupational mobility (Ferrie, 2005; Long and Ferrie, 2013). This interpretation has often been taken to imply that cohorts growing up around 1900–1920 faced an unusually fluid opportunity structure, with relatively weak intergenerational persistence compared to later periods. More recent work revises this interpretation by re-examining how mobility is measured in historical data. Ward (2023) shows that estimates based on single observations of fathers' occupations substantially overstate mobility due to classical measurement error and instability in occupational titles over the life cycle. Once these measurement issues are accounted for, the early 20th century appears less as a high-mobility benchmark and more as a transitional period in which upward movement coexisted with substantial intergenerational persistence.[4]

[4] Because we are comparing rates of intergenerational mobility across counties and time, and because we difference out directional bias in the measurement of mobility, the critique of historical mobility measures focused on one mobility parameter (Ward, 2023) does not threaten our main results.

Much of the earlier literature implicitly describes white mobility only, abstracting from the large and persistent racial disparities that characterized the early twentieth century. Accounting for these differences raises estimates of intergenerational persistence for cohorts observed between 1850 and 1940 and further weakens the view of the period as one of uniformly high mobility (Ward, 2023). Recent work shows that intergenerational mobility during this period differed sharply by race. Black Americans born around 1900 faced extremely limited upward mobility in the South, and while the Great Migration improved mobility for movers, it did not eliminate large Black–white gaps (Derenoncourt, 2022). Even within the white population, mobility varied sharply across space, with substantially higher upward mobility in coastal and industrial regions and persistent stagnation in the South (Tan, 2023). Newer linked data confirm that the Black–white gap in economic status was highly persistent across generations, contributing mechanically to lower aggregate mobility when the population is considered as a whole (Ward, 2023; Buckles et al., 2025).

# 3 Data

Our analysis is based on linked samples covering two cohorts observed before and after the introduction of the national origin quotas. We focus on US-born men residing in the contiguous United States in the full-count US Censuses of 1900, 1920, and 1940. To study how exposure to quota restrictions during childhood affects intergenerational mobility, we harmonize county boundaries across census years and assign individuals to 1900 counties using spatial information from IPUMS NHGIS (Manson et al., 2024) and the Census Place Project (Berkes et al., 2023).

**Exposure to the Immigration Acts.** To measure the impact of the 1921 and 1924 immigration acts across counties, we follow Ager et al. (2023), and construct a quota-shock exposure measure, described in detail in Section 4.1. The measure draws on immigration statistics from 1899 to 1930. For the years 1899–1924, we use data from Willcox (1929); for 1925–1930, we rely on the *Statistical Abstract of the United States* (1931, Table 99; U.S. Department of Commerce, 1936, Table 106). These sources also report country-specific quota allocations. In addition, we incorporate annual data on the number of immigrants denied entry at the US border by cause and race, drawn from the *Reports of the Commissioner General of Immigration*, 1900–1930.

**Linking Censuses.** To assess intergenerational mobility, we observe sons when they are aged 0–20, along with their fathers' outcomes, provided the father is co-resident and aged between 20 and 55. Leveraging record-linking methodologies, we track these sons into adulthood using the full-count US Censuses from IPUMS (Ruggles et al., 2024) and the Census Tree algorithm (Price et al., 2023).[5] We construct two linked cohorts to compare mobility before (1900–1920) and after (1920–1940) the implementation of the national origins quota system.[6] Our analysis focuses on two groups: US-born white men and US-born Black men. In both cases, we restrict the sample to individuals whose parents were also born in the US, ensuring that we capture the effects of immigration policy on the US-born population, rather than on the children of immigrants.

**Measuring intergenerational mobility.** Because earnings data are unavailable prior to 1940, we proxy for long-run economic status using occupation.[7] While this limits our ability to measure income directly, occupation-based measures offer several advantages. They are less sensitive to transitory shocks, evolve more smoothly over the life cycle, and are not as affected by differences in the age at which individuals are observed. Occupational status thus provides a more stable proxy for lifetime economic outcomes and mitigates life-cycle bias in intergenerational comparisons.

We focus on absolute occupational mobility, defined as whether sons attain an occupation with higher economic standing than their fathers. The literature also considers relative mobility, which captures the degree of rank persistence between parents and children (Abramitzky et al., 2025). We focus on absolute mobility because, especially in periods of rapid structural transformation—such as the early 20th-century United States (Eckert and Peters, 2025)—relative mobility may fail to capture improvements in living standards and thus obscure economically meaningful progress.

We define absolute mobility using a binary indicator for several reasons. First, it captures a clear and policy-relevant notion of upward mobility—whether sons surpass their fathers in occupational status. Second, it is straightforward to interpret across demographic groups and regression specifications, and is less sensitive to measurement noise or to changes in the scaling of occupation scores. Third, because we observe

[5] As shown below, results are robust to using the linked samples from Abramitzky et al. (2022).

[6] We study these 20-year census-to-census links to ensure that we can observe fathers and sons as adults. We do not include links from 1910 to 1930 because the quota exposure occurs in 1921 and 1924, and so it is unclear whether the sons of 1910 (who would be observed as adults in 1930) are treatment or control. By focusing on two linked census samples with clearer control (1900 to 1920) and treatment (1920 to 1940) status, our event study is cleaner.

[7] We complement these occupation-based measures with income data from the 1940 full-count Census.

actual earnings in 1940, we can verify that these occupational transitions correspond to meaningful differences in long-run economic outcomes. For completeness, we also present results using a continuous measure of absolute mobility—defined as the difference between sons' and fathers' adjusted occupational scores—as well as results based on relative mobility.

To measure occupational status, we use occupation scores based on the "adjusted Song rank" from Ward (2023). The original Song score, developed by Song et al. (2020), ranks occupations by the average educational attainment or literacy of men in a given birth cohort, generating a percentile-based status measure that is cohort-specific. However, Ward (2023) highlights that these "occupation-only" scores may obscure important variation by race and region: the same occupation could imply very different levels of human capital and labor market outcomes across groups. To address this, Ward (2023) adjusts the Song scores by computing average education within occupation-by-race-by-region-by-birth-decade cells, generating a more accurate status measure that reflects persistent structural inequality.

We extend this adjustment to additionally account for nativity. This is important because the national origin quotas may have altered the selection of workers into specific occupations and industries. Since native-born white men tended to have higher education levels than immigrants (Carter et al., 2006), a shift in the nativity composition of occupations—driven by immigration restrictions—could mechanically raise the average human capital of an occupation by cohort cell. Without adjusting for nativity, this would spuriously inflate occupational status scores. Our approach ensures that we attribute changes in occupational standing to actual upward mobility, rather than to compositional shifts in who enters a given job.[8]

**Summary statistics.** Figure 1 displays maps of intergenerational mobility, based on our adjusted occupation score measure, for the cohort of white (Panel A) and Black (Panel B) men observed as children in 1920 and as adults in 1940. The maps partial out Census region fixed effects to align with the empirical specification used in the analysis. Mobility varies widely across space for both groups. Among US-born white men, upward mobility is highest across much of the Midwest, particularly in states such as Illinois, Ohio, and Wisconsin, and in parts of the South, including Texas. For US-born Black men, mobility is more dispersed, with relatively higher rates in the

[8]Our adjusted scores are highly correlated with the original scores developed by Song et al. (2020) and with the adjusted measures used in Ward (2023). The correlations are 0.81 and 0.90, respectively.

Northeast, parts of the Midwest, and selected Southern counties.[9]

Table A1 reports summary statistics for the linked samples of white (Panel A) and Black (Panel B) men, and compares these to the full count censuses. Our main sample includes 10,541,094 white and 913,447 Black individuals across the two cohorts. Among whites, approximately 30% lived in urban areas during childhood, increasing to 45% in adulthood. In the full-count Census, the corresponding shares are 31% and 57%, suggesting that our linked sample somewhat over-represents individuals living in rural areas as adults. For Black men, who were more concentrated in the rural South during this period (Collins, 1997), the corresponding shares are 18% and 41%, respectively. In the full-count Census, the urban shares were 17% in childhood and 52% in adulthood, again indicating rural over-representation in the linked sample.

Occupational ranks are broadly similar across datasets. In the linked sample, white fathers average a score of 56, compared with 55 for their sons; in the full count, the figures are 55 and 59, respectively. For Black families, fathers and sons average 7 and 14 in both samples. In the linked data, 43% of white sons surpass their fathers' occupational rank, compared with 55% of Black sons—reflecting higher absolute mobility for Black men despite, or perhaps because of, their fathers' much lower initial occupational standing. Panel C summarizes the main variables for the 2,810 counties in our sample, including the quota exposure measure constructed in Section 4.1 below.

# 4 Empirical Strategy

## 4.1 Calculating Quota Exposure

To estimate the effects of immigration restrictions on intergenerational mobility of US-born men, we construct a county-level measure of exposure to the quotas. Our identification strategy leverages variation in pre-quota immigrant settlement patterns across counties. Because the national origin quotas imposed differential restrictions by country, counties with a larger share of immigrants from heavily restricted origins experienced a greater decline in immigrant inflows after the legislation. As in Ager

[9]Figure A2 presents the geographic distribution of the raw intergenerational mobility rates.

et al. (2023), we define quota exposure as:

$$Exposure_c = \sum_j \frac{\alpha_{cj,1900}}{Pop_{c,1900}} \times Missing_j, \tag{1}$$

where $\alpha_{cj,1900}$ denotes the number of immigrants from country $j$ residing in county $c$ in 1900, relative to all immigrants from country $j$ living in the US in that year. $Pop_{c,1900}$ is the total county population in that year. $Missing_j$ captures the intensity of restriction for country $j$, measured as the predicted number of missing immigrants due to the quota.

Following Ager et al. (2023), $Missing_j$ is defined as the cumulative difference between predicted and actual immigrant inflows from country $j$ after the introduction of the quotas:

$$Missing_j = \sum_{t=1922}^{1930} \max\left\{\widehat{M}_{jt} - M_{jt},\, 0\right\}, \tag{2}$$

where $M_{jt}$ denotes the observed number of immigrants from country $j$ entering the United States in year $t$, and $\widehat{M}_{jt}$ is the counterfactual inflow predicted using pre-quota migration trends for that country. The max operator ensures that only differences attributable to binding quota restrictions contribute to $Missing_j$.[10]

Figure 2, Panel A, plots the geographic distribution of our quota exposure measure, the share of predicted missing immigrants relative to the 1900 county population. Exposure is highest in the Northeast, Midwest, and California, reflecting historical settlement patterns of Southern and Eastern European immigrants who were disproportionately affected by the national origin quotas. Panel B of Figure 2 displays the residualized version of the exposure measure, after partialling out Census region fixed effects. It highlights substantial within-region variation.

[10] As in Ager et al. (2023), we obtain the predicted inflow $\widehat{M}_{jt}$ by estimating, separately for each origin $j$:

$$M_{jt} = \beta_{1j} \ln t + \beta_{2j} (\ln t)^2 + \varepsilon_{jt}, \tag{3}$$

In line with Ager et al. (2023), we focus on the 1900–1914 period to capture country-specific migration trends prior to the disruptions induced by WWI. Then, we use the fitted values from equation (3) to generate $\widehat{M}_{jt}$ for $t = 1922-1930$. If a fitted $\widehat{M}_{jt}$ is negative, we set $\widehat{M}_{jt} = 0$.

## 4.2 Baseline Estimating Equation

To evaluate the effects of immigration quotas on the intergenerational mobility of US-born men, we estimate a difference-in-differences specification that compares changes in mobility across counties differentially exposed to the quotas. We use two linked samples—sons observed in 1900 and 1920 linked to their adult records in 1920 and 1940, respectively—constructed from the full-count US Censuses, separately for white and Black men born in the United States to US-born parents. We estimate:

$$y_{ict}^{s,f} = \theta_c + \gamma_{dt} + \beta Exposure_c \times Post_t + X_{ict} + \epsilon_{ict} \tag{4}$$

where $y_{ict}^{s,f}$ is an indicator for absolute occupational mobility—equal to one if the son's occupation score exceeds that of his father—for individual $i$ observed in Census year $t$, living in county $c$ when young. The variable $Exposure_c$ denotes the county-level quota exposure measure described in Section 4.1, and $Post_t$ is a dummy equal to one for the post-restriction period (1940).

All regressions include county fixed effects ($\theta_c$) and Census region by period fixed effects ($\gamma_{dt}$) to absorb persistent differences across counties and region-specific shocks. These and all other geographic variables are measured based on the location of residence in childhood, and therefore correspond to the father's county and Census region. $X_{ic}$ includes dummies for the age of the individual when young as well as the interaction between $Post_t$ and county-level characteristics measured in 1900. To account for the fact that exposure is mechanically increasing in immigrant population size, the baseline specification interacts $Post_t$ with the log of the foreign-born population in 1900, as well as with the log of the total county population. We cluster standard errors at the county level.

The coefficient of interest, $\beta$, is estimated by comparing counties with different settlement patterns of immigrants from restricted countries before and after the quotas. The identification assumption is that, absent the quotas, counties with higher shares of immigrants from affected countries would have exhibited parallel trends in absolute mobility, conditional on controls and fixed effects. Consistent with this assumption, we find no evidence of pre-trends: counties with higher quota exposure do not exhibit differential mobility trends prior to the implementation of the quotas. We present additional robustness checks in Section 5.2 after discussing the main results.

