Assimilation in the Shadow of Restriction: Evidence from the 1921 Emergency Quota Act*

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Abstract

High levels of immigration in major destination countries have renewed pressure to restrict entry. How do such restrictions affect incumbent immigrants? We examine this question in the context of the 1921 Emergency Quota Act, focusing on the assimilation of immigrants already in the United States. In a simple model, restrictions may raise the return to assimilation through reduced growth of the immigrant community or increased anti-immigrant hostility. Although both channels foster assimilation, they yield opposite predictions about how the effect varies with the prevalence of targeted immigrants. Using the Americanization of children's names as a proxy for parental assimilation effort, we document two main findings. First, the Quota Act increased assimilation among immigrants from targeted countries. Second, the effect declines with the local prevalence of targeted immigrants, pointing to reduced community growth rather than hostility as the underlying mechanism. Patterns of selective migration further support this interpretation. In addition, children born after the Act were more likely to reach the upper tail of the occupational distribution, despite no detectable differences in their own assimilation outcomes.

Keywords: Immigration, Emergency Quota Act, cultural assimilation, long-run outcomes

JEL Codes: F22, J15, N32, N42, Z13

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1 Introduction

More than a century after the Age of Mass Migration, historically high levels of immigration across many major destination countries have renewed pressure to restrict entry (OECD, 2024; U.S. Census Bureau, 2024). The United States and Canada, two of the world's leading destinations for immigrants, have recently tightened legal admissions. The United States has imposed new limits on immigrant entry since 2017, and Canada cut planned permanent admissions by 21 percent in 2024.[1] A similar shift is evident across Europe, where the annual number of restrictive immigration policy changes nearly tripled between the early 2000s and the late 2010s (Figure A14).

The consequences of immigration restrictions are not limited to prospective entrants, but extend to immigrants already living in the country. Through changes in the size and composition of immigrant communities, as well as the broader social and political environment surrounding their reception, such policies may alter how immigrants assimilate into the host society. Given the substantial role of immigrants in shaping the social, political, and economic landscape of the host society (Giuliano and Tabellini, 2020; Sequeira et al., 2020; Alesina and Tabellini, 2024), understanding this relationship is important for a comprehensive assessment of the consequences of restrictive policies, yet it remains underexplored. We examine this question in the context of the 1921 Emergency Quota Act, which ended decades of open-door immigration from Europe to the United States and directly altered the environment that existing immigrant communities faced. We ask whether this restriction affected cultural assimilation among immigrant families already in the United States and, if so, through what mechanisms.

The 1921 Emergency Quota Act followed several decades of large-scale immigration whose composition had changed markedly. The Age of Mass Migration brought more than 30 million Europeans to the United States, raising the foreign-born share of the population to over 14 percent by 1920 (Abramitzky et al., 2014). Around the turn of the twentieth century, the main sources of these flows shifted from Northern and Western Europe to Southern, Eastern, and Central Europe. This shift heightened concerns over labor-market competition and cultural cohesion (Jenks and Lauck, 1912; Hourwich, 1912). Passed with bipartisan support, the Act capped annual admissions from each country at 3 percent of its foreign-born population residing in the United States in 1910. By tying quotas to the 1910 immigrant population, the Act disproportionately targeted the rapidly growing *new* immigration flows, while imposing much weaker restrictions on Northern and Western Europe. The restrictions were highly effective: in the years following the Act, immigration from

[1] In the US, Presidential Proclamation 9645, issued in September 2017, restricted immigrant entry for specified categories of nationals from eight countries. Presidential Proclamation 10014, issued in April 2020, subsequently suspended the entry of many prospective immigrants during the COVID-19 pandemic. Restrictions on legal entry were reintroduced and expanded beginning in 2025. In Canada, the 2025–2027 Immigration Levels Plan, announced in October 2024, reduced planned permanent resident admissions for 2025 from 500,000 to 395,000.

targeted countries fell by 88 percent, while immigration from non-targeted countries increased by 25 percent.

The legislation fundamentally changed the environment faced by immigrants from targeted origins. Most directly, the quotas reduced the expected growth of their local communities. At the same time, the Act may have served as a salient signal of anti-immigrant sentiment, now institutionalized through federal policy. Both changes could affect their assimilation decisions, and understanding what drives these responses has implications for how immigration policies should be designed to account for such unintended effects. If shifts in community size are the primary channel, policy design may focus on how admission rules affect access to immigrant networks and resources. If instead responses are driven by the institutional signal of heightened hostility, the relevant measures would address social and cultural barriers.

We formalize this intuition in a simple model in which immigrants choose how much effort to invest in assimilation. In the model, the relative benefit of assimilation effort depends on two channels: (i) *community size*, the size of the immigrant community, and (ii) *hostility*, the intensity of anti-immigrant sentiment. The model yields two predictions regarding the channels that operate. First, the quota raises assimilation effort regardless of the relative strength of the two channels. Second, the two channels differ in how the response varies with the prevalence of targeted immigrants in the locality before the Act. If the community-size channel dominates, the response is weaker where the targeted population is already large, because the resident stock buffers the decline in new arrivals. If the hostility channel dominates, the opposite holds due to the higher visibility of a larger group. The second prediction relies on the assumption that community size and hostility are complements in the return to assimilation, which is supported by county-level evidence from the pre-Act period. We use these contrasting predictions on how assimilation responses vary with pre-Act prevalence of targeted immigrant groups to empirically assess the relative importance of the two mechanisms.

In the empirical analysis, we use children's first names as a measure of parental assimilation effort (Fouka, 2019; Abramitzky et al., 2020; Carneiro et al., 2020; Saavedra, 2021; Fouka et al., 2022; Abramitzky et al., 2025). From the full-count 1930 Census (Ruggles et al., 2024, 2025), we construct an American name index that measures the prevalence of each name among native-born whites relative to the foreign-born. Using this index, we compare the names given to children born before and after the Quota Act across households from targeted and non-targeted regions in an event-study specification. To address the concern that households from the two groups may differ in ways that independently influence naming practices, we include a full set of household fixed effects. These absorb any time-invariant family characteristics, including origin-specific naming customs, assimilation attitudes, religious background, socioeconomic conditions, as well as unobserved parental preferences, that may influence the naming decisions of parents. Birth-year fixed effects

are also included to absorb time trends and year-specific shocks to naming patterns common across all households. Identification therefore comes from changes in naming practices within the same family around the passage of the Act, conditional on aggregate shocks to immigrants over time. In addition, we control for birth-order fixed effects, which account for any differences in naming practices between earlier- and later-born children, and gender, which controls for differential naming practices across boys and girls.

The results indicate that in households from targeted regions, children born after the Quota Act received more Americanized names than their siblings born before the Act. The estimated increase in the American name index corresponds to the level of assimilation that immigrants typically achieve with two and a half additional years of residence in the United States (Abramitzky et al., 2020). This pattern is not driven by differential pre-trends between the targeted and non-targeted groups. The coefficients for pre-Act cohorts are close to zero, and placebo treatment periods yield no comparable effects. Moreover, the sensitivity analysis proposed by Rambachan and Roth (2023) suggests that the effect is robust to substantial departures from parallel trends. We further show that the results hold under alternative classifications of treatment status, group-specific birth-order effects, and different sample windows. The estimates also change little when we restrict the sample to European countries or exclude Southern, Central, or Eastern European origins from the treated group individually. The response is no weaker among households whose heads had naturalized, which speaks against an explanation based on the cost of re-entry for temporary migrants. In addition, consistent with the role of cultural attachment in the model, the effect is stronger among those whose household heads likely had weaker ties to their origin culture, as proxied by longer length of residence in the US.

Next, we turn to the mechanisms. The model predicts that the community-size and hostility channels differ in how the assimilation response varies with the prevalence of the targeted group in a locality: assimilation is weaker in areas where the targeted group is already prevalent if the community-size channel dominates, and stronger if the hostility channel does. To test this prediction, we examine heterogeneity in the effect of the Quota Act on name Americanization by the 1920 county-level share of the population born in Quota-affected countries. The increase in name Americanization is larger in counties below the median. Triple-difference estimates show a similar pattern and further support the community-size channel. The heterogeneity is associated with the concentration of the targeted group rather than with the overall immigrant share or the co-national population. This implies that the buffer mechanism, through which a larger existing community mitigates the effect of a decline in its expected growth, functioned at the level of the restricted population as a whole rather than within specific co-ethnic enclaves.