# 5 Results

## 5.1 Baseline Estimates

We present estimates from equation (4) in Table 1, reporting results for US-born white men in Panel A and for US-born Black men in Panel B. Column 1 reports the baseline specification. For white men, the estimated coefficient is negative and statistically significant: after the quotas, individuals in more exposed counties were less likely to have an occupational status higher than that of their fathers. The magnitude is sizable: a 5 percentage point increase in quota exposure reduces the probability of upward mobility by 1.90 percentage points, or 3.70% relative to the sample mean. This is equivalent to roughly one-half to one-third of the total occupational advancement that US-born whites living in urban areas could expect over a 20-year period at the time. For comparison, real wages for US-born white men increased by approximately 10–20% between 1920 and 1940 (Carter et al., 2006; Goldin and Katz, 2008). If occupational advancement tracks long-run economic progress, our estimates imply that the quota-induced mobility loss was substantial in light of typical gains over a generation.

For Black men, the coefficient in column 1 is positive, although not statistically significant. Nonetheless, the implied magnitude is nontrivial: a 5 percentage point increase in exposure is associated with an increase in absolute mobility of 1.12 percentage points, or 2.22% relative to the sample mean. For context, Derenoncourt (2022) finds that migrating from the South to the North during the second Great Migration (1940-1970) increased upward mobility for Black men by approximately 4.3 percentage points. Our estimated effect, though imprecise, is roughly a quarter as large—suggesting that immigration restrictions may have modestly expanded occupational opportunities for Black men, particularly in industrial labor markets.

While imprecise, the contrast in signs between the white and Black estimates is consistent with the interpretation that immigration restrictions affected racial groups differently, likely due to differences in labor market segmentation and the degree of substitutability with immigrant workers. Southern and Eastern European immigrants arriving during the Age of Mass Migration were often concentrated in low-skilled urban jobs (Hatton and Williamson, 1998), many of which overlapped with the sectors available to Black workers—particularly in the North. According to the "immigrant-as-deterrent" hypothesis, originally proposed by Brinley Thomas and empirically tested by Collins (1997), the large inflow of European immigrants prior to the 1920s limited the occupational and geographic mobility of Black Americans by crowding them out of northern labor markets. Consistent with this view, our results suggest that immigration restrictions modestly expanded opportunities for Black men by easing competition in industrial employment.

In contrast, US-born white workers were more likely to be complements to immigrant labor. A large body of economic research shows that immigration can increase native wages and employment by pushing natives into higher-paying or more skilled tasks (Peri and Sparber, 2009; Ottaviano and Peri, 2012; Foged and Peri, 2016). In such settings, restricting immigration reduces the productivity of native workers, as they are forced into less specialized or lower-quality jobs. Our finding that mobility declined for whites in more exposed counties is consistent with this mechanism, suggesting that the removal of immigrant complements reduced occupational advancement for US-born whites. We document further supporting evidence for this channel in Section 6 below.

## 5.2 Robustness Checks

The remainder of Table 1 documents that the main results in column 1 are robust to alternative samples, specifications, and controls.

**Urban and non-southern samples.** Columns 2 and 3 report estimates for two subsamples: individuals living in urban counties (column 2) and those residing outside the South (column 3).[11] The results remain stable. These subsample analyses are important for two reasons. First, quota exposure was higher in urban counties and outside the South, where European immigrants were historically concentrated (Abramitzky et al., 2023; Ager et al., 2023). Second, Black Americans during this period were disproportionately concentrated in rural areas and in the South, where higher discrimination limited their economic opportunities and resulted in lower intergenerational mobility (Collins and Wanamaker, 2022; Althoff and Reichardt, 2024). The stability of coefficients suggests that our findings are not driven by differences in regional or urban composition.

[11] We classify a county as urban if it had a positive share of urban population in 1900. In that year, the mean and median urban shares across all counties were 13.27% and 0, respectively. We define the South following the US Census Bureau classification, which includes the following states: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia, and the District of Columbia.

**Testing for pre-trends.** Next, we test for differential pre-trends in intergenerational mobility. Specifically, we examine whether counties that would later be more exposed to the quota shock were already on diverging mobility trajectories before 1920. To do so, we construct a pre-period sample by linking sons aged 0–20 in 1880 to their adult outcomes in 1900 and measure mobility relative to their fathers. We then estimate a version of equation (4) where the outcome is pre-quota mobility and the key regressor is quota exposure. The estimated coefficients are shown in column 4. For whites (Panel A), the coefficient is close to zero and statistically insignificant. For Black Americans (Panel B), the estimate is also statistically insignificant and negative. These findings suggest that counties more exposed to the quotas were not already on different mobility paths prior to the policy change.

**Industry-level quota exposure.** One remaining concern is that, despite the placebo check shown in column 4, quota exposure may be correlated with unobserved county-level trends in mobility. To address these concerns, we replicate the analysis using an alternative measure of quota exposure based on industry-level variation. The intuition follows our main approach: in 1900, immigrant workers from different countries were unevenly distributed across industries. We exploit this variation to construct a measure of industry-level exposure, defined as the weighted average of the national quota shock across origin countries, where weights reflect the share of immigrants from each country employed in a given industry in 1900.

This measure captures how strongly each industry was affected by the quotas based on its pre-restriction immigrant composition. We then assign this industry-level exposure to individuals using the industry reported for their fathers in the 1900 and 1920 censuses. In this alternative specification, we replace county fixed effects with industry fixed effects and adjust our controls accordingly. Specifically, we interact the post-quota indicator with 1900 industry-level foreign-born employment and total employment. We continue to control for Census region-by-decade fixed effects based on childhood location.

Results are shown in column 5. For the white sample (Panel A), the coefficient remains negative and statistically significant, reinforcing our main finding that quota exposure reduced intergenerational mobility for native-born whites. For the Black sample (Panel B), the coefficient becomes negative and close to zero, but remains statistically insignificant.[12]

[12] Results, not reported for brevity, are virtually unchanged when augmenting the specification in column 5 by

**Continuous mobility measure.** In column 6, we replicate the analysis using a continuous measure of mobility, defined as the difference between the son's and father's adjusted occupational scores. For white men, the coefficient remains negative and statistically significant, confirming the results from the baseline specification. The magnitude is economically meaningful: a 5 percentage point increase in quota exposure reduces the score gap by approximately 0.409 points, which corresponds to about 25% of the mean intergenerational score difference. For Black men, the coefficient is also negative—unlike in the baseline binary specification—but quantitatively small and statistically insignificant.

**Relative mobility.** As discussed above, we focus on absolute mobility because, in a historical setting characterized by rapid structural change, it more directly captures economically meaningful improvements in living standards. Nonetheless, in Table A2 we replicate our main analysis using a measure of relative intergenerational mobility. We measure relative mobility as the strength of the association between sons' and fathers' occupational ranks, and augment our baseline specification by interacting fathers' rank with quota exposure and the post-quota indicator, fully saturating the regression with all lower-order interactions. The dependent variable is the son's occupational rank.

Columns 1–3 report results for US-born white men, while columns 4–6 focus on US-born Black men. For white men, the coefficient on the triple interaction in column 1 is positive, indicating that quota exposure increased intergenerational persistence by strengthening the relationship between fathers' and sons' ranks, although the estimate is imprecisely estimated. The magnitude of the effect is larger when restricting the sample to individuals living in urban counties (column 2) and to those residing outside the South (column 3); in the latter case, the increase in persistence—corresponding to lower relative mobility—is statistically significant. For Black Americans, the estimated effects are qualitatively similar.

**Spatial correlation and spillover effects.** Columns 2 and 3 of Table A3 address potential concerns about spatial correlation by clustering standard errors at the Standard Economic Area and state level, respectively. In both cases, the estimates remain statistically significant. The remainder of Table A3 examines whether quota exposure in nearby counties affected local outcomes. To account for such spillovers, we augment the baseline specification by including average quota exposure in neighboring

controlling for father's county of residence fixed effects.

counties and in other counties within the same state, interacted with the post indicator. Columns 4–5 add these controls directly, while columns 6–7 construct the quota exposure in other counties weighting by the population of the neighboring counties or other in-state counties. Reassuringly, results are close to the baseline (presented in column 1).

**Allowing for differential trends.** A potential concern with our main results is that counties more exposed to the quotas may have already been on different trajectories due to pre-existing characteristics. The placebo test in column 4 and the industry-based exposure design in column 5 help to mitigate this concern. We further address it in Figure A3, where we allow counties to follow differential trends based on observable characteristics prior to the quotas. Specifically, we interact the post-quota indicator with a vector of county-level variables measured in 1900: urban population, Black population, the number of literate individuals, and the number of workers in manufacturing and agriculture. We present results for white men in Panel A and for Black men in Panel B. In both cases, the estimated effects remain similar to our baseline, reported as the top coefficient in each panel.

**Controlling for Great Depression effects.** A related concern is that the decline in white mobility for post-quota cohorts could reflect the effects of the Great Depression rather than the quotas themselves. To rule this out, we extend the baseline specification by interacting the post-quota indicator with a measure of local Depression severity—the change in retail sales per capita between 1929 and 1933.[13] Depression severity is uncorrelated with quota exposure: the cross-county correlation between the two measures is close to zero.[14] This alleviates concerns that our estimates confound immigration restrictions with Depression-era shocks. Consistent with this, the estimated effect of quota exposure—shown at the bottom of Figure A3—remains virtually unchanged.

**Controlling for initial immigrant shares.** One remaining concern is that our results may be driven by the pre-existing geographic distribution of specific immigrant groups—especially those most restricted by the quotas—if those settlement patterns are correlated with underlying trends in intergenerational mobility. To address this, we interact the post-treatment indicator with the 1900 county shares of immigrants

[13]This measure has been widely used to capture local economic distress during the Depression (Fishback et al., 2005, 2007, 2010; Feigenbaum, 2015).

[14]The raw cross-county correlation between the quota exposure and the Depression severity is -0.0003; the one partialling out the Census region fixed effect is -0.0015.

from each origin group. In Figure A4, we add these interactions one at a time for the 15 most quota-restricted nationalities, as well as jointly for the top 5, top 10, and top 15 groups. In all cases, the estimates remain very close to the baseline (shown as the first coefficient), indicating that our findings are not driven by the initial clustering of specific quota-exposed groups.

**Alternative linked samples.** Our baseline results are based on linked samples from Price et al. (2023). In Table A4, we replicate the main results using the linked samples from Abramitzky et al. (2022). Both datasets draw on the full-count US Censuses, but differ in scope and linking approach: Price et al. (2023) provide broader coverage using machine-learning methods, while Abramitzky et al. (2022) produce smaller samples through more conservative linking algorithms. Reassuringly, the results remain very similar.[15]

**Additional robustness checks.** In Figure A5, we present additional robustness checks. First, we re-estimate our baseline specification, interacting the post-dummy with state (rather than Census region) fixed effects, thereby accounting for potential unobserved heterogeneity in state-level policies or institutions. Second, we show that our findings are not driven by extreme values in the distribution of either quota exposure or intergenerational mobility. We sequentially drop the bottom 10% of counties with the lowest exposure, the top 10% with the highest, and both tails of the intergenerational mobility distribution. Finally, we augment the specification with dummies for the father's 3-digit industry code. In all cases, the estimated coefficients remain similar to our baseline estimates.

## 5.3 The Role of Parental Income

Section 5.1 shows that the quota effects varied sharply by race. Yet, even within racial groups, families did not experience the shock in the same way. Resources passed from parents to children—financial, social, or informational—may have shaped the ability to adapt. Feigenbaum (2015) documents this in the Great Depression, when sons of richer fathers were more likely to migrate to healthier labor markets. Motivated by this evidence, we investigate whether parental income cushioned the impact of the quotas. To do so, we augment the baseline specification by interacting the *post* ×

[15] Table A5 compares the characteristics of the Census Tree and the Census Linking Project samples and confirms that they are very similar, even though the Census Linking Project sample is smaller. Robustness across linking methods is common in the historical census linking literature (Abramitzky et al., 2021b).

*quota exposure* term with an indicator for fathers with above-median Song scores, fully saturating the regression with lower-order interactions.[16] Results are reported in column 1 of Table 2.

For whites (Panel A), the main effect of quota exposure remains negative and statistically significant, while the interaction with high father's income is positive and statistically significant at the 10% level. This indicates that sons of richer fathers were partly shielded from the negative impact of the quotas. For Black Americans (Panel B), the pattern is reversed: the main effect is positive, while the interaction is negative and significant at the 10% level, suggesting that the modest gains from the quotas accrued disproportionately to poorer Black families, who were more likely to have competed directly with European immigrants.

Why did richer white families fare better? One possibility is that higher parental income preserved access to good jobs or transmitted knowledge and connections that eased job search. Another is that it relaxed credit constraints on migration, allowing sons to leave counties where opportunities had contracted. Feigenbaum (2015) shows that during the Great Depression, sons of richer fathers were more likely to move to areas less exposed to the downturn. To test whether a similar mechanism operated here, we examine whether sons lived in a different county in adulthood than in childhood. As shown in column 2 of Table 2, the main effect for whites is small and imprecise, but the interaction is positive and statistically significant: sons of richer fathers in more exposed counties were more likely to migrate. For Black Americans, coefficients go in the opposite direction, but are imprecisely estimated.

One implication of this mechanism is that quota effects should be concentrated among those who remained in their childhood county, whereas movers should experience weaker effects. To explore this, Table A6 estimates the baseline specification separately for stayers (column 2) and movers (column 3). For whites, the coefficient on quota exposure is roughly twice as large (in absolute value) for stayers as for the full sample, whereas for movers it is substantially smaller and less precisely estimated. Although these patterns are only suggestive—since migration is itself an outcome and movers may differ systematically from stayers—they align with the idea that mobility mitigated the impact of the quotas. Columns 4–6 repeat the exercise for Black Americans. The estimates are noisier, but the positive baseline effect appears to be driven

[16] The specification further interacts the indicator for fathers with above-median Song scores with county fixed effects.

primarily by movers (column 6), again with the same caveats regarding selection.

These findings raise the question of whether richer sons also migrated to better destinations. Table 2 examines this possibility in columns 3–6. Columns 3–4 use as outcomes indicators for moving to a county with average Song scores, measured in childhood, above or below those in the origin county. Columns 5–6 instead use indicators for moving to counties with higher versus lower intergenerational mobility (measured over the childhood period).[17] For whites (Panel A), the main effect of quota exposure is small and imprecise, while the coefficient on the triple interaction term is positive and statistically significant when the destination is higher-ranked or has higher intergenerational mobility, and negative and imprecise when the destination is lower-ranked or less mobile. That is, richer white sons were able to move to better locations. For Black Americans (Panel B), the coefficients are reversed, but noisier.

## 5.4 Evidence from the 1940 Census

To provide additional evidence on how immigration restrictions shaped the labor-market outcomes of the 1920 cohort, we turn to the 1940 full-count Census. This is the first census to report income, weeks, and hours worked in a systematic way, allowing us to study adult earnings in a more granular manner than the intergenerational mobility measures alone permit. Although the analysis relies on a single cross-section and should therefore be interpreted as suggestive, it offers a complementary perspective on the economic consequences of the quotas. Table 3 reports these estimates for white and Black men in Panels A and B, respectively. We analyze (log) weekly and hourly wages in columns 1-3 and 4-6, respectively.

For white men, the wage results closely mirror the decline in intergenerational mobility documented in Table 1. Column 1 of Table 3 shows that a 5 percentage point increase in quota exposure is associated with a statistically significant 2.6% reduction in weekly wages. Given that real wages for US-born white men rose by roughly 10–20% over 1920–1940 (Carter et al., 2006; Goldin and Katz, 2008), this loss amounts to about one-tenth to one-fifth of total wage growth. Columns 2 and 3 assess whether this decline reflects sorting across or within sectors. Controlling for three-digit industry fixed effects (column 2) leaves the magnitude and the precision of

[17] We define intergenerational mobility as the average adjusted occupational scores of sons with fathers' scores in the bottom quartile, measured in the childhood period. We use the 1880-1900 and 1900-1920 linked samples to construct the intergenerational mobility measures, respectively.

the coefficient unchanged, indicating that the effect occurs within industries. When controlling for occupation fixed effects (column 3), the point estimate remains statistically significant but becomes 20% smaller. This implies that at least one-fifth of weekly wage losses reflects occupational downgrading, consistent with the idea that immigration facilitated occupational upgrading for white natives and the quotas impeded it.

Columns 4–6 report estimates for hourly wages. In column 4, the coefficient remains negative and statistically significant, consistent with a decline in labor productivity. According to our estimates, a 5 percentage point increase in quota exposure is associated with 2.3% lower hourly wages for white men. The effect persists when controlling for industry (column 5) and occupation (column 6) fixed effects. This suggests that productivity declined even within narrowly defined industry and occupational categories. Taken together, these results support the interpretation, also discussed in Section 6, that immigration restrictions undermined productivity-enhancing complementarities.

For Black men, the coefficient on quota exposure in column 1 is positive but imprecisely estimated and not statistically significant, mirroring the intergenerational mobility results. The point estimate suggests that a 5 percentage point increase in quota exposure increases weekly wages for Black Americans by 2.1%—nontrivial in magnitude but not statistically distinguishable from zero. The effect becomes somewhat smaller when controlling for industry (column 2) and occupation (column 3) fixed effects. These results suggest that, if anything, Black workers may have experienced wage gains driven by occupational reallocation. Columns 4–6 examine log hourly wages and reveal a similar pattern. In column 4, the coefficient is positive and imprecise, consistent with the mobility and weekly wage results. The point estimate becomes again smaller when controlling for industry (column 5) and occupation (column 6) fixed effects.

Columns 1–6 examined intensive-margin effects by focusing on earnings among those who were employed. In column 7, we turn to the probability of being employed, capturing the extensive-margin impact of the quotas.[18] We find that whites in more exposed counties were less likely to be employed: a 5 percentage point increase in the share of missing immigrants reduces the employment rate by 1.29 percentage points

[18]Employment status is available only in the 1910, 1930, and 1940 Censuses, so we cannot analyze employment dynamics over time or control for the unemployment status of fathers in our linked sample.

(1.38% relative to the mean). For Black men, the coefficient is positive—consistent with our other results—but imprecisely estimated. Taken together, these patterns indicate that the slowdown in white intergenerational mobility reflects both intensive-margin losses (lower earnings among workers) and extensive-margin effects (reduced employment).

# 6 Mechanisms

## 6.1 Migration Responses to the Quota Restrictions

One potential mechanism behind our results is that missing immigrants from highly restricted origins may have been replaced by inflows from unrestricted groups—either immigrants from countries not subject to quotas (e.g., Canada and Mexico) or US-born internal migrants. If these replacement migrants were closer substitutes for US-born whites than the restricted Southern and Eastern Europeans they replaced, such substitution could generate the patterns we observe. This interpretation does not undermine our results, but might have policy implications for how local labor markets adjust to immigration restrictions.

Abramitzky et al. (2023) document substantial substitution responses to the quotas: in the 460 State Economic Areas (SEAs) they analyze, urban labor markets that lost European immigrants attracted internal migrants and immigrants from Mexico and Canada, while rural areas shifted toward more capital-intensive agriculture and mining communities contracted. Their spatial unit is much coarser than ours (roughly 460 SEAs versus around 2,800 counties), and the channels they document need not map directly to the finer geographic scale at which we measure intergenerational mobility.

Similar to Abramitzky et al. (2023), we estimate panel regressions for 1900, 1920, and 1940, controlling for county fixed effects and Census region–by–period fixed effects, and expressing population growth for each demographic group as its change relative to its baseline size in the prior period.[19] Table A7 reports the relationship between quota exposure and population growth across these groups. Panel A presents results for all counties; Panel B restricts the sample to urban counties, allowing us

[19] We weight observations by the number of individuals in the linked samples at the county level to keep the analysis as comparable as possible to our individual-level regressions. We cluster standard errors at the county level.

to assess whether substitution responses were concentrated in more urbanized labor markets.[20]

Column 1 considers the foreign-born population from restricted origins. The negative and statistically significant coefficient confirms that counties more exposed to the quotas experienced larger declines in the targeted immigrant groups—effectively a first stage for our analysis. Column 2 examines total population growth. In the full sample, quota exposure is associated with a sizable and statistically significant decline in total population, consistent with limited offsetting inflows. In urban counties, the coefficient remains negative but is smaller in magnitude and imprecisely estimated. These patterns suggest that any substitution occurred primarily in urban labor markets, but did not fully replace the restricted immigrant losses. Column 3 pools the population of US-born residents and immigrants from unrestricted origins (e.g., Canada, Mexico, the Caribbean). The point estimate is negative in the full sample but positive in the urban sample, although neither is statistically significant. This pattern mirrors the idea that substitution was limited in aggregate but somewhat more pronounced in urban counties.

The remaining columns separately analyze foreign-born (column 4) and US-born populations (columns 5 to 7). Foreign-born populations decline sharply in more exposed counties—driven by the loss of restricted-origin immigrants. For the US-born population, the point estimates are small and statistically insignificant, but the signs differ across groups: modest declines for whites and a near-zero effect for Black men. These heterogeneous and imprecise patterns suggest that internal migration only partially compensated for the lost immigrant labor and that any such responses varied across demographic groups.

Additional evidence, reported in Table A8, further supports these patterns. Consistent with Abramitzky et al. (2023), we find that counties more exposed to the quotas experienced small but statistically significant increases in Mexican (column 1) and Canadian (column 2) immigrants, as well as positive inflows of unrestricted Europeans (column 3) in urban areas.[21] These substitution responses are modest in magnitude, but match the direction of the adjustments documented at the SEA level by Abramitzky et al. (2023). Taken together, these results indicate that migration responses to the quotas—while present in some settings—were far from complete at

[20] As in Table 1, we define urban counties as those with a positive urban population share in 1900.

[21] Table A8 (column 4) also shows that quota exposure is not associated with increases in the Southern-born Black population in non-Southern counties.

the county level.

## 6.2 Immigrant-Native Complementarities

A large body of research suggests that immigrants can complement native workers by raising their productivity and facilitating occupational upgrading. When immigrants specialize in certain tasks or segments of the labor market, natives may reallocate toward higher-status or more skill-intensive roles (Peri and Sparber, 2009; Ottaviano and Peri, 2012; Foged and Peri, 2016). Tabellini (2020) shows that immigration in the early 20th century expanded employment opportunities for native-born whites by fostering the growth of the manufacturing sector. In cities more exposed to immigration, US-born white workers moved up the occupational ladder, consistent with models of immigrant-native complementarities.

Restricting immigration may have therefore reversed these gains. If the quotas disproportionately removed groups whose skills were complementary to those of US-born white workers, we should expect larger declines in whites' intergenerational mobility in counties more exposed to the loss of these groups. Conversely, if Black workers were more likely to compete with the excluded immigrants, the quotas may have eased competition for some jobs and thus mitigated or even reversed the impact on Black men. Although we cannot directly estimate complementarity and substitutability elasticities for the early 20th century, we can examine whether the effects of the quotas vary systematically with the characteristics of the excluded immigrants. We implement a series of exercises that, taken together, provide suggestive evidence on this mechanism. Table 4 reports the results for US-born white men. The baseline results for Black Americans are considerably noisier—and thus more difficult to interpret; however, for completeness, we present this analysis in Table A9.[22]

**Intergenerational mobility.** Columns 1 and 2 re-estimate the baseline specification in equation (4) using alternative constructions of quota exposure. In column 1, we divide countries of origin according to whether immigrants from that country reported above- or below-median English proficiency in the 1900 Census.[23] This

[22]To facilitate comparisons across coefficients within each table, all regressors are standardized to have mean zero and unit standard deviation.

[23]For each country of origin, we compute the share of immigrants aged 15 to 65 residing in the United States in 1900 who reported that they could speak English. We then split the distribution at the median of this measure and classify countries above the median as "English speaking" and those below as "non-English speaking." Using this approach, the English-speaking group includes: Albania, Andorra, Bulgaria, Denmark, England, Gibraltar, Ireland, Liechtenstein, Malta, Scotland, Sweden, Switzerland, Wales. The non-English-speaking group comprises: Austria, Belgium,

classification is necessarily imperfect and likely captures broader differences across origin groups—such as education, occupational mix, or settlement patterns—beyond English proficiency alone. With this caveat in mind, results indicate that both exposure measures are statistically significant for whites, but the coefficient on exposure to non-English-speaking immigrants is nearly twice as large in magnitude. Although the confidence intervals overlap, the difference is suggestive: the loss of less substitutable, more complementary immigrants appears to have a larger effect on the decline in white intergenerational mobility. For Black men (Table A9), exposure to missing non-English-speaking immigrants is associated with a statistically imprecise increase in mobility, while exposure to missing English-speaking immigrants is associated with a small, negative, and again imprecise effect.

In column 2, we divide immigrants into Southern/Eastern Europeans (S/E) and Northern/Western Europeans (N/W). The S/E groups were less educated, less likely to speak English, and were disproportionately concentrated in occupations and tasks that complemented the skills of US-born white workers (Tabellini, 2020; Medici, 2024). As with the English-proficiency split, this classification is imperfect: S/E and N/W immigrants differed along a range of other dimensions—including cultural distance, settlement patterns, and social integration (Higham, 2002; Fouka et al., 2022)—that may also shape their economic interactions with natives. Notwithstanding these limitations, the results mirror those above: for whites, the estimated effect of exposure to missing S/E immigrants is larger (in absolute value) than the corresponding effect for N/W immigrants, while for Black men, the pattern reverses—exposure to missing S/E immigrants is associated with more upward mobility than exposure to missing N/W immigrants.

In most cases, the coefficients across immigrant groups are not statistically distinguishable from one another, and neither split perfectly isolates complementarity patterns—since these groups also differ along many other dimensions. Yet, the consistency of the sign patterns across specifications is informative. Taken together, the intergenerational mobility estimates in columns 1–2 align with the complementarity–substitutability mechanism: the loss of more complementary groups (non–English-speaking and S/E immigrants) has larger negative effects for whites and more positive effects for Black men.

Czechoslovakia, Finland, France, Germany, Greece, Hungary, Iceland, Italy, Lithuania, Luxembourg, Netherlands, Norway, Russia, Poland, Portugal, Romania, Spain, Yugoslavia.

**Income and employment.** The remainder of the table provides additional, suggestive evidence by estimating cross-sectional regressions using data from the 1940 census. Columns 3–4 examine weekly wages, columns 5–6 focus on hourly earnings, and columns 7–8 analyze employment probabilities for the 1920 cohort (adults in 1940). The weekly earnings results closely mirror the mobility patterns: for whites, declines are driven by exposure to missing non–English-speaking and S/E immigrants. The hourly earnings results are similar to the weekly wages. For whites, exposure to missing non–English-speaking and S/E immigrants is associated with large and statistically significant wage losses, whereas the coefficients for English-speaking and N/W immigrants are positive and significant—precisely the pattern one would expect if the former were stronger complements. For Black men, exposure to missing English-speaking and N/W immigrants yields positive and statistically significant effects on hourly earnings, though the interpretation is less clear given the noisier estimates elsewhere.

Finally, the employment results in columns 7–8 reinforce the extensive-margin component of these patterns. For whites, the decline in employment is concentrated among men who grew up in counties more exposed to missing non–English-speaking and S/E immigrants, while exposure to English-speaking and N/W groups has smaller effects.[24] Overall, these findings, while not definitive, consistently point toward a mechanism in which the loss of more complementary immigrant groups reduces both employment and earnings prospects for white workers, while easing labor-market competition for Black workers.