We find further evidence for the community-size channel by examining internal migration after the Quota Act. Households from targeted origins that relocated between 1920 and 1930

moved toward areas with greater concentrations of immigrants targeted by the Act. By contrast, their destinations show no significant association with local support for immigration restriction, proxied by congressional roll-call votes on the Emergency Quota Act. This pattern is consistent with immigrants seeking out larger Quota-origin communities when relocating, rather than responding primarily to differences in local anti-immigrant hostility. The weaker estimated naming response among movers further suggests that relocation may have served as a substitute for cultural adjustment.

Finally, we examine whether the children born after the Act, who received more Americanized names on average, differed in adulthood economic outcomes from their older siblings born before the Act. Between siblings, differences in average occupation-based socioeconomic status were modest, but children born after the Act were significantly more likely to reach the top of the occupational distribution. To interpret these results, we consider two channels through which names may shape economic outcomes: an *external* channel, in which names shape how others perceive the bearer's belonging to the host society, and an *internal* channel, in which names reflect or reinforce the bearer's identification with the host society and adoption of its cultural norms and values. The concentration of effects at the top of the occupational distribution, alongside only modest differences in average occupational status, is more consistent with the external channel in which Americanized names facilitate access to high-status occupations through selection and screening in the labor market, rather than a broader shift in cultural identification among second generation immigrants (the internal channel). Supporting this interpretation, we find no detectable differences in adult assimilation outcomes between siblings, including spousal characteristics, the naming of their own children, and voluntary enlistment during World War II. Taken together, our results provide suggestive evidence that Americanized names mattered more through how others perceived their bearers than through changes in the bearers' own cultural identification.

This study contributes to the understanding of the determinants of immigrant assimilation. The literature examines a wide array of factors, including individual exposure to the host society (Bleakley and Chin, 2010; Abramitzky et al., 2014, 2020) and the social environments or policies of the host country (Lleras-Muney and Shertzer, 2015; Bandiera et al., 2019). In the context of the shift in assimilation decisions among immigrants, two factors have received particular attention as underlying mechanisms. The first is the size of the co-ethnic community (Lazear, 1999; Bisin and Verdier, 2000; Carneiro et al., 2020; Abramitzky et al., 2024). The second is hostility from the host society, whose effects on assimilation are documented in episodes such as World War I, Pearl Harbor, and 9/11 (Gould and Klor, 2016; Fouka, 2019; Saavedra, 2021). We add a new determinant, the expected growth of the immigrant community. Whereas existing studies focus on co-ethnic concentration shaped by past location choices, we show that by lowering expected future inflows without changing the existing stock, the Quota Act had unintended consequences for the

assimilation of incumbent immigrants. Moreover, by incorporating both the community-size and hostility channels within a common framework, our model sheds light on the relative importance of these competing channels.

Our paper also adds to the literature on the consequences of immigration restrictions. Studies of the various quota regimes in the 1920s and other restrictive episodes in US history have largely focused on how restriction affects natives and local economies, including labor markets, the Great Black Migration, and innovation (Clemens et al., 2018; Price et al., 2020; Tabellini, 2020; Lee et al., 2022; Abramitzky et al., 2023; Cornejo-Costas and Morrison, 2026). Much less is known about how these restrictions affected existing immigrants. An exception is Chan (2024), which examines the 1921 and 1924 Acts jointly and finds increased demographic integration among incumbent immigrant men, measured by naturalization and marriage patterns. We extend this approach in two ways. First, we study a different margin of assimilation: the naming of children. Unlike naturalization and marriage, which may have been directly affected by the restrictions and resulting changes in the socioeconomic environment, naming remained an unrestricted choice under the Act. By exploiting within-household shifts in naming practices, we examine cultural assimilation directly, conditional on the individual and local socioeconomic conditions that households faced. Second, we formalize and test the mechanisms underlying the assimilation response. Our model generates opposing predictions for how the effect varies with local immigrant prevalence, allowing us to evaluate the competing explanations.

More broadly, our findings relate to research on how policies shape culture. Some interventions target cultural traits directly. Changes in school curricula and state propaganda have been shown to shift political attitudes and ethnic identity (Cantoni et al., 2017; Blouin and Mukand, 2019). Such efforts can also provoke a backlash, as with bans on the German language in US schools after World War I (Fouka, 2020). Other interventions operate indirectly by altering the economic and social conditions that sustain these traits. For example, pension programs have been shown to affect traditional kinship practices (Bau, 2021), a government resettlement program shaped national identity through intergroup contact (Bazzi et al., 2019), and the Green Revolution in Indonesia persistently increased individualism as measured by naming practices (Jung et al., 2027). Our findings provide another example of cultural change arising indirectly from policy. Although the purpose of the Quota Act was to regulate the entry of new immigrants, it additionally altered the cultural practices of those already in the United States by changing the social environment faced by the incumbent immigrant population.

Finally, this paper builds on the literature on the economic consequences of names. A well-established literature reports that ethnically distinctive names carry labor market consequences (Bertrand and Mullainathan, 2004; Rubinstein and Brenner, 2014; Biavaschi et al., 2017). However, evidence on the underlying mechanisms is mixed. More ethnic names are correlated

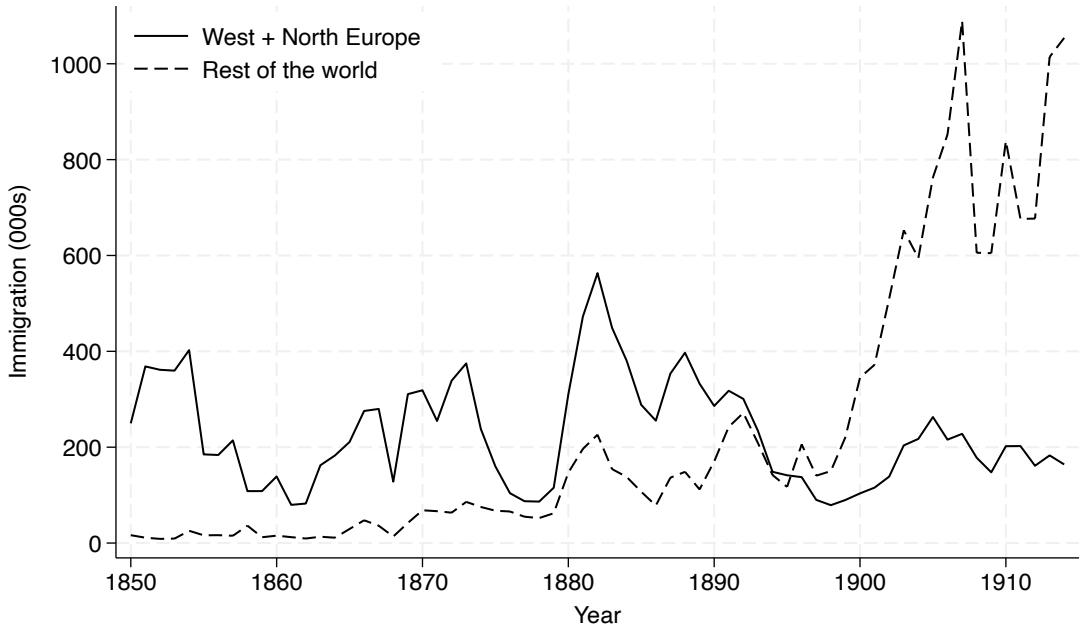
with family background, and some studies find that accounting for these differences leaves little independent effect of names on later outcomes (Fryer and Levitt, 2004; Abramitzky et al., 2020). In contrast, Goldstein and Stecklov (2016) attribute part of the economic advantage associated with more American names to the signaling value of the name itself. Such effects may operate through an external channel, by shaping how others perceive and respond to the bearer (Zhao and Biernat, 2017). Names are also closely connected to identity and belonging, suggesting a potential internal channel through how the bearer identifies with and integrates into the host society (Amit and Dolberg, 2023). Our setting helps address both issues. Name differences between siblings born before and after the Quota Act arise within the same household, so these comparisons absorb any influence of family background. Consistent with Abramitzky et al. (2020), we find little difference at the mean of the occupational distribution. However, post-Act siblings are more likely to reach the upper tail of the distribution, a pattern not explained by differences in adult assimilation outcomes. These results suggest that names could have direct effects through the perceptions of others beyond what family background can explain.