**Migration.** One may wonder whether these heterogeneous effects partly reflect different migration responses at the county level to the loss of S/E or non-English-speaking immigrants. To examine this possibility, Tables A10 and A11 repeat the population-growth regressions from Section 6.1 separately for exposure to missing English versus non-English speakers (Table A10) and missing N/W versus S/E Europeans (Table A11). In both tables, the migration responses to the different immigrant groups are statistically indistinguishable from each other. The one consistent and economically meaningful exception appears in column 7: Black population growth is positive and statistically significant in counties more exposed to missing non-English-speaking or S/E immigrants. This pattern is consistent with Collins (1997), who argues that

[24] For Black men, the coefficients on exposure to missing non–English-speaking and Northern and Western European immigrants are negative and statistically significant, whereas those for English-speaking and Southern and Eastern European groups are positive but imprecisely estimated.

Southern and Eastern European immigrants deterred Black migration to northern labor markets by competing for similar jobs. The quotas thus created modest openings for Black workers in certain counties, a pattern that resonates with the mobility estimates we report above.

Beyond this response of Black migration, however, we see no systematic evidence that differential internal or international inflows across immigrant groups can account for the larger negative effects for whites. This reinforces the interpretation that the complementarity structure of the local labor market—rather than compositional shifts in county population—is the primary mechanism behind the heterogeneous effects of the quotas.

### 6.3 Human Capital Investment

The effects of immigration restrictions may reflect changes in productivity on the job, but they may also arise because US-born white individuals adjusted their educational investment in response to declining immigration. If immigrants raise the returns to skill for US-born whites—for example, by complementing educated native workers or by shifting task structures in ways that reward schooling—then restricting immigration could weaken incentives to acquire education. To assess this channel, we examine educational attainment in 1940 for the same cohort of US-born men. As with the other 1940 outcomes, these regressions rely on a single cross-section, since years of schooling are first reported in 1940. Results are presented in Table 5, with columns 1–2 reporting estimates for white men and columns 3–4 for Black men.

For white men, the coefficient on quota exposure in column 1 is negative and statistically significant: individuals in more exposed counties completed slightly fewer years of schooling. The magnitude, however, is modest—a 5 percentage point increase in exposure reduces schooling by roughly 0.16 years, or about 1.5% of the sample mean. For comparison, cohorts born twenty years later completed roughly two additional years of schooling (Goldin and Katz, 2008), implying that quota exposure offset only about 8% of this long-run educational gain—far less than the share of wage growth it explains. Column 2 turns to the probability of completing high school. The coefficient is again negative and statistically significant, but quantitatively small: a 5 percentage point increase in exposure lowers the probability of high-school completion by 1.2 percentage points (3% of the mean). Taken together, these findings indicate that the quotas reduced educational investment among whites, either by weakening incentives to use schooling as a means of escaping immigrant competition or by lowering the returns to education previously enhanced by immigrant–native complementarities.

No matter the mechanism linking quota exposure to educational attainment, the implied magnitudes are small and unlikely to account for the main mobility and wage effects we document. To more formally assess the contribution of schooling to the overall effects of the quotas, we implement a simple accounting exercise. We conduct a back-of-the-envelope calculation using estimates of the causal Mincerian return to education from the literature for this historical period. Specifically, we draw on estimates from Feigenbaum and Tan (2020) and Clay et al. (2021), who report returns to schooling in the range of 3.5–5.5 and 6.4–7.9 percent per additional year of education.[25] We then multiply these returns by our estimated effect of quota exposure on educational attainment, and compare the implied wage change to the total effect of quota exposure on weekly wages. Formally, we compute

$$\frac{\widehat{\beta}_{\text{school}\rightarrow\text{wage}} \times \widehat{\beta}_{\text{quota}\rightarrow\text{school}}}{\widehat{\beta}_{\text{quota}\rightarrow\text{wage}}},$$

where $\widehat{\beta}_{\text{school}\rightarrow\text{wage}}$ denotes the Mincerian return to schooling (the percent increase in wages per additional year of education) from the literature, $\widehat{\beta}_{\text{quota}\rightarrow\text{school}}$ is our estimated effect of quota exposure on educational attainment (Table 5, column 1), and $\widehat{\beta}_{\text{quota}\rightarrow\text{wage}}$ is the effect of quota exposure on weekly wages (Table 3, column 1).

Using the weekly wage and education effects of approximately $-2.6\%$ and 0.16 years for a 5 percentage point increase in exposure, and substituting in the previous expression the values from the literature (Feigenbaum and Tan, 2020; Clay et al., 2021) yields estimates between 0.22 and 0.49. Thus, under the assumption that the full Mincer return is causal and that no other channels operate, reduced schooling can account for between 22 and 49 percent of the weekly wage decline. While this is a non-trivial share, it suggests that adjustments in human capital investment are unlikely to be the main drivers of the overall effects of the quotas.

For Black men, the effect on years of schooling (column 3) is larger in magnitude but statistically insignificant, possibly reflecting higher opportunity costs of remaining in school in counties where the quotas expanded occupational opportunities for

[25] Feigenbaum and Tan (2020) compare twin brothers in a linked sample, while Clay et al. (2021) exploit variation in compulsory schooling laws and child labor laws across states and cohorts.

Black workers. This interpretation is consistent with recent evidence that educational choices respond to local labor-market conditions (Cascio and Narayan, 2022; Carlana and Tabellini, 2025). The coefficient for high-school completion (column 4) is instead positive, very small, and again imprecisely estimated.

# 7 Conclusion

This paper studies the effects of immigration restrictions on the intergenerational mobility of US-born Americans. We focus on the national origin quotas introduced by the 1921 and 1924 Immigration Acts, which marked a turning point in US immigration policy and drastically curtailed inflows from Southern and Eastern Europe. We find that immigration restrictions reduced upward occupational mobility among US-born white men. In contrast, we find modest, though imprecisely estimated, increases in upward mobility for Black men.

Our findings underscore the long-run consequences of immigration policy for native-born populations and highlight its distributional implications across racial groups. While much of the contemporary debate focuses on the short-run labor market effects of immigration, our results suggest that restricting inflows can have persistent, negative consequences for native-born workers—particularly when those workers are occupationally complementary to immigrants. Yet, these restrictions may also have expanded opportunities for historically marginalized groups who competed more directly with immigrants.

Several open questions remain. While our findings point to productivity declines and occupational downgrading for whites as key channels, more work is needed to unpack the precise mechanisms—for example, by exploring firm-level reallocation, task-based shifts, or changes in local labor demand. In addition, our analysis focuses on men, reflecting data limitations in historical census linkage. Understanding how immigration policy affected US-born women's economic trajectories—and how those interacted with household dynamics—remains an important direction for future research. Finally, it would be fruitful to compare our findings to the impacts of more recent immigration policy shocks in other countries.

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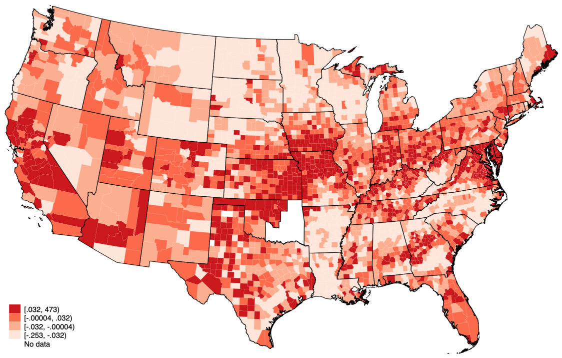
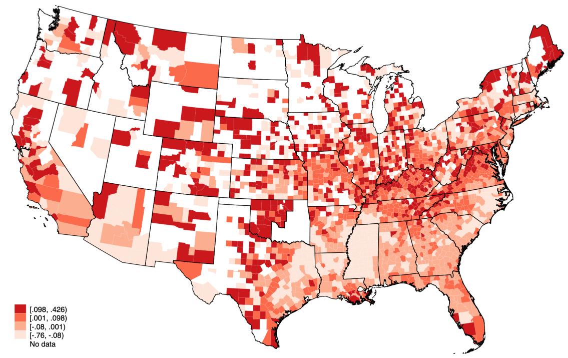
# Figures and Tables



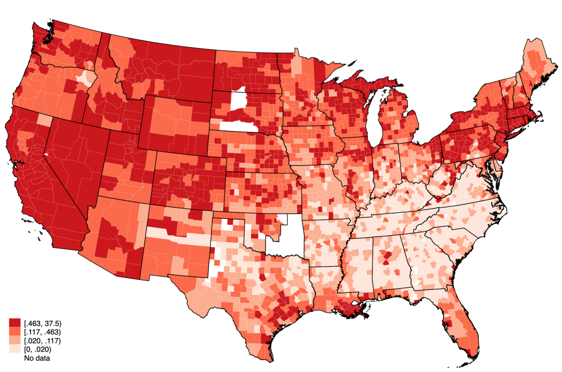

(a) Panel A: US-Born White Men



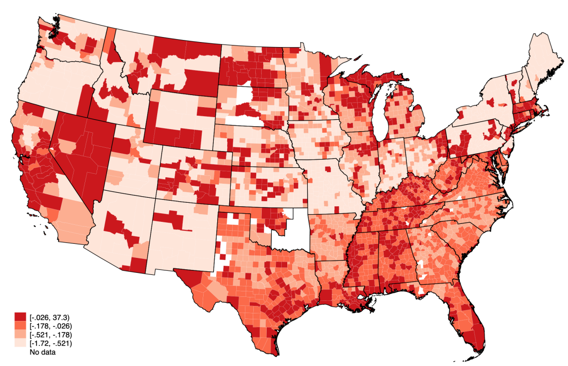

(b) Panel B: US-Born Black Men

Figure 1. Intergenerational Mobility Rates (1920-1940)

Notes: The figure plots absolute intergenerational mobility for the cohorts of US-born white (Panel A) and Black (Panel B) men who were children in 1920 and adults in 1940, after partialling out Census region fixed effects. Intergenerational mobility is defined as the probability that the son has a higher occupational rank than the father. See Section 3 for more details.




(a) Panel A: Raw Data




(b) Panel B: Partialling Out Census Region Fixed Effects

Figure 2. Immigration Quota Exposure

Notes: Panel A plots the immigration quota exposure, defined as the number of predicted missing immigrants, scaled by 1900 county population, while Panel B presents the same measure after partialling out Census region fixed effects (see Section 4.1 for more details).

Table 1. Quota Exposure and Natives' Intergenerational Mobility

| Dep. Var | **1**(Higher Rank) | | | | | Diff. Rank |
|---|---|---|---|---|---|---|
| | Full | Urban | Non-South | Pre-trends | Quota by Industry | Full |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: US-Born White Men | | | | | | |
| Quota Exposure x Post | -0.0038*** | -0.0032*** | -0.0023*** | 0.0006 | -0.0062*** | -0.0818** |
| | (0.0009) | (0.0008) | (0.0008) | (0.0010) | (0.0003) | (0.0375) |
| R-squared | 0.064 | 0.055 | 0.056 | 0.066 | 0.104 | 0.077 |
| Observations | 10,541,094 | 6,779,453 | 6,941,397 | 6,305,539 | 10,541,094 | 10,541,094 |
| Dep. Var. Mean | 0.513 | 0.495 | 0.481 | 0.478 | 0.513 | 1.650 |
| Dep. Var. SD | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 | 23.416 |
| Quota Exposure Mean | 0.803 | 1.178 | 1.184 | 0.753 | 3.098 | 0.803 |
| Quota Exposure SD | 2.078 | 2.431 | 2.413 | 2.087 | 6.543 | 2.078 |
| Panel B: US-Born Black Men | | | | | | |
| Quota Exposure x Post | 0.0022 | 0.0038 | 0.0021 | -0.0038 | -0.0005 | -0.0185 |
| | (0.0044) | (0.0054) | (0.0047) | (0.0034) | (0.0004) | (0.1438) |
| R-squared | 0.048 | 0.046 | 0.082 | 0.089 | 0.076 | 0.035 |
| Observations | 913,447 | 451,371 | 108,617 | 564,034 | 913,446 | 913,447 |
| Dep. Var. Mean | 0.503 | 0.539 | 0.724 | 0.421 | 0.503 | 6.128 |
| Dep. Var. SD | 0.500 | 0.498 | 0.447 | 0.494 | 0.500 | 17.272 |
| Quota Exposure Mean | 0.211 | 0.365 | 1.086 | 0.174 | 2.042 | 0.211 |
| Quota Exposure SD | 0.777 | 0.977 | 1.891 | 0.864 | 5.034 | 0.777 |

*Notes:* The table reports estimates from Equation 4. In columns 1–5, the dependent variable is an indicator equal to one if the son has a higher occupational rank than his father; in column 6, it is the difference between the son's and father's occupational ranks. Both outcomes are based on the nativity-adjusted Song scores (see Section 3). Columns 1 and 6 use the full sample, while columns 2 and 3 restrict attention to counties with a positive urban population share and to non-Southern counties, respectively. Column 4 presents a pre-trend test, estimating the relationship between quota exposure and intergenerational mobility in the pre-1920 period. Column 5 replaces county-level exposure with an industry-based measure, constructed from the 1900 distribution of immigrants across industries. All regressions control for individuals' age, county fixed effects, Census region–by–period fixed effects, and the interactions of the *Post* indicator with the log of 1900 county population and foreign-born population. Standard errors are clustered at the county level. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2. Heterogeneous Effects of Quota Exposure, by Father's Rank

| | Baseline | $\mathbb{1}$(Migrated) | Migrated to | | | |
|---|---|---|---|---|---|---|
| | | | Higher-Scores | Lower-Scores | Higher-IGM | Lower-IGM |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: US-Born White Men | | | | | | |
| Quota Exposure x Post | -0.0067*** | 0.0004 | -0.0013 | 0.0016 | -0.0011 | -0.0005 |
| | (0.0023) | (0.0011) | (0.0017) | (0.0014) | (0.0014) | (0.0010) |
| Above Median Father's Rank x Quota x Post | 0.0044* | 0.0021** | 0.0026** | -0.0007 | 0.0053*** | -0.0017 |
| | (0.0026) | (0.0010) | (0.0013) | (0.0012) | (0.0020) | (0.0014) |
| R-squared | 0.147 | 0.087 | 0.116 | 0.077 | 0.114 | 0.079 |
| Observations | 10,541,091 | 10,541,091 | 10,092,903 | 10,092,903 | 8,423,497 | 8,423,497 |
| Dep. Var. Mean | 0.513 | 0.496 | 0.298 | 0.173 | 0.242 | 0.156 |
| Dep. Var. SD | 0.500 | 0.500 | 0.457 | 0.378 | 0.428 | 0.363 |
| Quota Exposure Mean | 0.803 | 0.803 | 0.834 | 0.834 | 1.027 | 1.027 |
| Quota Exposure SD | 2.078 | 2.078 | 2.119 | 2.119 | 2.397 | 2.397 |
| Panel B: US-Born Black Men | | | | | | |
| Quota Exposure x Post | 0.0421 | 0.0380 | 0.0327** | -0.0037 | 0.0132* | -0.0030 |
| | (0.0260) | (0.0245) | (0.0155) | (0.0050) | (0.0078) | (0.0056) |
| Above Median Father's Rank x Quota x Post | -0.0482* | -0.0318 | -0.0284** | 0.0054 | -0.0094 | 0.0053 |
| | (0.0269) | (0.0218) | (0.0137) | (0.0055) | (0.0079) | (0.0057) |
| R-squared | 0.082 | 0.096 | 0.102 | 0.069 | 0.091 | 0.067 |
| Observations | 913,414 | 913,414 | 842,585 | 842,585 | 799,443 | 799,443 |
| Dep. Var. Mean | 0.503 | 0.585 | 0.393 | 0.150 | 0.365 | 0.151 |
| Dep. Var. SD | 0.500 | 0.493 | 0.488 | 0.357 | 0.481 | 0.358 |
| Quota Exposure Mean | 0.211 | 0.211 | 0.212 | 0.212 | 0.204 | 0.204 |
| Quota Exposure SD | 0.777 | 0.777 | 0.799 | 0.799 | 0.823 | 0.823 |

*Notes:* The table reports heterogeneous effects of quota exposure by father's occupational rank. We extend the baseline specification (Table 1, column 1) by interacting quota exposure × post-quota dummy with an indicator for fathers whose Song score is above the sample median, fully saturating the regression with all lower-order interactions. The above-median indicator is also interacted with county fixed effects. The dependent variable is an indicator equal to one if the son has a higher occupational rank than his father (column 1) or if the son resides in a different county in adulthood than in childhood (column 2). In columns 3–6, the dependent variable indicates whether the son moved to a county with higher or lower average occupational scores or intergenerational mobility, measured in the childhood period. All regressions also include age dummies, county fixed effects, Census region–by–period fixed effects, and the interactions of *Post* with the log of 1900 county population and foreign-born population. Standard errors are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3. Quota Exposure, Wages, and Education: Evidence from the 1940 Census

| | Log Weekly Wage | | | Log Hourly Wage | | | Employed |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: US-Born White Men | | | | | | | |
| Quota Exposure | -0.0051*** | -0.0053*** | -0.0042*** | -0.0044* | -0.0054*** | -0.0038** | -0.0026*** |
| | (0.0020) | (0.0015) | (0.0013) | (0.0025) | (0.0020) | (0.0017) | (0.0008) |
| R-squared | 0.163 | 0.340 | 0.392 | 0.159 | 0.342 | 0.378 | 0.008 |
| Observations | 4,959,921 | 4,959,920 | 4,959,921 | 4,149,843 | 4,149,842 | 4,149,843 | 6,400,237 |
| Dep. Var. Mean | 2.942 | 2.942 | 2.942 | 2.979 | 2.979 | 2.979 | 0.935 |
| Dep. Var. SD | 0.723 | 0.723 | 0.723 | 0.882 | 0.882 | 0.882 | 0.246 |
| Quota Exposure Mean | 1.038 | 1.038 | 1.038 | 1.042 | 1.042 | 1.042 | 0.959 |
| Quota Exposure SD | 2.160 | 2.160 | 2.160 | 2.135 | 2.135 | 2.135 | 2.116 |
| Panel B: US-Born Black Men | | | | | | | |
| Quota Exposure | 0.0042 | 0.0022 | 0.0004 | 0.0087 | 0.0044 | 0.0019 | 0.0012 |
| | (0.0048) | (0.0033) | (0.0032) | (0.0065) | (0.0041) | (0.0041) | (0.0016) |
| R-squared | 0.102 | 0.355 | 0.381 | 0.096 | 0.341 | 0.358 | 0.016 |
| Observations | 391,331 | 391,331 | 391,330 | 318,269 | 318,269 | 318,268 | 528,176 |
| Dep. Var. Mean | 2.282 | 2.282 | 2.282 | 2.239 | 2.239 | 2.239 | 0.918 |
| Dep. Var. SD | 0.765 | 0.765 | 0.765 | 0.918 | 0.918 | 0.918 | 0.274 |
| Quota Exposure Mean | 0.370 | 0.370 | 0.370 | 0.350 | 0.350 | 0.350 | 0.327 |
| Quota Exposure SD | 1.052 | 1.052 | 1.052 | 1.015 | 1.015 | 1.015 | 0.989 |
| 3-Digit Industry FE | | Y | | | Y | | |
| 3-Digit Occupation FE | | | Y | | | Y | |

*Notes:* The table reports the association between quota exposure and 1940 outcomes for the 1920 cohort. The dependent variable is the log of weekly wage in columns 1–3, the log of the hourly wage in columns 4–6, and a dummy for being employed in column 7. All regressions include age dummies, Census region fixed effects, the log of 1900 county population, and the log of 1900 foreign-born population. Columns 2 and 5 (respectively, columns 3 and 6) add three-digit industry (respectively, occupation) fixed effects. Standard errors are clustered at the county level. Significance levels: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

## Table 4. Quota Exposure by type, US-born White Men

| **Cohorts** | **1920-1900** | | **1920** | | | | | |
|---|---|---|---|---|---|---|---|---|
| Dep. Variable | **1**(Higher Rank) | | Log Weekly Wage | | Log Hourly Wage | | Employed | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Quota Exposure (x Post), English Speakers | -0.0045*** | | 0.0071 | | 0.0117* | | -0.0030** | |
| | (0.0016) | | (0.0053) | | (0.0069) | | (0.0012) | |
| Quota Exposure (x Post), Non-English Speakers | -0.0066*** | | -0.0124*** | | -0.0121** | | -0.0045*** | |
| | (0.0018) | | (0.0043) | | (0.0055) | | (0.0017) | |
| Quota Exposure (x Post), from N/W | | -0.0043*** | | 0.0080* | | 0.0138** | | -0.0026** |
| | | (0.0016) | | (0.0046) | | (0.0061) | | (0.0012) |
| Quota Exposure (x Post), from S/E | | -0.0066*** | | -0.0131*** | | -0.0133** | | -0.0046*** |
| | | (0.0018) | | (0.0042) | | (0.0055) | | (0.0017) |
| R-squared | 0.064 | 0.064 | 0.163 | 0.163 | 0.159 | 0.159 | 0.008 | 0.008 |
| Observations | 10,528,319 | 10,528,319 | 4,955,634 | 4,955,634 | 4,146,338 | 4,146,338 | 6,394,108 | 6394108 |
| Dep. Var. Mean | 0.513 | 0.513 | 2.942 | 2.942 | 2.979 | 2.979 | 0.935 | 0.935 |
| Dep. Var. SD | 0.500 | 0.500 | 0.723 | 0.723 | 0.882 | 0.882 | 0.246 | 0.246 |

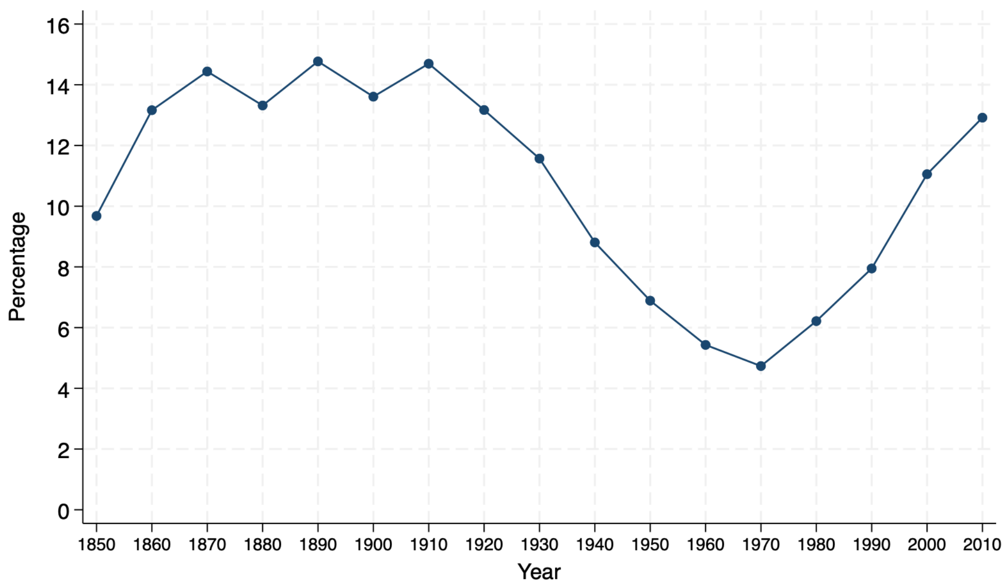
*Notes:* The table reports estimates from constructing alternative measures of the quota exposure, restricting to US-born white men. Odd columns report the results when dividing countries of origin according to the share of immigrants from that country reporting above- or below-median English proficiency. Even columns report the results when dividing immigrants into Southern/Eastern Europeans (S/E) and Northern/Western Europeans (N/W). The dependent variable is an indicator equal to one if the son has a higher occupational rank than his father in columns 1 and 2; the log of the weekly wage (1940) in columns 3 and 4; the log of the hourly wage (1940) in columns 5 and 6, and dummy for being employed (1940) in columns 7 and 8. All regressions control for individual age, county fixed effects, Census region–by–period fixed effects, and the interactions of *Post* with the log of 1900 county population and foreign-born population. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

## Table 5. Education

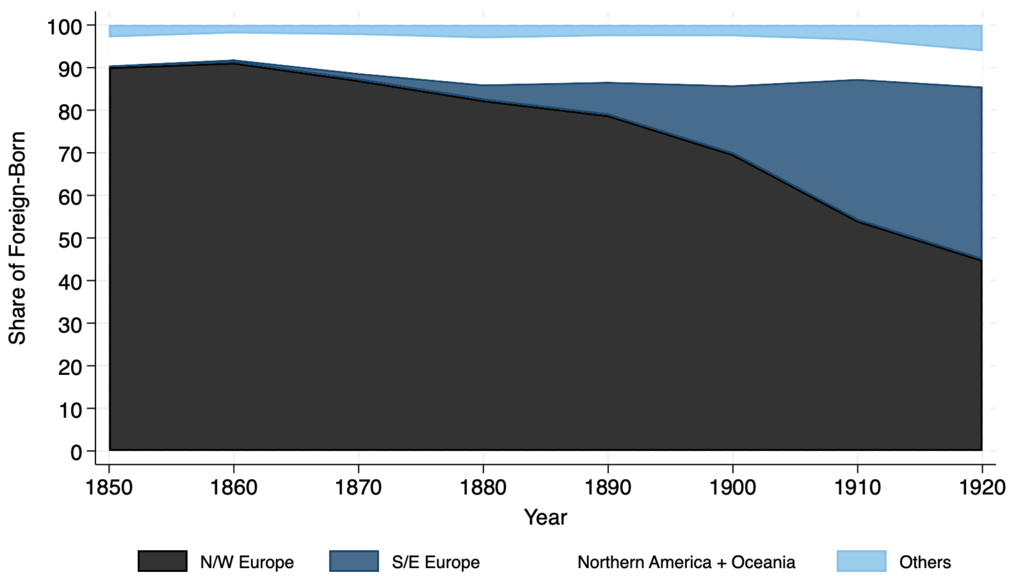
| Sample | **US-born White Men** | | **US-born Black Men** | |
|---|---|---|---|---|
| Dep. Variable | Education | **1**(High School) | Education | **1**(High School) |
| | (1) | (2) | (3) | (4) |
| Quota Exposure | -0.0325*** | -0.0024** | -0.0604 | 0.0007 |
| | (0.0083) | (0.0012) | (0.0382) | (0.0027) |
| R-squared | 0.094 | 0.053 | 0.129 | 0.056 |
| Observations | 6,392,372 | 6,365,105 | 524,976 | 503,346 |
| Dep. Var. Mean | 10.020 | 0.393 | 6.410 | 0.115 |
| Dep. Var. SD | 3.125 | 0.488 | 3.514 | 0.319 |