The remainder of the paper proceeds as follows. Section 2 provides historical background on the Emergency Quota Act. Section 3 develops the conceptual framework and its empirical predictions. Section 4 describes the data and estimation strategy. Section 5 presents the main results and robustness exercises. Section 6 examines the mechanisms underlying the assimilation response. Section 7 studies the long-run consequences of naming choices, and Section 8 concludes.

2 The Emergency Quota Act

Background The passage of the 1921 Emergency Quota Act marked a decisive end to a long-standing tradition of openness toward European immigration in the United States (Higham, 2002). Prior to the legislation, the Age of Mass Migration had brought more than 30 million Europeans to the country, raising the foreign-born share of the population to over 14 percent by 1920 (Abramitzky et al., 2014). However, the composition of these immigrants did not remain constant. Following prolonged periods of immigration flows from Northern and Western Europe, the primary origins of arrivals shifted around the turn of the twentieth century to poorer, culturally distinct regions, primarily from Southern, Eastern, and Central Europe. Figure 1 plots the flow of immigrants from Western and Northern Europe, compared to immigrants from the rest of the world, prior to the passing of the Quota Act. By 1910, immigrant arrivals from the rest of the world were four times those from Western and Northern Europe.

Figure 1: Immigration Prior to the Quota Act



Notes: The figure plots historical immigration flows from Abramitzky et al. (2023). The underlying series are based on historical immigration statistics reported in *Historical Statistics of the United States* ("Immigrants, by country of last residence—Europe: 1820–1997").

Rising Anti-Immigrant Sentiment This shifting demographic landscape contributed to growing anti-immigrant sentiment, reflecting concerns over labor-market competition and cultural cohesion (Jenks and Lauck, 1912; Hourwich, 1912). Contemporaneous scholars and labor groups argued that the influx of foreign workers depressed wages and displaced native-born Americans in sectors such as mining and manufacturing. These concerns were formalized by the Dillingham Commission in 1911, which concluded:

> The extensive employment of southern and eastern Europeans has seriously affected the native American and older immigrant employees from Great Britain and northern Europe by causing displacements and by retarding advancement in rates of pay and improvements in conditions of employment. (Jenks and Lauck, 1912, pp. 209–210)

The Commission's policy recommendation followed directly from this assessment, calling the influx of immigrants "a condition which demands legislation restricting the further admission of such unskilled labor" (Jenks and Lauck, 1912, p. 363).

Anti-immigrant sentiment also extended beyond economic concerns to the perceived cultural consequences of the new immigration. In contrast to earlier waves from northern and western Europe, immigrants arriving from southern and eastern Europe were increasingly portrayed as culturally distinct and less capable of assimilating into American society. As Handlin (1957)

describes, the premise underlying the restrictionist movement was that "the national origin of an immigrant was a reliable indication of his capacity for Americanization," with immigrants who came in earlier in the 1800s being "drawn from superior stocks of northern and western Europe," while the newer arrivals were "drawn from the inferior breeds of southern and eastern Europe" (pp. 94–95). Such concerns were also reflected in the writings of members of the Dillingham Commission, who argued that "the coming in of people who will not be assimilated creates discord and makes separate classes or castes in a community" and that an immigrant who "can not be adjusted with a reasonable degree of readiness to the customs and institutions of his adopted country brings an undesirable element into the community and would better be excluded" (Jenks and Lauck, 1912, pp. 285–286).

The Legislation In response to these pressures, Congress enacted the Emergency Quota Act in 1921. The passing of the Act with bipartisan support reflected a political consensus for immigration restriction at the time (Goldin, 1994). The legislation established a strict limit on the number of immigrants allowed to enter the United States, reducing entry from around 800,000 to approximately 360,000 immigrants per year.

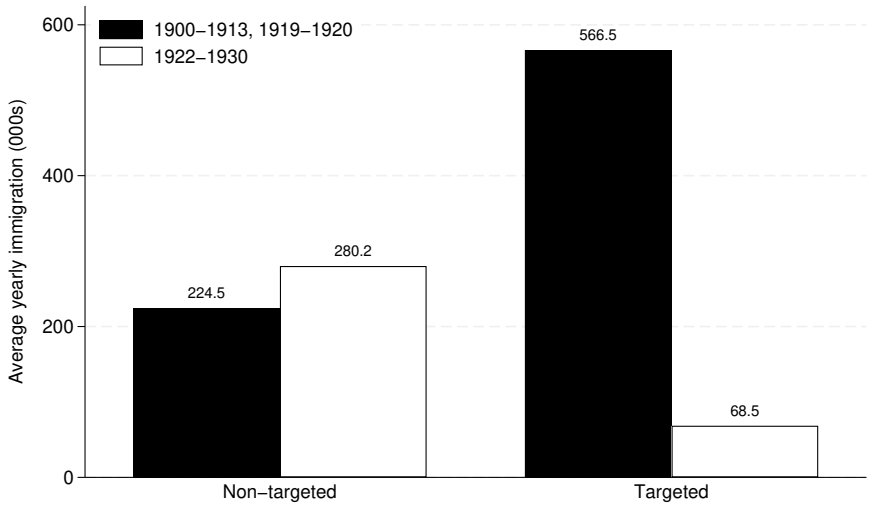
Specifically, the Act introduced an unprecedented system of immigration quotas by country of origin, restricting the annual number of immigrants from any single nation to 3 percent of that nationality's population already residing in the United States, as recorded in the 1910 Census. The primary purpose of this national origins formula was to deliberately reshape the ethnic composition of immigrants, largely by limiting the arrival of Southern, Eastern, and Central Europeans. As the quotas were tied to the existing foreign-born population, the law placed resident immigrant communities at the center of the policy: the permitted inflow of each nationality was now defined as a proportion of its own stock.

The Effect of the Quotas on Immigration The Emergency Quota Act was immediately successful in reducing immigration from targeted countries. In Figure 2, we plot the average yearly immigration flows in the years before and after the Quota Act separately for targeted and non-targeted countries, based on the classification described in Section 4.1.1. Prior to the Quota Act, approximately 566,500 immigrants from targeted countries arrived on average per year, compared to 224,500 from non-targeted countries. Following the Act, yearly immigration flows from targeted countries decreased by 88 percent (from 566,500 to 68,500) while those from non-targeted countries increased by 25 percent (from 224,500 to 280,200).[2] The Act therefore generated sharply different experiences for immigrants already in the United States: targeted immigrants experienced an immediate collapse in new arrivals from their countries of origin, while immigration from non-targeted countries continued

[2]Pre-quota yearly flows are averaged from 1900–1913 and 1919–1920 to avoid immigration flows during World War I, which were lower across all countries.

at rates comparable to or above their pre-Act levels.

Figure 2: Average Yearly Immigration Flows, Pre- vs Post-Quota Act



Notes: The figure plots the average yearly immigration flows before and after the 1921 Quota Act, separately for regions that were targeted and not targeted by the Act. Pre-Quota Act flows do not include years 1914–1918 due to World War I decreasing immigration overall. The immigration flows data are from Abramitzky et al. (2023). The underlying series are based on historical immigration statistics reported in *Historical Statistics of the United States* ("Immigrants, by country of last residence—Europe: 1820–1997").

Visibility of the Policy The effects of the Emergency Quota Act were highly visible to immigrants already in the United States. It sharply restricted the flow of new arrivals into their communities and drew close attention from ethnic mutual-aid societies, foreign-language newspapers, and community leaders (Ngai, 2014). The Act's visibility was further heightened by its strict enforcement. Quotas were enforced monthly at ports of entry such as Ellis Island, and steamship lines engaged in widely reported "midnight races" to land passengers before national quotas were filled. Even the slightest delay resulted in passengers being detained or returned (Daniels, 2005).