*Notes:* The table reports the association between quota exposure and educational outcomes reported in the 1940 Census for the 1920 cohort. The dependent variable is years of education in odd columns and a dummy for having completed high school in even columns. All regressions control for individual age, the Census region fixed effects, and the log of county total and foreign-born population in 1900. We cluster the standard errors at the county level. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

# Appendix



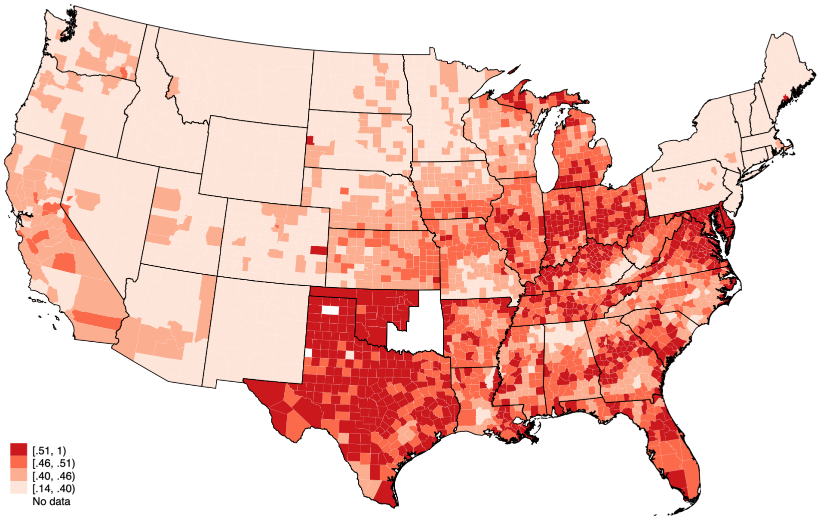

(a) Panel A: Immigrant Population Share



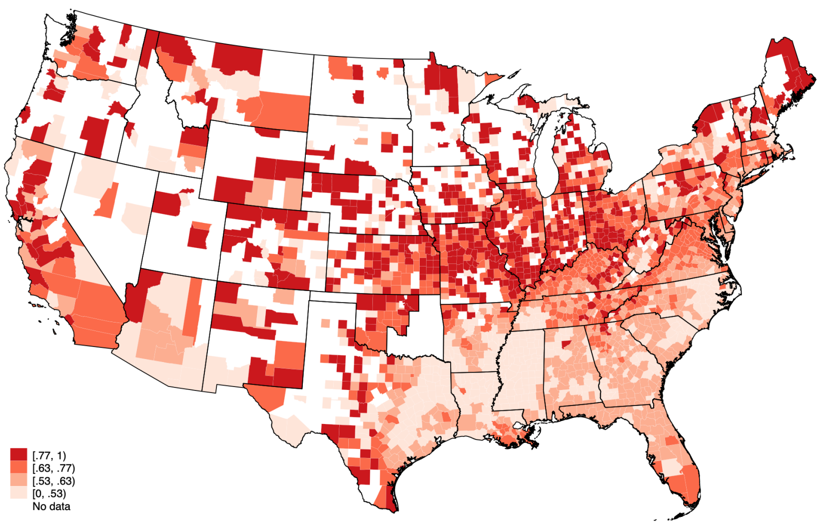

(b) Panel B: Immigrants by Origin

Figure A1. US Immigration: Population Share and Origin Regions

Notes: Panel A plots the share of the foreign-born population relative to the US population between 1850 and 2010, while Panel B presents the share of the foreign-born population by sending region between 1850 and 1920.



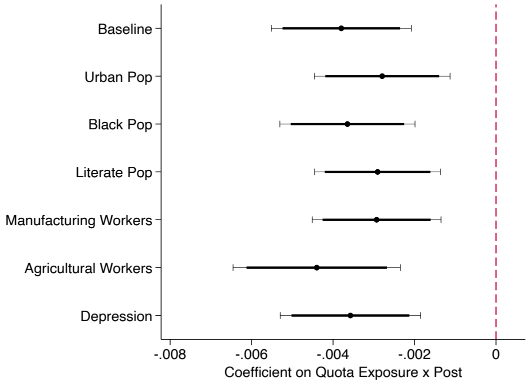

(a) Panel A: US-Born White Men



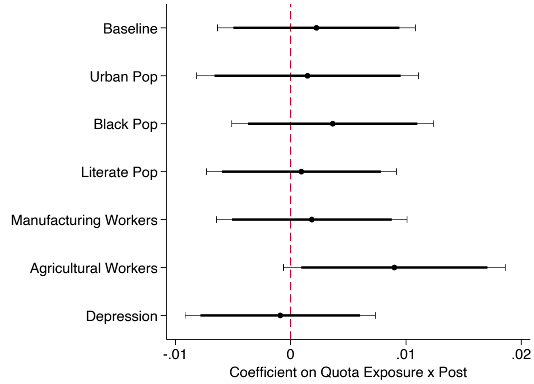

(b) Panel B: US-Born Black Men

Figure A2. Intergenerational Mobility Rates (1920-1940)

Notes: The figure plots the raw absolute intergenerational mobility for the cohorts of US-born white (Panel A) and Black (Panel B) men who were children in 1920 and adults in 1940. Intergenerational mobility is defined as the probability that the son has a higher occupational rank than the father. See Section 3 for more details.



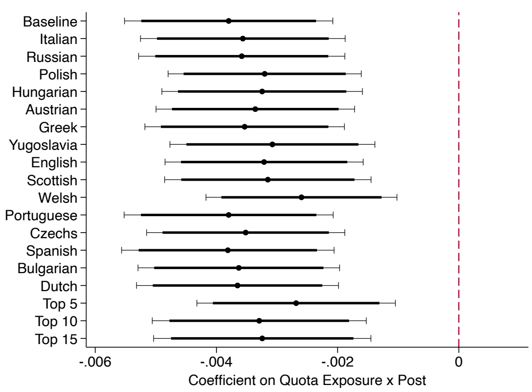

(a) Panel A: US-Born White



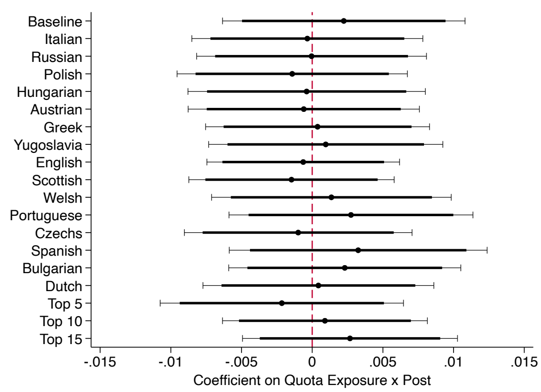

(b) Panel B: US-Born Black

Figure A3. Robustness Checks, Allowing for Differential Trends

Note: The figure reports estimates from specifications that extend the baseline model (shown as the first marker on the top) for US-born white men (Panel A) and Black men (Panel B). Subsequent specifications sequentially interact the post-quota indicator with the log of 1900 county characteristics: urban population, Black population, literate population, manufacturing employment, and agricultural employment. The final specification adds an interaction between the post-quota indicator and local Great Depression severity, measured by the change in retail sales per capita between 1929 and 1933. All regressions control for age dummies, county fixed effects, the Census region–by–period fixed effects, and the interactions of the post-quota indicator with the log of 1900 county population and foreign-born population. Markers and horizontal lines represent point estimates and 90% and 95% confidence intervals. Standard errors are clustered at the county level.



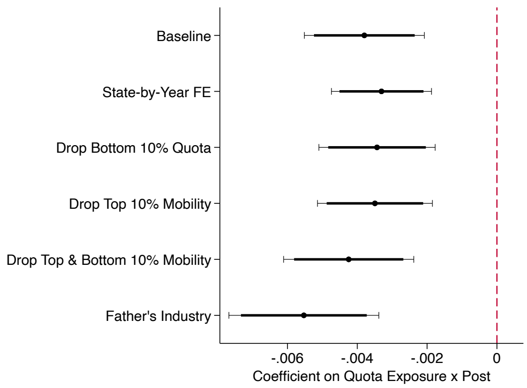

(a) Panel A: US-Born White



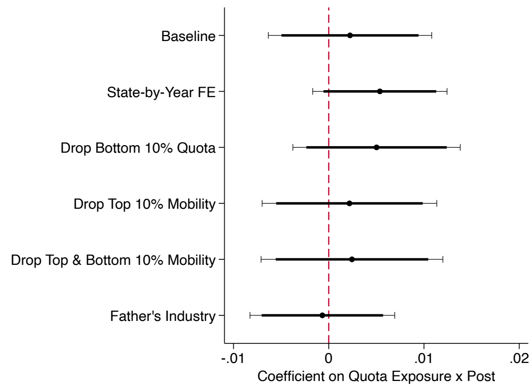

(b) Panel B: US-Born Black

Figure A4. Robustness Checks, Initial Immigrant Shares

Notes: This figure replicates the baseline results while controlling for the 1900 county shares of immigrants from each origin group. Specifically, we separately include the share of immigrants from origin country $j$ residing in county $c$ interacted with *Post* dummy. Panels A and B plot the results for the US-born white and Black males, respectively.




(a) Panel A: US-Born White




(b) Panel B: US-Born Black

Figure A5. Additional Robustness Checks

Note: The figure presents additional robustness checks for US-born white men (Panel A) and Black men (Panel B). The first marker on the top reproduces the baseline estimate. The following specifications (top to bottom) successively: (i) replace Census region–by–period with state–by–period fixed effects; (ii) drop counties in the bottom 10% and top 10% of the quota exposure distribution; (iii) exclude counties in both tails of the intergenerational mobility distribution; and (iv) include fathers' 3-digit industry dummy. All regressions include age dummies, county fixed effects, the Census region–by–period fixed effects (if not replaced by state–by–period fixed effects), and interactions of the post-quota indicator with the log of 1900 county population and foreign-born population. Markers and horizontal lines show point estimates and 90% and 95% confidence intervals, with standard errors clustered at the county level.

Table A1. Summary Statistics

| Variables | Linked Sample | | | Full-Count Census | | |
|---|---|---|---|---|---|---|
| | Mean | St. Dev. | Obs. | Mean | St. Dev. | Obs. |
| Panel A: US-Born White Men | | | | | | |
| *Childhood Outcomes* | | | | | | |
| Urban Indicator | 0.304 | 0.460 | 10,541,094 | 0.307 | 0.461 | 16,258,432 |
| Age | 8.657 | 5.524 | 10,541,094 | 8.387 | 5.643 | 16,258,432 |
| Father's Age | 39.812 | 7.866 | 10,541,094 | 39.604 | 7.967 | 16,258,432 |
| Father's Occupation Rank | 56.230 | 24.428 | 10,541,094 | 55.498 | 24.880 | 16,258,432 |
| *Adulthood Outcomes* | | | | | | |
| Urban Indicator | 0.453 | 0.498 | 10,541,094 | 0.569 | 0.495 | 27,608,892 |
| Age | 28.650 | 5.559 | 10,541,094 | 29.521 | 5.948 | 27,608,892 |
| Occupation Rank | 55.154 | 25.496 | 10,541,094 | 59.422 | 23.968 | 27,608,892 |
| 1(Higher Rank) Indicator | 0.433 | 0.495 | 10,541,094 | | | |
| Panel B: US-Born Black Men | | | | | | |
| *Childhood Outcomes* | | | | | | |
| Urban Indicator | 0.175 | 0.380 | 913,447 | 0.165 | 0.371 | 2,506,199 |
| Age | 8.400 | 5.393 | 913,447 | 8.397 | 5.514 | 2,506,199 |
| Father's Age | 39.672 | 8.562 | 913,447 | 39.636 | 8.723 | 2,506,199 |
| Father's Occupation Rank | 7.332 | 11.252 | 913,447 | 6.902 | 10.624 | 2,506,199 |
| *Adulthood Outcomes* | | | | | | |
| Urban Indicator | 0.406 | 0.491 | 913,447 | 0.518 | 0.500 | 3,236,415 |
| Age | 28.309 | 5.564 | 913,447 | 29.735 | 6.171 | 3,236,415 |
| Occupation Rank | 13.852 | 18.548 | 913,447 | 13.915 | 16.573 | 3,236,415 |
| 1(Higher Rank) Indicator | 0.545 | 0.498 | 913,447 | | | |
| Panel C: 1900 County Demographics | | | | | | |
| County Population (1,000s) | 26.947 | 73.994 | 2,810 | | | |
| Foreign-Born Population Share | 0.091 | 0.107 | 2,810 | | | |
| Urban Population Share | 0.133 | 0.216 | 2,810 | | | |
| Black Population Share | 0.131 | 0.213 | 2,810 | | | |
| Quota Exposure | 0.558 | 1.648 | 2,810 | | | |

*Notes:* The table summarizes the stacked cross-sections of the 1900 and 1920 cohorts. Each cohort links individuals' childhood (ages 0–20) and adulthood (ages 20–40) observations. Panels A and B report summary statistics for US-born white and Black men, respectively. In both panels, the first three columns correspond to the linked samples, while the next three report the same statistics from the full-count Censuses. The variable *Urban indicator* denotes whether individuals lived in urban areas. *Occupation rank* and the indicator for higher rank are both based on the nativity-adjusted Song scores (see Section 3 for more details). Panel C reports 1900 county characteristics, including the measure of quota exposure—the share of predicted missing immigrants described in Section 4.1.