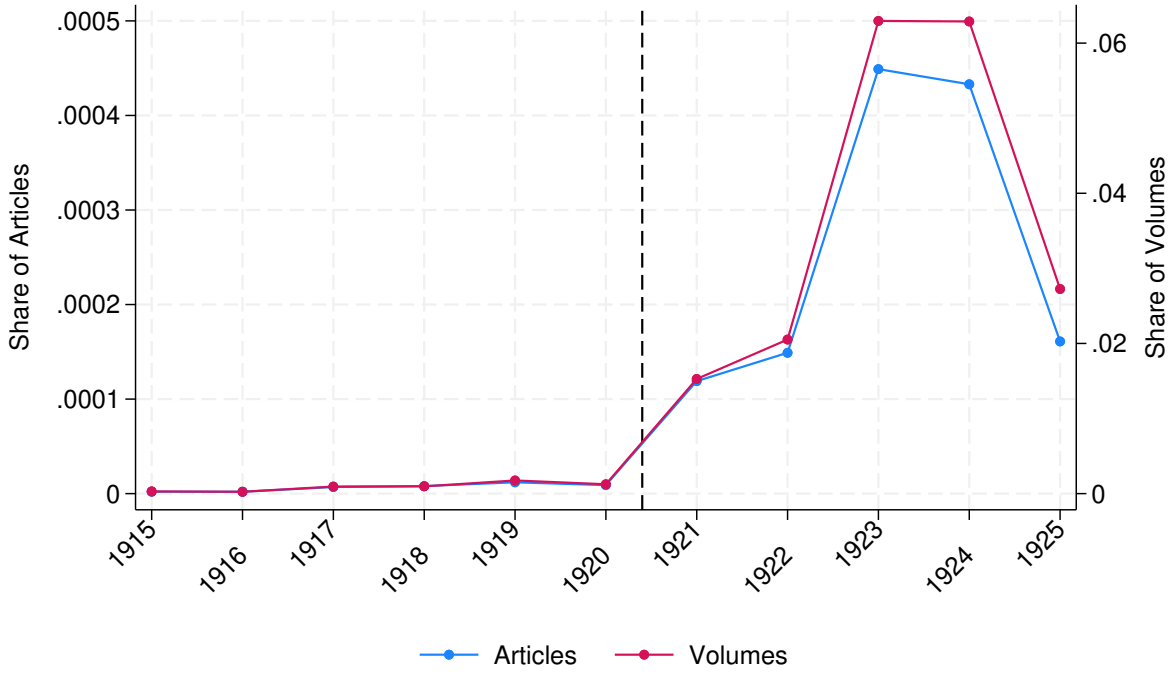
To gauge the prominence of the Quota Act in contemporary public discourse, we use newspaper coverage surrounding its passage. We draw on the American Stories dataset, which extracts article-level text from the Chronicling America collection that contains millions of pages from a broad cross-section of local and national newspapers and provides extensive coverage of public discourse during this period (Dell et al., 2023).[3] Figure 3 plots the share of articles and volumes in the collection that jointly mention "immigration" and "quota."[4] Although these shares are small in absolute terms, reflecting the fact that they are calculated relative to the full universe of newspaper

[3]Similar approaches have been used in the economics literature to infer the contemporary salience of issues from their prevalence in Chronicling America newspapers; see, for example, Esposito et al. (2023), Ferrara and Fishback (2024), and Saavedra (2025).

[4]We search for the terms in both the headline and the body text.

content, their evolution is striking. Mentions are negligible before 1921 but increase sharply following the passage of the Act, suggesting that immigration quotas became a salient topic of public discussion precisely as the new restrictions took effect. The widespread attention surrounding the Act makes it likely that immigrants in the US were aware of a policy that directly restricted entry from their countries of origin and affected the size of their communities. We argue that this shock, in turn, altered their incentives to assimilate. The following section formalizes this hypothesis.

Figure 3: Newspaper Coverage of the Emergency Quota Act



Notes: The figure plots the shares of articles and volumes that mention "immigration" and "quota" out of the total number of articles and volumes, respectively, from the American Stories dataset (Dell et al., 2023).

3 Conceptual Framework

We hypothesize that the Quota Act fostered the assimilation efforts among existing immigrants from Quota-affected countries. The effects of the Quota shock could have operated through two distinct mechanisms. The first is the *community-size* channel: a quota reduces the inflow of new arrivals and thus the expected growth of the immigrant community, lowering the return to retaining an immigrant identity and raising the relative return to assimilating into the mainstream community (Lazear, 1999). The second is the *hostility* channel: a restrictive policy regime may intensify perceived or actual antipathy toward immigrants, thereby raising the cost of retaining an immigrant identity and

encouraging assimilation.[5] These two channels are observationally equivalent in the aggregate: both predict that existing immigrants would increase their assimilation effort in response to the Quota Act, so the average response alone cannot identify which force is at work. However, distinguishing between these channels can inform the design of policies to account for the integration of incumbent immigrants and their broader economic and social contributions to the host country. Although a full welfare analysis is beyond the scope of this paper, prior research shows that integration policies can improve immigrants' economic outcomes and generate fiscal benefits for the receiving society (Sarvimäki and Hämäläinen, 2016; Heller and Mumma, 2023). Importantly, their effectiveness depends on policy design (Foged et al., 2024), and poorly designed assimilation policies may even generate counterproductive responses (Fouka, 2020).

In this section, we propose a simple model to distinguish the relative importance of these two channels for the effects of the Quota shock. While they yield identical implications for the assimilation response to immigration restriction, they generate opposite predictions for how the response varies with the size of the pre-existing immigrant community, which allows the data to shed light on the operative channel.

3.1 Setup

An immigrant parent i chooses assimilation effort $a_i \geq 0$, which corresponds to the choice of name given to their children in our empirical analysis.[6] Parent i solves:

$$\max_{a_i \geq 0} \; a_i \, B(M, H) \; - \; \frac{\kappa_i}{2} \, a_i^2. \tag{1}$$

$B(M, H)$ is the relative benefit of assimilation into the mainstream community per unit of effort.[7] It depends on two features of the local environment: the size of the immigrant community that constitutes the individual's reference group, M, and the degree of nativist hostility, H.[8] Assimilation

[5] This scenario aligns with the literature on increased assimilation in response to heightened discrimination (Fouka, 2019; Saavedra, 2021; Chen and Xie, 2024). However, an increase in perceived or actual hostility may also produce the opposite effect. Theoretical studies suggest that greater hostility could reinforce existing identity (Akerlof and Kranton, 2000; Bisin and Verdier, 2000), consistent with evidence on backlash among minority groups facing discrimination (Gould and Klor, 2016; Fouka, 2020). In this case, immigration restriction would predict a reduction in assimilation among existing immigrants, which is inconsistent with our empirical findings. We therefore do not incorporate this possibility into our conceptual framework.

[6] The model can also be interpreted as an individual choice of the level of effort that maximizes one's own relative benefits from assimilation. Because our empirical analysis relies on the names that parents give to siblings, we retain the parental-choice framework throughout.

[7] $B(M, H)$ is a reduced-form benefit that can be derived from primitives, such as labor-market returns to assimilation and social interactions within the immigrant community. We provide a brief overview of a microfoundation of $B(M, H)$ in Appendix D.1.

[8] For simplicity, the model defines M as the entire immigrant population. In practice, interpretation of M may involve different dimensions. In our empirical context, immigrants from the targeted countries may serve as the relevant reference group, or the ethnic enclave may matter if ethnicity-specific capital and networks shaped the relative benefits

effort involves a quadratic cost as parents hold attachment to their origin culture, with $\kappa_i > 0$ capturing the strength of their cultural attachment (Akerlof and Kranton, 2000; Bisin and Verdier, 2000, 2001).

We impose three assumptions on B.

(A1) *Community substitution*: $B_M < 0$ and $B_{MM} > 0$.
A larger immigrant community substitutes for integration into the mainstream community, lowering the marginal return to assimilation at a diminishing rate.

(A2) *Costly distinctiveness*: $B_H > 0$ and $B_{HH} < 0$.
Greater hostility raises the cost of remaining distinct, and thus increases the return to assimilation at a diminishing rate.

(A3) *Complementarity*: $B_{MH} > 0$.
Hostility and immigrant prevalence are complements in the return to assimilation: hostility erodes the value of ties within the immigrant community, making the community a less effective substitute for mainstream assimilation ($\partial B_M/\partial H > 0$). Equivalently, a given increase in hostility raises the return to assimilation more when the immigrant community is larger, because more community-based value is at stake ($\partial B_H/\partial M > 0$).

Assumption (A3) is the key assumption that separates the role of the two channels in the model. However, alternative scenarios could be possible. For example, an increase in hostility might make the ethnic or immigrant community a more valuable asset, leading to $B_{MH} < 0$. To evaluate the plausibility of Assumption (A3), Table A8 examines the county-level association between assimilation outcomes, immigrant prevalence, and anti-immigrant sentiment before the introduction of the Emergency Quota Act. We regress the American name index of children born to immigrant parents between 1910 and 1920 on county-level hostility, the 1910 share of Quota-origin immigrants, and their interaction. Hostility is measured by roll-call votes on immigration restriction bills. Because (A3) implies that hostility and immigrant prevalence are complements, the model predicts a positive coefficient on the interaction term, and the estimated coefficient is positive and statistically significant across all hostility measures. The implied marginal effects are also consistent with (A1) and (A2).

Assuming $B > 0$ throughout (i.e., net positive benefits from assimilation), the optimal level of a_i is uniquely given by:

$$a_i^* = \frac{B(M, H)}{\kappa_i} \tag{2}$$

of assimilation. The results in Table 5 suggest that the relevant reference group consists of immigrants from the targeted countries rather than the immigrant population as a whole or co-nationals.

The optimal level of assimilation effort is increasing in the relative benefits from assimilation (B) and decreasing in the extent of parental attachment to cultural heritage (κ_i). Table A6 provides evidence on the latter, indicating that the naming response to the Quota Act was weaker among more recent immigrants, who likely retained stronger attachment. This pattern is robust to ranking arrival timing within each origin country.

3.2 The Quota Shock

The model represents the Emergency Quota Act with a parameter q, where higher values denote tighter restriction. The Quota shock operates through the two channels discussed above:

$$M(q) = M_0 + m(q), \quad m_q < 0; \qquad\qquad H(q) = H_0 + h(q), \quad h_q > 0, \tag{3}$$

where M_0 and H_0 are pre-Quota immigrant community size and anti-immigrant sentiment: a tighter quota shrinks the inflow of immigrants ($m_q < 0$) and raises perceived antipathy ($h_q > 0$). $h(q)$ may reflect both changes in subjective perception among immigrants and actual hostility among natives stimulated by the policy. Differentiating Equation (2) gives the assimilation response to the quota,

$$\frac{da_i^*}{dq} = \frac{1}{\kappa_i}\Big(\underbrace{B_M\, m_q}_{\text{community-size channel} > 0} + \underbrace{B_H\, h_q}_{\text{hostility channel} > 0} \Big) > 0 \tag{4}$$

Both terms are positive: the community-size and hostility channels each raise assimilation.

Prediction 1. *Introduction of the immigration quota raises assimilation effort among existing immigrants, regardless of which channel dominates.*

3.3 Heterogeneity in Assimilation Response

The channels diverge once we ask how the response depends on the existing prevalence of immigrants, M_0. Using $\partial M/\partial M_0 = 1$, with m_q and h_q independent of M_0, differentiating Equation (4) gives[9]

$$\frac{\partial}{\partial M_0}\left(\frac{da_i^*}{dq}\right) = \frac{1}{\kappa_i}\Big(\underbrace{B_{MM}\, m_q}_{<0} + \underbrace{B_{MH}\, h_q}_{>0} \Big) \tag{5}$$

The two channels now pull in opposite directions. Through the community-size channel, $B_{MM}\, m_q < 0$: since the marginal return to immigrant prevalence decreases as M grows (A1), a quota-induced contraction in M shifts the benefit less where the immigrant community is already large. The

[9] This simplification is not essential: allowing existing community size to affect the magnitude of either shock does not alter the predictions of the model. Appendix D.3 provides details.

resident stock *buffers* the shock, so the assimilation response is smaller. Through the hostility channel, $B_{MH}\, h_q > 0$: by complementarity (A3), the hostility-driven gains from assimilation are *amplified* in larger immigrant communities (e.g., greater costs from non-assimilation due to stronger visibility (Tabellini, 2020)), so the response is larger there.

The sign of Equation (5) therefore reveals the dominant channel:

$$\frac{\partial}{\partial M_0}\left(\frac{da_i^*}{dq}\right) \begin{cases} < 0 & \Longrightarrow \text{ community-size channel dominates} \\ > 0 & \Longrightarrow \text{ hostility channel dominates} \end{cases} \tag{6}$$

Prediction 2. *The effect of the Quota shock on assimilation effort attenuates with the baseline prevalence of immigrants if the community-size channel dominates, and increases with it if the hostility channel dominates.*[10]

The intuition is clear. If the effect operates through the reduced inflow of future immigrants, it would be weaker where the immigrant community is already large enough to buffer the decline in new arrivals; if it works by raising perceived hostility, the effect would be stronger where immigrants are more prevalent and thus more visible.

A final consideration concerns the composition of the immigrant community M. For simplicity, our model treats immigrants as a single group. In practice, three nested definitions of M are possible: co-national immigrants, immigrants from Quota-affected countries, and all immigrants. This is not merely a matter of measurement but a significant factor in the underlying mechanism, as it bears on the scope of the community whose size affects the relative return to assimilation in Assumption (A1). First, the overall immigrant share should not be the operative measure. Given the targeted nature of the Quota Act, heterogeneity that tracks the aggregate immigrant share would not point to the mechanism the model proposes.

Second, even among Quota-affected immigrants, the boundary of the reference group may vary. One possibility is that the relevant benefits are origin-specific, arising from co-ethnic networks such as shared language, kinship ties, and origin-country institutions. Alternatively, they may extend across origins, arising from the collective standing of immigrants from restricted countries, such as their relative prevalence in the locality and the social norms sustained by that presence. The former implies that co-nationals constitute the operative reference group, so that only the local co-national stock buffers the decline in new arrivals. The latter, by contrast, implies that the broader Quota-origin population underlies the community-size channel. In Section 6, we evaluate Prediction

[10]The analogous comparison in baseline hostility gives $\frac{\partial}{\partial H_0}\left(da_i^*/dq\right) = \frac{1}{\kappa_i}\left(B_{MH}\, m_q + B_{HH}\, h_q\right) < 0$, unambiguously negative under Assumptions (A2) and (A3): both channels exhibit diminishing returns to hostility, so the quota's effect is smaller where baseline hostility is already high, regardless of the operative channel. Our main analysis does not focus on the hostility margin, which does not distinguish between the two channels. Related evidence can be found in Table A7.

2 across the three definitions of the reference group.

4 Estimation Strategy

4.1 Data

We use the full-count 1930 US Census to identify the universe of immigrant families in the US during the Quota Act period (Ruggles et al., 2024, 2025). We define immigrant parents as household heads and spouses born outside the United States, as indicated by their reported country of birth. Our analysis focuses on the US-born children of these immigrant parents during the period around the Quota Act (1918–1923), and uses the names they give their children as the primary measure of assimilation effort.

4.1.1 Quota Targeting

We classify immigrant families as either targeted or non-targeted by the Quota Act, based on the birth country of the father. Following Abramitzky et al. (2023), countries are categorized into targeted versus non-targeted by the degree to which the Quota Act restricted immigrant flows, which we compute through the "Quota Intensity Index". The index captures the share of a country's predicted immigration in the absence of the Quota Act that would have been excluded by the quotas assigned in the 1920s. Formally, the index is defined as

$$QI_c = \max\left\{0, \frac{\widehat{Imm}_c - Quota_c}{\widehat{Imm}_c}\right\} \tag{7}$$