## Table A2. Quota Exposure and Natives' Relative Mobility

| Dep. Var | Son's Song Rank | | | | | |
|---|---|---|---|---|---|---|
| | White Men | | | Black Men | | |
| | Full | Urban | Non-South | Full | Urban | Non-South |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Quota Exposure x Post | -0.1176 | -0.2328** | -0.2950** | -0.1150 | -0.2611 | -0.3588* |
| | (0.0809) | (0.1181) | (0.1167) | (0.1398) | (0.1792) | (0.1941) |
| Father's Rank | 0.3439*** | 0.3325*** | 0.2947*** | 0.2383*** | 0.2652*** | 0.2101*** |
| | (0.0042) | (0.0062) | (0.0055) | (0.0067) | (0.0087) | (0.0125) |
| Quota Exposure x Post x Father's Rank | 0.0005 | 0.0026 | 0.0038** | 0.0010 | 0.0094** | 0.0086* |
| | (0.0013) | (0.0018) | (0.0018) | (0.0045) | (0.0045) | (0.0049) |
| R-squared | 0.372 | 0.313 | 0.194 | 0.215 | 0.272 | 0.265 |
| Observations | 10,541,094 | 6,779,453 | 6,941,397 | 913,447 | 451,371 | 108,617 |
| Dep. Var. Mean | 55.819 | 62.285 | 66.415 | 12.375 | 15.202 | 30.623 |
| Dep. Var. SD | 25.868 | 23.679 | 19.978 | 17.642 | 19.370 | 22.333 |
| Quota Exposure Mean | 0.803 | 1.178 | 1.184 | 0.211 | 0.365 | 1.086 |
| Quota Exposure SD | 2.078 | 2.431 | 2.413 | 0.777 | 0.977 | 1.891 |

*Notes:* The table reports the effects of quota exposure on relative mobility by augmenting our baseline specification with the interaction term of fathers' rank with quota exposure and the post-quota indicator and fully-saturated lower-order interactions. The dependent variable is the son's occupational rank. Columns 1-3 show results for US-born white men, while columns 4-6 focus on US-born Black men. For each group, we examine the effect using the full sample, and then restrict our attention to urban and non-Southern counties. We cluster standard errors at the county level. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

## Table A3. Main Results: Alternative Standard Errors and Spillovers

| | Alternative clusters | | | Control Quota Exposure in other Counties | | | |
|---|---|---|---|---|---|---|---|
| | Baseline | SEA | State | Neighboring | w/in State | Neighboring | w/in State |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: US-Born White Men | | | | | | | |
| Quota Exposure x Post | -0.0038*** | -0.0038*** | -0.0038*** | -0.0036*** | -0.0034*** | -0.0037*** | -0.0037*** |
| | (0.0009) | (0.0010) | (0.0011) | (0.0009) | (0.0008) | (0.0009) | (0.0009) |
| Quota in Neighboring Counties x Post | | | | -0.0014 | -0.0139*** | -0.0012 | -0.0124 |
| | | | | (0.0009) | (0.0032) | (0.0009) | (0.0086) |
| R-squared | 0.064 | 0.064 | 0.064 | 0.064 | 0.065 | 0.064 | 0.065 |
| Observations | 10,541,094 | 10,541,094 | 10,541,094 | 10,491,197 | 10,469,990 | 10,491,197 | 10,469,990 |
| Dep. Var. Mean | 0.513 | 0.513 | 0.513 | 0.513 | 0.513 | 0.513 | 0.513 |
| Dep. Var. SD | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 |
| Quota Exposure Mean | 0.803 | 0.803 | 0.803 | 0.806 | 0.806 | 0.806 | 0.806 |
| Quota Exposure SD | 2.078 | 2.078 | 2.078 | 2.083 | 2.085 | 2.083 | 2.085 |
| Panel B: US-Born Black Men | | | | | | | |
| Quota Exposure x Post | 0.0022 | 0.0022 | 0.0022 | 0.0021 | 0.0019 | 0.0018 | 0.0014 |
| | (0.0044) | (0.0055) | (0.0060) | (0.0041) | (0.0042) | (0.0043) | (0.0042) |
| Quota in Neighboring Counties x Post | | | | 0.0011 | -0.0527*** | 0.0044 | 0.0065 |
| | | | | (0.0071) | (0.0194) | (0.0071) | (0.0358) |
| R-squared | 0.048 | 0.048 | 0.048 | 0.048 | 0.049 | 0.048 | 0.049 |
| Observations | 913,447 | 913,447 | 913,447 | 909,673 | 904,207 | 909,673 | 904,207 |
| Dep. Var. Mean | 0.503 | 0.503 | 0.503 | 0.503 | 0.502 | 0.503 | 0.502 |
| Dep. Var. SD | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 | 0.500 |
| Quota Exposure Mean | 0.211 | 0.211 | 0.211 | 0.211 | 0.210 | 0.211 | 0.210 |
| Quota Exposure SD | 0.777 | 0.777 | 0.777 | 0.778 | 0.780 | 0.778 | 0.780 |
| Cluster Standard Errors | County | SEA | State | | | | |
| Quota Weighted by County Pop | | | | | | Y | Y |

*Notes:* The table replicates the baseline specification, reported in column 1, by clustering the standard errors at the Standard Economic Area (column 2) and at the state (column 3) level, and by separately controlling for quota exposure in neighboring counties (column 4) and in other counties in the same state (column 5). In columns 6 and 7, quota exposure in other counties is constructed weighting by the populations of neighboring counties. All regressions also include age dummies, county fixed effects, the Census region–by–period fixed effects, and the interactions of post-quota indicator with the log of 1900 county population and foreign-born population. Unless otherwise specified, standard errors are clustered at the county level. Significance levels: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A4. Main Results, Alternative Linked Samples

| | **1**(Higher Rank) | | | | | Diff. Rank |
|---|---|---|---|---|---|---|
| | Full | Urban | Non-South | Pre-trends | Quota by Industry | Full |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: US-Born White Men | | | | | | |
| Quota Exposure x Post | -0.0032*** | -0.0022*** | -0.0018** | 0.0003 | -0.0043*** | -0.0820** |
| | (0.0007) | (0.0007) | (0.0007) | (0.0011) | (0.0008) | (0.0319) |
| R-squared | 0.059 | 0.053 | 0.055 | 0.064 | 0.100 | 0.074 |
| Observations | 4,504,067 | 2,990,028 | 3,174,110 | 2,535,356 | 4,504,067 | 4,504,067 |
| Dep. Var. Mean | 0.505 | 0.489 | 0.481 | 0.471 | 0.505 | 1.143 |
| Dep. Var. SD | 0.500 | 0.500 | 0.500 | 0.499 | 0.500 | 24.065 |
| Quota Exposure Mean | 0.841 | 1.182 | 1.155 | 0.788 | 0.841 | 0.841 |
| Quota Exposure SD | 2.098 | 2.401 | 2.348 | 2.104 | 2.098 | 2.098 |
| Panel B: US-Born Black Men | | | | | | |
| Quota Exposure x Post | 0.0034 | 0.0037 | 0.0019 | -0.0022 | -0.0054 | 0.0488 |
| | (0.0045) | (0.0057) | (0.0056) | (0.0030) | (0.0038) | (0.1766) |
| R-squared | 0.043 | 0.042 | 0.086 | 0.079 | 0.073 | 0.039 |
| Observations | 335,708 | 169,928 | 46,416 | 213,536 | 335,704 | 335,708 |
| Dep. Var. Mean | 0.607 | 0.634 | 0.760 | 0.517 | 0.607 | 11.477 |
| Dep. Var. SD | 0.488 | 0.482 | 0.427 | 0.500 | 0.488 | 21.925 |
| Quota Exposure Mean | 0.221 | 0.386 | 1.090 | 0.179 | 0.221 | 0.221 |
| Quota Exposure SD | 0.838 | 1.061 | 1.963 | 0.884 | 0.838 | 0.838 |

*Notes:* The table replicates Table 1 using the alternative linked sample from Abramitzky et al. (2022). In columns 1–5, the dependent variable is an indicator equal to one if the son has a higher occupational rank than his father; in column 6, it is the difference between the son's and father's occupational ranks. Both outcomes are based on the nativity-adjusted Song scores (see Section 3). Columns 1 and 6 use the full sample, while columns 2 and 3 restrict attention to counties with a positive urban population share and to non-Southern counties, respectively. Column 4 presents a pre-trend test, estimating the relationship between quota exposure and intergenerational mobility in the pre-1920 period. Column 5 replaces county-level exposure with an industry-based measure, constructed from the 1900 distribution of immigrants across industries. All regressions control for age dummies, county fixed effects, Census region–by–period fixed effects, and the interactions of the *Post* indicator with the log of 1900 county population and foreign-born population. Standard errors are clustered at the county level. Significance levels: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A5. Summary Statistics, Alternative Linked Sample

| Variables | Census Tree | | | Census Linking Project | | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | Obs. | Mean | S.D. | Obs. |
| Panel A: US-Born White Men | | | | | | |
| *Childhood Outcomes* | | | | | | |
| Urban Indicator | 0.304 | 0.460 | 10,541,094 | 0.322 | 0.467 | 4,504,067 |
| Age | 8.657 | 5.524 | 10,541,094 | 8.649 | 5.505 | 4,504,067 |
| Father's Age | 39.812 | 7.866 | 10,541,094 | 39.817 | 7.846 | 4,504,067 |
| Father's Occupation Rank | 56.230 | 24.428 | 10,541,094 | 58.377 | 23.918 | 4,504,067 |
| *Adulthood Outcomes* | | | | | | |
| Urban Indicator | 0.453 | 0.498 | 10,541,094 | 0.476 | 0.499 | 4,504,067 |
| Age | 28.650 | 5.559 | 10,541,094 | 28.663 | 5.526 | 4,504,067 |
| Occupation Rank | 55.154 | 25.496 | 10,541,094 | 56.792 | 25.294 | 4,504,067 |
| 1(Higher Rank) Indicator | 0.433 | 0.495 | 10,541,094 | 0.429 | 0.495 | 4,504,067 |
| Panel B: US-Born Black Men | | | | | | |
| *Childhood Outcomes* | | | | | | |
| Urban Indicator | 0.175 | 0.380 | 913,447 | 0.191 | 0.393 | 335,708 |
| Age | 8.400 | 5.393 | 913,447 | 8.439 | 5.447 | 335,708 |
| Father's Age | 39.672 | 8.562 | 913,447 | 39.750 | 8.560 | 335,708 |
| Father's Occupation Rank | 7.332 | 11.252 | 913,447 | 7.850 | 11.918 | 335,708 |
| *Adulthood Outcomes* | | | | | | |
| Urban Indicator | 0.406 | 0.491 | 913,447 | 0.434 | 0.496 | 335,708 |
| Age | 28.309 | 5.564 | 913,447 | 28.417 | 5.525 | 335,708 |
| Occupation Rank | 13.852 | 18.548 | 913,447 | 19.087 | 22.460 | 335,708 |
| 1(Higher Rank) Indicator | 0.545 | 0.498 | 913,447 | 0.630 | 0.483 | 335,708 |

*Notes:* The table summarizes the stacked cross-sections of the 1900 and 1920 cohorts. Each cohort links individuals' childhood (ages 0–20) and adulthood (ages 20–40) observations. Panels A and B report summary statistics for US-born white and Black men, respectively. In both panels, the first three columns correspond to the linked samples from the Census Tree, while the next three report the same statistics from the linked sample from Abramitzky et al. (2022). The variable *Urban indicator* denotes whether individuals lived in urban areas. *Occupation rank* and the indicator for higher rank are both based on the nativity-adjusted Song scores (see Section 3 for more details).

Table A6. Quota Exposure and Intergenerational Mobility, Stayers and Movers

| | US-Born Whites | | | US-Born Blacks | | |
|---|---|---|---|---|---|---|
| | Baseline | Stayers | Movers | Baseline | Stayers | Movers |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Quota Exposure x Post | -0.0038*** | -0.0067*** | -0.0010* | 0.0022 | -0.0023 | 0.0037 |
| | (0.0009) | (0.0013) | (0.0006) | (0.0044) | (0.0067) | (0.0031) |
| R-squared | 0.064 | 0.078 | 0.061 | 0.048 | 0.095 | 0.067 |
| Observations | 10,541,094 | 5,541,118 | 4,999,976 | 913,447 | 404,588 | 508,704 |
| Dep. Var. Mean | 0.513 | 0.474 | 0.552 | 0.503 | 0.347 | 0.613 |
| Dep. Var. SD | 0.500 | 0.499 | 0.497 | 0.500 | 0.476 | 0.487 |
| Quota Exposure Mean | 0.803 | 0.954 | 0.650 | 0.211 | 0.248 | 0.184 |
| Quota Exposure SD | 2.078 | 2.250 | 1.875 | 0.777 | 0.881 | 0.692 |

*Notes:* This table reports estimates from Equation 4. The first three columns focus on the US-born white men. Column 1 replicates the baseline; columns 2 and 3 present the estimates for stayers and movers, respectively. Columns 4-6 present the results for US-born Black men. All regressions control for individuals' age, county fixed effects, the Census region–by–period fixed effects, and the interactions of the post-quota indicator with the log of 1900 county population and foreign-born population. Standard errors are clustered at the county level. Significance levels: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