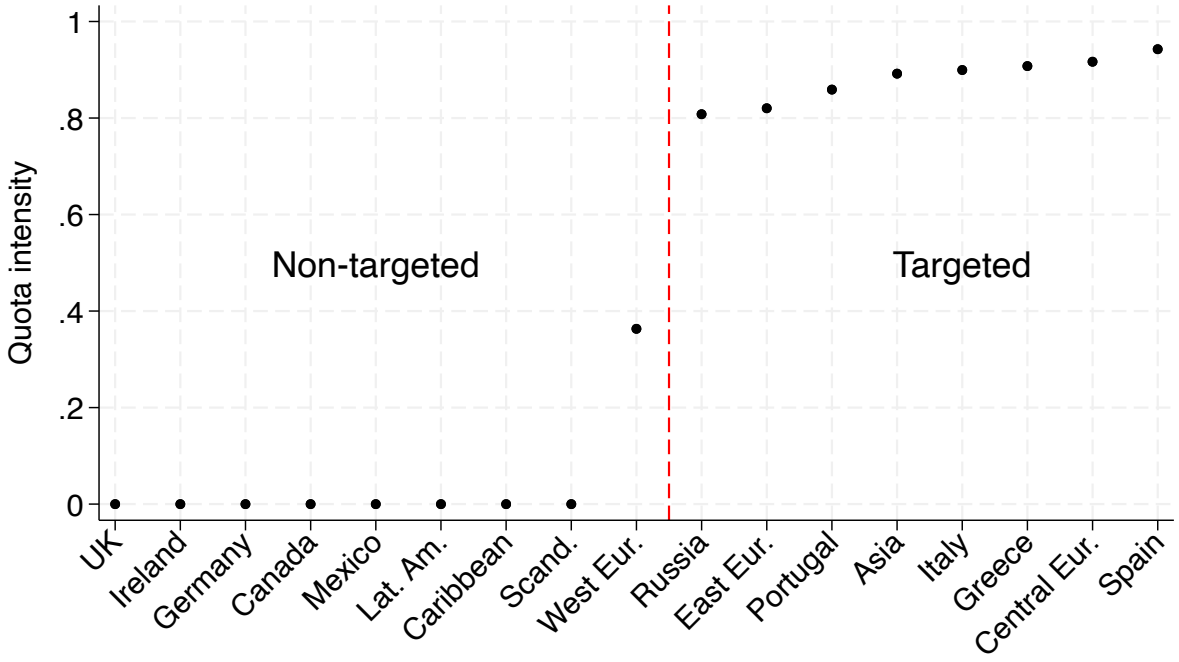
where $\widehat{Imm}_c$ is the predicted immigration flow from country c in the absence of the Quota Act and $Quota_c$ is the total quota slots allotted to country c.[11] The index is zero if the assigned quota slots are greater than the predicted immigrant flows (i.e., the quota is non-binding), and increases as the quota slots become increasingly restrictive relative to the predicted immigrant flow.

Figure 4 reports the categorization along with the index values. The index aligns closely with the historical narrative, with targeted countries having index values near one, and non-targeted countries having values near or equal to zero. The value of the index is relatively high for Western Europe, including the Netherlands, Switzerland, Belgium, France, and Luxembourg, because the stock of immigrants from these countries was comparatively small. However, these countries are considered

[11] Following Abramitzky et al. (2023), we predict immigration flows by regressing annual immigrant flows from each country group on a quadratic function of time and an indicator for war or recession years. A country's series begins in the first year its flow exceeds 2,000 arrivals, giving most countries about 100 years of unrestricted immigration flows. We plot yearly immigration flows from 1900 to 1930, along with the counterfactual immigration flow in the absence of the Quota Act from 1922 to 1930 in Appendix Figure A15.

to have faced low restrictions in terms of both historical context and actual immigration flows (Abramitzky et al., 2023). As shown in Figure A6, the results are robust to dropping households whose parents are from Western Europe.

Figure 4: Quota Intensity by Country



Notes: The figure plots quota intensity by country of origin, following Abramitzky et al. (2023). Quota intensity measures the share of predicted immigration flows in the absence of restrictions that would have been excluded under the quotas imposed in the 1920s. The index ranges from 0 to 1, with higher values indicating more restrictive quotas.

4.1.2 American Name Index

Assimilation reflects choices that immigrants make about how much cultural distance to maintain from the host society. However, such underlying choices are difficult to measure directly, and the literature has often used proxies of assimilation such as languages spoken, religious participation, naturalization, and intermarriage. These outcomes reflect both an individual's decision to assimilate, and the constraints imposed by their environment, including the availability of services in their native language and the composition of the marriage market. Changes in these outcomes therefore do not distinguish changes in an immigrant's willingness to assimilate from changes in the constraints they face. For this reason, a large literature uses the names that parents give their children as a measure of assimilation (Fouka, 2020; Abramitzky et al., 2020; Carneiro et al., 2020; Saavedra,

2021; Fouka et al., 2022; Abramitzky et al., 2025).[12] Naming represents a relatively unconstrained parental choice and thus provides a more direct indication of the cultural identity parents wish to maintain and transmit to their children.

We measure assimilation effort using the names that immigrant parents give to their children born in the United States. Specifically, following Abramitzky et al. (2020), we construct an "American name index" (ANI) for the first names reported in the restricted-access 1930 Census. For a given name k, the index is defined as

$$\text{ANI}_k = 100 \times \frac{\text{Share}(\text{name} = k|\text{Native})}{\text{Share}(\text{name} = k|\text{Native}) + \text{Share}(\text{name} = k|\text{Foreign})}. \tag{8}$$

The index captures the extent to which name k is held by native-born whites with native-born parents, relative to foreign-born individuals.[13] The index ranges from 0 to 100, with a value of 100 reflecting a name only observed among native-born individuals, and a value of zero reflecting a name only observed among foreign-born individuals.

We assign index values to the names of all children born in the United States with immigrant parents. For a child born in year t, we calculate the name shares using the prior 21 birth cohorts starting from $t-1$. A high index value indicates that the (immigrant) parents assigned their child a name that is more frequent among the native-born population relative to the foreign-born population, hence more "Americanized".

Table 1 reports summary statistics for the American name index by group and birth cohort. In both groups, children born after the Quota Act received more Americanized names than those born before it, consistent with Abramitzky et al. (2020) who show that immigrants tend to give their children less foreign names as they spend more time in the country. However, the increase is more than twice as large in targeted households, motivating our analysis of whether the Quota Act contributed to this differential change.

Despite this larger increase, children in non-targeted households have higher index values in both cohorts, reflecting the higher share of families of northwestern European origin in the non-targeted group.[14] Our identification strategy does not require the two groups to have similar levels of name Americanization. It requires instead that name Americanization would have followed parallel trends across targeted and non-targeted households in the absence of the Quota Act (Section 4.2). The two groups are similar in other child and household characteristics. Although birth order is slightly

[12] A broader social sciences literature documents that names are a key marker of cultural identity (Fryer and Levitt, 2004; Twenge et al., 2010; Varnum and Kitayama, 2011; Bazzi et al., 2020).

[13] Distinctiveness of names by group has been applied in different contexts. For instance, Fryer and Levitt (2004) use this approach to measure distinctively Black names, while Fouka (2020) and Abramitzky et al. (2020) use this approach to identify names of specific immigrant origins in the US population.

[14] Table A12 presents the most popular names given to the children born to immigrants between 1918 and 1923, separately for targeted and non-targeted households.

higher in targeted households, it increases by a similar amount between cohorts in both groups.

Table 1: American Name Index: Summary Statistics

	Non-targeted		Targeted	
	Pre	Post	Pre	Post
American name index: mean	45.46	46.32	37.71	39.70
American name index: standard deviation	23.94	24.04	21.44	21.66
Female	0.50	0.50	0.50	0.50
Age	10.48	7.51	10.56	7.52
Birth order	2.89	3.98	3.09	4.20
Total number of children	161,191		659,807	
Total number of families	65,342		262,615	

Notes: The sample consists of children born in the United States between 1918 and 1923 whose parents were both foreign-born, restricted to households with at least one child born in or before 1921 and one born after 1921. Targeted and non-targeted households are classified according to the father's country of birth. The American Name Index measures the degree of name Americanization on a scale from 0 to 100, with higher values indicating more Americanized names. Pre-Quota children are those born between 1918 and 1921, and post-Quota children are those born between 1922 and 1923.

4.2 Empirical Strategy

Sibling Event Study We estimate the assimilation effort induced by the Quota Act by comparing names given to *siblings* born before and after the Quota Act, across immigrant families from targeted versus non-targeted countries. Specifically, we estimate the following equation:

$$y_{iht} = \alpha_h + \alpha_t + \sum_{\substack{k=1918,\\ k\neq 1921}}^{1923} \beta_k \mathbb{1}\{t = k\} Quota_h + \Gamma X_i + \epsilon_{iht}. \tag{9}$$

y_{iht} is the American name index of a child i born in year t to household h. The sample consists of children whose parents were both born outside the United States, and $Quota_h$ is an indicator equal to one if the father of child i is from a targeted country.[15] α_h and α_t are household and birth-year fixed effects, respectively. We additionally control for child sex and birth order in X_i to address differences in naming patterns correlated with these characteristics (Abramitzky et al., 2020).

[15] In Section 5.2, we show that the results are robust to using the mother's origin country or that of either parent to define whether child i is from a targeted country.

Sample Period Our primary analysis sample consists of children born between 1918 and 1923, roughly three years on either side of the Emergency Quota Act's enactment in 1921. The choice of sample window reflects two considerations: (i) the temporal distance between siblings' births, and (ii) household-level exposure to other historical shocks that may differentially affect naming decisions across immigrant groups.

The first consideration arises from evidence that immigrant parents give progressively more American names to their children as their time in the United States increases (Abramitzky et al., 2020). To the extent that this process differs across targeted and non-targeted groups, the resulting changes in naming may confound the estimated response to the Quota Act. Birth-order fixed effects account for systematic differences in naming across positions in the birth sequence but do not capture the time elapsed between siblings' births. We therefore restrict the sample to a relatively narrow window around enactment, limiting the temporal distance between siblings' births and hence the scope for gradual, differential assimilation to influence our estimates. The baseline window balances this consideration against the need to retain sufficient pre-treatment years to assess differential trends and a sufficiently large sample for statistical power. In Section 5.2, we consider alternative birth-year windows and find that the results remain robust.

The second consideration concerns exposure to other historical events that may have affected naming decisions differentially across immigrant groups. Because the 1918–1923 sample period lies between the Barred Zone Act of 1917 and the Johnson-Reed Act of 1924, our specification separates the effects of the 1921 Act from these other restrictive policies during the early 1900s.[16] The 1918 start date also limits overlap with World War I, during which hostility toward German immigrants increased their assimilation (Fouka, 2019). In Figure A3, we further show that the results are robust to starting the sample in 1919 or 1920. In Figure A4, we present robustness checks that exclude households from Germany, Hungary, and Austria, which were allied during the war. Overall, our main analysis sample includes 820,998 children born in 1918–1923 from 327,957 households (Table 1).

Identification Our specification addresses two primary identification concerns. First, we include household fixed effects α_h to account for time-invariant differences in household characteristics between targeted and non-targeted families that may independently affect naming decisions. Naming

[16]The Immigration Act of 1924 changed the temporary quota system introduced in 1921 to a permanent basis and imposed more stringent limits, reducing the quota rate from 3 to 2 percent and changing the reference census from 1910 to 1890. The 1917 Act imposed a literacy test on immigrants over the age of 16 and prohibited most immigration from Asia by establishing the Asiatic Barred Zone. Because our sample begins in 1918, the pre-treatment period may partly reflect the effects of the 1917 Act. However, because every child in the sample was born after its enactment, household fixed effects absorb any potential effects of the 1917 Act that are constant across siblings. Our estimates should therefore be interpreted as the impact of the 1921 Act conditional on the restrictions already in place. Consistent with this, the placebo estimates in Table 3 indicate no significant differences between siblings born before and after 1917.

customs, religion, and parental socioeconomic characteristics may influence naming choices and differ systematically between targeted and non-targeted families, leading comparisons across households to conflate their influence with the effect of the Quota Act. The household fixed effects absorb all such time-invariant household characteristics, including the origin country itself and any latent parental taste for assimilation. Identification therefore rests exclusively on how naming practices change within a family before and after the Quota Act.

Second, birth-year fixed effects α_t absorb time trends and year-specific shocks to naming patterns that are common across all families. For example, the Americanization movement during 1910–1930 sought to promote immigrant assimilation through English-language instruction requirements and compulsory schooling laws (Lleras-Muney and Shertzer, 2015). To the extent that these policies induced common changes in naming decisions across immigrant families, their effects are absorbed by the inclusion of birth-year fixed effects.[17]

Taken together, the β_k coefficients capture differential changes in siblings' name Americanization across targeted and non-targeted households. The estimates are normalized relative to children born in 1921, such that β_{1923}, for example, measures how much more American the name of a child born in 1923 is relative to a sibling born in 1921 in targeted households, relative to the corresponding difference in non-targeted households. The identifying assumption is that conditional on family and birth-year fixed effects, name Americanization would have followed parallel trends across targeted and non-targeted households in the absence of the Quota Act. While this counterfactual is unobservable, the lack of pre-trends in Figure 5 supports the plausibility of this assumption. Moreover, placebo exercises in Section 5 that redefine the post-treatment period using alternative cutoff years from 1915 to 1920 yield small and statistically insignificant estimates, lending further support to the assumption that naming trends would have remained parallel absent the Quota Act.

Static Specification We also estimate the average effect over the post-Act period using the following equation:

$$y_{iht} = \alpha_h + \alpha_t + \beta Quota_h \times Post_t + \Gamma X_i + \epsilon_{iht}. \tag{10}$$

$Post_t$ is an indicator variable equal to one for the post-Quota period (1922 to 1923).[18] The coefficient β identifies the difference-in-differences estimate of the Quota Act, and represents the average within-family change in assimilation effort across targeted and non-targeted families. Having established the baseline results from Equation (9), we use the static specification to examine

[17] Other potential sources of common changes in naming patterns during this period include the First Red Scare of 1919–1920, the recession of 1920–1921, and secular shifts in the popularity of particular names.

[18] Because the Act was enacted in May 1921, some naming decisions in the reference cohort may have occurred after its passage. To the extent that parents began adjusting their naming choices immediately, our estimates may understate the full assimilation response.

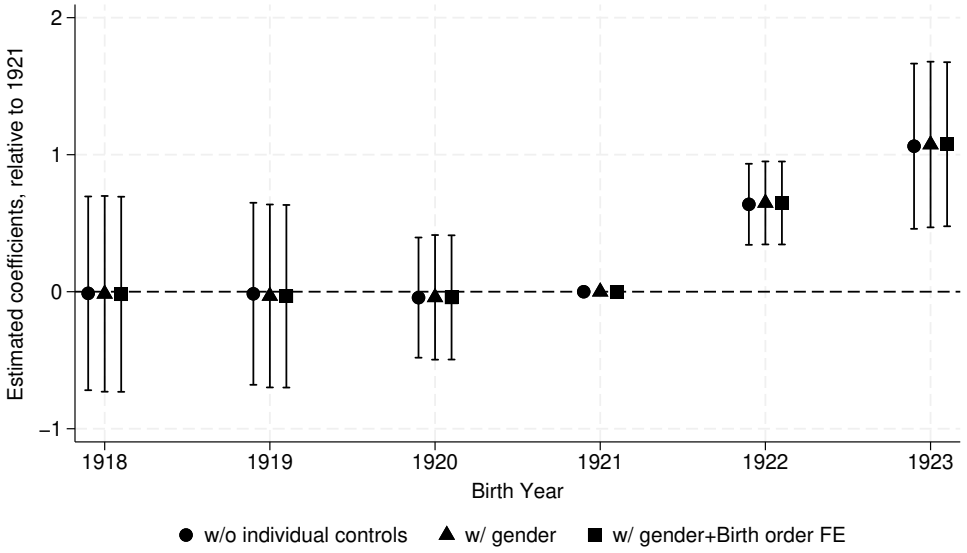
heterogeneity across subsamples and to conduct analyses of additional outcomes.

5 The Quota Act and Immigrant Assimilation

5.1 Baseline Results

The Emergency Quota Act increased assimilation effort among immigrant families from targeted countries. Figure 5 reports the estimates from Equation (9) across three specifications: without individual controls, with a control for child gender, and adding birth-order fixed effects. Before 1921, the year of the Quota Act, the coefficients are close to zero, indicating no differential trends in name Americanization between targeted and non-targeted families in years prior to the Quota Act. After 1921, the coefficients become positive and statistically significant: parents from targeted countries give their children more American names relative to their older siblings, compared to parents from non-targeted countries. This pattern, along with the lack of pre-trends, suggests that the Quota Act induced parents to increase assimilation effort.