## Table A7. Quota Exposure and Population Growth

| | Restricted | Full | Unrestricted | Foreign-Born | US-Born | | |
|---|---|---|---|---|---|---|---|
| | | | | | All | White | Black |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Full Sample | | | | | | | |
| Quota Exposure x Post | -0.0217*** | -0.0292*** | -0.0075 | -0.0214*** | -0.0078 | -0.0081 | 0.0004 |
| | (0.0066) | (0.0111) | (0.0077) | (0.0065) | (0.0078) | (0.0076) | (0.0007) |
| R-squared | 0.849 | 0.938 | 0.946 | 0.849 | 0.946 | 0.943 | 0.924 |
| Observations | 5,482 | 5,482 | 5,482 | 5,482 | 5,482 | 5,482 | 5,482 |
| Dep. Var. Mean | 0.033 | 0.462 | 0.428 | 0.034 | 0.428 | 0.413 | 0.014 |
| Dep. Var. SD | 0.280 | 3.099 | 2.905 | 0.281 | 2.905 | 2.772 | 0.159 |
| Quota Exposure Mean | 0.566 | 0.566 | 0.566 | 0.566 | 0.566 | 0.566 | 0.566 |
| Quota Exposure SD | 1.666 | 1.666 | 1.666 | 1.666 | 1.666 | 1.666 | 1.666 |
| Panel B: Urban Sample | | | | | | | |
| Quota Exposure x Post | -0.0152*** | -0.0136 | 0.0016 | -0.0149*** | 0.0013 | 0.0009 | 0.0005 |
| | (0.0058) | (0.0092) | (0.0082) | (0.0057) | (0.0083) | (0.0078) | (0.0008) |
| R-squared | 0.774 | 0.915 | 0.916 | 0.774 | 0.916 | 0.918 | 0.790 |
| Observations | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 |
| Dep. Var. Mean | 0.025 | 0.299 | 0.274 | 0.025 | 0.274 | 0.258 | 0.016 |
| Dep. Var. SD | 0.173 | 0.583 | 0.459 | 0.173 | 0.459 | 0.407 | 0.104 |
| Quota Exposure Mean | 0.743 | 0.743 | 0.743 | 0.743 | 0.743 | 0.743 | 0.743 |
| Quota Exposure SD | 1.729 | 1.729 | 1.729 | 1.729 | 1.729 | 1.729 | 1.729 |

*Notes:* This table reports results from county-level panel regressions for 1900, 1920, and 1940 that examine the relationship between quota exposure and population growth across different demographic groups. The dependent variable is the change in a group's county population between censuses, scaled by that group's baseline population in the preceding period. All specifications include county fixed effects and Census region–by–period fixed effects and are weighted by the number of linked individuals. Column 1 focuses on the foreign-born population from quota-restricted countries. Columns 2 and 3 examine growth in the total county population and in the unrestricted population (i.e., US-born individuals and immigrants not targeted by the quotas). The remaining columns report results separately for foreign-born residents (column 4) and for US-born residents overall and by race (columns 5–7). Panels A and B present estimates for all counties and for urban counties (defined as those with a strictly positive urban population share in 1900), respectively. Standard errors are clustered at the county level. Significance levels: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A8. Quota Exposure and Population Growth, Other Groups

| | Mexican | Canadian | Unrestricted European | Southern Blacks |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Panel A: Full Sample | | | | |
| Quota Exposure x Post | 0.0006** | 0.0005** | 0.0000 | 0.0002 |
| | (0.0003) | (0.0002) | (0.0013) | (0.0005) |
| R-squared | 0.922 | 0.848 | 0.837 | 0.840 |
| Observations | 5,180 | 5,180 | 5,180 | 1,692 |
| Dep. Var. Mean | 0.014 | -0.003 | -0.015 | 0.007 |
| Dep. Var. SD | 0.098 | 0.032 | 0.137 | 0.061 |
| Quota Exposure Mean | 0.598 | 0.598 | 0.598 | 0.931 |
| Quota Exposure SD | 1.708 | 1.708 | 1.708 | 1.948 |
| Panel B: Urban Sample | | | | |
| Quota Exposure x Post | 0.0005** | 0.0006** | 0.0023** | 0.0001 |
| | (0.0003) | (0.0003) | (0.0011) | (0.0005) |
| R-squared | 0.542 | 0.863 | 0.803 | 0.894 |
| Observations | 2,168 | 2,168 | 2,168 | 1,244 |
| Dep. Var. Mean | 0.009 | -0.004 | -0.031 | 0.006 |
| Dep. Var. SD | 0.052 | 0.021 | 0.064 | 0.029 |
| Quota Exposure Mean | 0.745 | 0.745 | 0.745 | 1.048 |
| Quota Exposure SD | 1.731 | 1.731 | 1.731 | 2.169 |

*Notes:* This table reports results from county-level panel regressions for 1900, 1920, and 1940 that examine the relationship between quota exposure and population growth across specific demographic groups. The dependent variable is the change in a group's county population between censuses, scaled by that group's baseline population in the preceding period. All specifications include county fixed effects and Census region–by–period fixed effects and are weighted by the number of linked individuals. Columns 1 and 2 focus on the Mexican and Canadian populations. Column 3 examines growth in the European immigrants not targeted by the quotas. Column 4 reports the result for Southern Black Americans. Panels A and B present estimates for all counties and for urban counties (defined as those with a strictly positive urban population share in 1900), respectively. For the Southern Black Americans (column 4), we always restrict the sample to non-Southern counties. Standard errors are clustered at the county level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

## Table A9. Quota Exposure by Type, US-Born Black Men

| Cohorts | 1920-1900 | | 1920 | | | | | |
|---|---|---|---|---|---|---|---|---|
| Dep. Variable | **1**(Higher Rank) | | Log Weekly Wage | | Log Hourly Wage | | Employed | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Quota Exposure (x Post), English Speakers | -0.0108 | | 0.0302*** | | 0.0393*** | | -0.0019** | |
| | (0.0067) | | (0.0094) | | (0.0140) | | (0.0008) | |
| Quota Exposure (x Post), Non-English Speakers | 0.0036 | | -0.0015 | | 0.0010 | | 0.0014 | |
| | (0.0040) | | (0.0045) | | (0.0059) | | (0.0014) | |
| Quota Exposure (x Post), from N/W | | -0.0210*** | | 0.0361*** | | 0.0536*** | | -0.0029*** |
| | | (0.0056) | | (0.0082) | | (0.0121) | | (0.0011) |
| Quota Exposure (x Post), from S/E | | 0.0064 | | -0.0045 | | -0.0042 | | 0.0018 |
| | | (0.0042) | | (0.0045) | | (0.0059) | | (0.0014) |
| R-squared | 0.049 | 0.049 | 0.103 | 0.103 | 0.097 | 0.098 | 0.016 | 0.016 |
| Observations | 911,469 | 911,469 | 390,605 | 390,605 | 317,650 | 317,650 | 527,133 | 527,133 |
| Dep. Var. Mean | 0.503 | 0.503 | 2.282 | 2.282 | 2.240 | 2.240 | 0.918 | 0.918 |
| Dep. Var. SD | 0.500 | 0.500 | 0.765 | 0.765 | 0.918 | 0.918 | 0.274 | 0.274 |

*Notes:* The table reports estimates from constructing alternative measures of the quota exposure, restricting to US-born Black men. Odd columns report the results when dividing countries of origin according to the share of immigrants from that country reporting above- or below-median English proficiency. Even columns report the results when dividing immigrants into Southern/Eastern Europeans (S/E) and Northern/Western Europeans (N/W). The dependent variable is an indicator equal to one if the son has a higher occupational rank than his father in columns 1 and 2; the log of the weekly wage in columns 3 and 4; the log of the hourly wage in columns 5 and 6, and the dummy for being employed in columns 7 and 8. All regressions include age dummies, the Census region fixed effects, the log of 1900 county population, and the log of 1900 foreign-born population, with standard errors clustered at the county level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# Table A10. Quota Exposure and Population Growth, by English Proficiency

| | Restricted | Full | Unrestricted | Foreign-Born | US-Born | | |
|---|---|---|---|---|---|---|---|
| | | | | | All | White | Black |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Full Sample | | | | | | | |
| Quota Exposure x Post, English Speakers | -0.0254** | -0.0361** | -0.0107 | -0.0259** | -0.0102 | -0.0078 | -0.0022** |
| | (0.0106) | (0.0152) | (0.0100) | (0.0106) | (0.0101) | (0.0097) | (0.0011) |
| Quota Exposure x Post, Non-English Speakers | -0.0291*** | -0.0192* | 0.0099 | -0.0285*** | 0.0093 | 0.0065 | 0.0028** |
| | (0.0094) | (0.0099) | (0.0084) | (0.0092) | (0.0085) | (0.0082) | (0.0013) |
| R-squared | 0.841 | 0.938 | 0.946 | 0.841 | 0.946 | 0.943 | 0.924 |
| Observations | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 |
| Dep. Var. Mean | 0.033 | 0.463 | 0.430 | 0.034 | 0.429 | 0.414 | 0.014 |
| Dep. Var. SD | 0.280 | 3.102 | 2.908 | 0.281 | 2.907 | 2.774 | 0.159 |
| Panel B: Urban Sample | | | | | | | |
| Quota Exposure x Post, English Speakers | -0.0267** | -0.0508** | -0.0241* | -0.0273** | -0.0235* | -0.0196 | -0.0034*** |
| | (0.0123) | (0.0206) | (0.0140) | (0.0122) | (0.0140) | (0.0135) | (0.0012) |
| Quota Exposure x Post, Non-English Speakers | -0.0251** | -0.0094 | 0.0157 | -0.0244** | 0.0150 | 0.0115 | 0.0034** |
| | (0.0099) | (0.0104) | (0.0100) | (0.0096) | (0.0102) | (0.0097) | (0.0016) |
| R-squared | 0.750 | 0.916 | 0.916 | 0.751 | 0.916 | 0.918 | 0.788 |
| Observations | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 |
| Dep. Var. Mean | 0.025 | 0.299 | 0.274 | 0.025 | 0.274 | 0.258 | 0.016 |
| Dep. Var. SD | 0.173 | 0.583 | 0.459 | 0.173 | 0.459 | 0.407 | 0.104 |

*Notes:* This table reports results from county-level panel regressions for 1900, 1920, and 1940 that examine the relationship between quota exposure and population growth across different demographic groups. The dependent variable is the change in a group's county population between censuses, scaled by that group's baseline population in the preceding period. We use an alternative measure of the quota exposure: we divide countries of origin according to the share of immigrants from that country reporting above- or below-median English proficiency. All specifications include county fixed effects and the Census region–by–period fixed effects and are weighted by the number of linked individuals. Column 1 focuses on the foreign-born population from quota-restricted countries. Columns 2 and 3 examine growth in the total county population and in the unrestricted population (i.e., US-born individuals and immigrants not targeted by the quotas). The remaining columns report results separately for foreign-born residents (column 4) and for US-born residents overall and by race (columns 5–7). Panels A and B present estimates for all counties and for urban counties (defined as those with a strictly positive urban population share in 1900), respectively. Standard errors are clustered at the county level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

## Table A11. Quota Exposure and Population Growth, by Origin

| | Restricted | Full | Unrestricted | Foreign-Born | US-Born | | |
|---|---|---|---|---|---|---|---|
| | | | | | All | White | Black |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Panel A: Full Sample | | | | | | | |
| Quota Exposure x Post, N/W | -0.0297*** | -0.0247** | 0.0050 | -0.0302*** | 0.0055 | 0.0076 | -0.0018* |
| | (0.0095) | (0.0113) | (0.0076) | (0.0095) | (0.0076) | (0.0073) | (0.0011) |
| Quota Exposure x Post, S/E | -0.0290*** | -0.0252** | 0.0039 | -0.0284*** | 0.0033 | 0.0005 | 0.0027** |
| | (0.0096) | (0.0107) | (0.0081) | (0.0094) | (0.0082) | (0.0080) | (0.0013) |
| R-squared | 0.842 | 0.938 | 0.946 | 0.842 | 0.946 | 0.943 | 0.924 |
| Observations | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 | 5,472 |
| Dep. Var. Mean | 0.033 | 0.463 | 0.430 | 0.034 | 0.429 | 0.414 | 0.014 |
| Dep. Var. SD | 0.280 | 3.102 | 2.908 | 0.281 | 2.907 | 2.774 | 0.159 |
| Panel B: Urban Sample | | | | | | | |
| Quota Exposure x Post, N/W | -0.0300*** | -0.0355** | -0.0055 | -0.0306*** | -0.0049 | -0.0014 | -0.0030** |
| | (0.0108) | (0.0140) | (0.0093) | (0.0107) | (0.0094) | (0.0091) | (0.0012) |
| Quota Exposure x Post, S/E | -0.0248** | -0.0154 | 0.0093 | -0.0241** | 0.0086 | 0.0051 | 0.0033** |
| | (0.0101) | (0.0107) | (0.0093) | (0.0098) | (0.0095) | (0.0091) | (0.0016) |
| R-squared | 0.750 | 0.915 | 0.916 | 0.751 | 0.916 | 0.918 | 0.788 |
| Observations | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 | 2,172 |
| Dep. Var. Mean | 0.025 | 0.299 | 0.274 | 0.025 | 0.274 | 0.258 | 0.016 |
| Dep. Var. SD | 0.173 | 0.583 | 0.459 | 0.173 | 0.459 | 0.407 | 0.104 |

*Notes:* This table reports results from county-level panel regressions for 1900, 1920, and 1940 that examine the relationship between quota exposure and population growth across different demographic groups. The dependent variable is the change in a group's county population between censuses, scaled by that group's baseline population in the preceding period. We use an alternative measure of the quota exposure: we divide immigrants into Southern/Eastern Europeans (S/E) and Northern/Western Europeans (N/W). All specifications include county fixed effects and Census region–by–period fixed effects and are weighted by the number of linked individuals. Column 1 focuses on the foreign-born population from quota-restricted countries. Columns 2 and 3 examine growth in the total county population and in the unrestricted population (i.e., US-born individuals and immigrants not targeted by the quotas). The remaining columns report results separately for foreign-born residents (column 4) and for US-born residents overall and by race (columns 5–7). Panels A and B present estimates for all counties and for urban counties (defined as those with a strictly positive urban population share in 1900), respectively. Standard errors are clustered at the county level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.