Figure 5: Effect of the 1921 Emergency Quota Act on Immigrant Assimilation



Notes: This figure plots the estimates of β_k from Equation (9). The sample consists of children born in the United States between 1918 and 1923 to foreign-born parents. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. The y-axis shows the coefficient on the interaction between birth year and an indicator for the father being born in a country targeted by the Emergency Quota Act. All specifications include household and birth-year fixed effects, with child gender and birth-order fixed effects added as indicated. Confidence intervals are based on standard errors clustered by father's country of birth.

The estimates change little when we control for child gender and birth order. Birth order is

particularly relevant in our setting because children born after the Act are necessarily later-born, and naming conventions may differ across positions in the birth sequence.[19] However, including birth-order fixed effects leaves the results largely unchanged.

Table 2 reports the difference-in-differences estimates from Equation (10). The results indicate a 0.88-point increase in the American name index following the Emergency Quota Act. Relative to the mean American name index of 45.75 among the children of untreated fathers, the effect amounts to roughly a two percent or 0.04 standard deviation increase. This effect corresponds to approximately two and a half years of assimilation associated with residence in the United States among immigrants during the same historical period (Abramitzky et al., 2020).[20]

Table 2: Effect of the 1921 Emergency Quota Act on Immigrant Assimilation

Dep. var.: American name index			
	(1)	(2)	(3)
Quota × Post	0.865***	0.880***	0.882***
	(0.323)	(0.327)	(0.324)
R-squared	0.51	0.52	0.52
Observations	820998	820998	820998
Household fixed effects	✓	✓	✓
Birth year fixed effects	✓	✓	✓
Child gender		✓	✓
Birth order fixed effects			✓

Notes: Each column reports estimates from Equation (10). The sample consists of children born in the United States between 1918 and 1923 whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include household and birth-year fixed effects. The mean ANI for children with fathers from non-targeted countries is 45.75 (SD = 24.01). Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

In addition to the flat pre-Quota coefficients, two other pieces of evidence alleviate concerns about pre-trends. First, we conduct a sensitivity analysis following Rambachan and Roth (2023). Rather than requiring exact parallel trends, this approach allows differential pre-trends between

[19] For example, if later-born children were less likely to receive ancestral names, the positive coefficients post-Quota Act could arise even without a causal effect of the Act itself.

[20] Abramitzky et al. (2020) report a 7.2 point decline in name foreignness for immigrant children over 20 years of maternal residence when using a birth-cohort-specific name foreignness index. Their estimate comes from comparing siblings using mother, birth order, and birth year fixed effects (in 5 year bands).

treated and control households to continue after the Quota and tests whether the estimated effect remains significant. The key parameter $\bar{M}$ determines how large these post-Quota deviations from parallel trends can be relative to those in the pre-Quota period.[21] Given our three pre-Quota and two post-Quota periods, $\bar{M} = 1$ is a natural benchmark (Rambachan and Roth, 2023), which allows the violation of parallel trends after the Quota to be as large as the maximum violation before it. In our preferred specification, which controls for child gender and birth-order fixed effects, the 95% confidence set is [0.482, 1.242] under exact parallel trends. Relaxing the assumption to $\bar{M} = 1$ yields a robust confidence set of [0.164, 1.568], which still excludes zero. This suggests that the effect survives even if confounding shocks after the Quota were as large as the largest movement observed in the pre-Quota estimates.

Table 3: Effect of the 1921 Emergency Quota Act: Placebo Estimates

Dep. var.: American name index							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Placebo year	[1915]	[1916]	[1917]	[1918]	[1919]	[1920]	[1921]
Quota × Post	0.042	0.137	-0.010	-0.175	-0.184	0.280	0.768***
	(0.224)	(0.255)	(0.251)	(0.293)	(0.253)	(0.342)	(0.185)
R-squared	0.57	0.57	0.57	0.58	0.58	0.58	0.59
Observations	270347	270913	285575	250602	267636	221328	230003
Child gender	✓	✓	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓	✓	✓

Notes: Each column re-estimates Equation (10) using the year indicated in brackets as a placebo date for 1921, when the Emergency Quota Act was enacted. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for the year immediately following the placebo date. The sample in each column consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before and one born after the placebo date. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Second, we conduct placebo tests by re-estimating Equation (10) for a sequence of alternative treatment years, following the sample-construction rule described in Section 4. To avoid post-treatment periods overlapping across different placebo tests, we trim one pre- and one post-treatment

[21] Specifically, $\bar{M}$ scales the largest change in the treated–control gap between adjacent pre-Quota periods. $\bar{M} = 0$ corresponds to exact parallel trends, $\bar{M} = 0.5$ allows post-Quota deviations up to half of this change, and $\bar{M} = 2$ allows deviations up to twice as large.

period from the baseline sample window, which leaves a single post-treatment year in each case. If the post-Quota increase in the American name index reflects existing trends specific to immigrants from Quota-affected countries, comparable patterns should also appear for these placebo treatment dates. However, Table 3 indicates that the coefficients are small and statistically insignificant for all placebo treatment years (Columns 1–6), with a significant positive coefficient observed only for the baseline treatment period (Column 7). These results reinforce the interpretation that the rise in assimilation effort is a response to the 1921 Emergency Quota Act, rather than a consequence of pre-existing differential trends.

5.2 Robustness Checks

This section provides a series of robustness checks to address potential concerns about our identification strategy and interpretation of the results. The corresponding estimates are reported in Appendix A.

Definition of Treatment Status The baseline specification defines treatment status by the father's country of origin. Along with the baseline estimates, Figure A1 reports estimates that instead assign treatment by the mother's country of birth, and by whether either parent was born in a Quota-affected country. The estimates are nearly identical across the three definitions, largely reflecting the prevalence of ethnic endogamy among immigrant households during this period.[22]

Group-specific Birth-order Effects Because our identification relies on variation between siblings born before and after 1921, a treated child is necessarily of higher birth order than a pre-Quota sibling. While we include birth-order fixed effects that absorb any differential naming patterns by birth order, there could still be concerns about group-specific birth-order effects. For example, Greek families traditionally named the first son after the paternal grandfather and the second after the maternal grandfather (Herzfeld, 1982), leaving later-born children subject to weaker naming constraints. If such conventions were specific to the treated group, the American name index would rise mechanically with birth order among treated households. Figure A2 addresses this concern by interacting the birth-order fixed effects with treatment status and with origin-country dummies, respectively. The estimates are robust to these alternative birth-order interactions, suggesting that group-specific differences in naming practices by birth order do not influence our findings.

Alternative Sample Windows Abramitzky et al. (2020) document that immigrant parents' naming choices become progressively more American as their time in the United States increases. If the

[22] In our analysis sample, 91% of household heads share the same birth country as their spouses and 98% share the same treatment status.

relationship between length of residence and name Americanization were stronger among treated parents, later-born children in treated households could receive more American names even in the absence of the Quota Act. This pattern predicts larger estimated effects as the sample expands to include siblings born further apart. To mitigate this concern, Figure A3 replicates the estimates under three alternative restrictions to the sample window: 1918–1924, 1919–1923, and 1920–1922. All three alternative sample windows estimate a positive effect of the Quota Act on name Americanization, with no statistically significant pre-trends. In the smallest sample window from 1920 to 1922, with only one pre- and one post-Quota cohort, we still estimate a positive post-Quota coefficient, suggesting that our findings are not attributable to anticipation of the 1924 Johnson-Reed Act, which made the quota system more restrictive and permanent.

Exclusion of World War I Enemy Countries Because our sample window begins in 1918, it overlaps only briefly with the World War I period, during which hostility toward German immigrants increased their assimilation (Fouka, 2019). Nevertheless, if this wartime effect on immigrants from enemy countries persisted, it could confound the baseline estimates. Figure A4 presents estimates from two restricted samples. The first excludes households from Germany, and the second also excludes those from Austria and Hungary, the Central Powers with which the United States was at war. In both samples, the coefficients for cohorts born before 1921 are close to zero and statistically insignificant, whereas the post-Quota coefficients remain positive and significant.

Exclusion of Southern, Central, and Eastern Europe The Quota Act coincided with several events that generated hostility toward particular origin groups, raising the concern that our estimates reflect assimilation efforts related to these shocks rather than the Quota Act. For instance, the arrest of Sacco and Vanzetti in 1920 and their conviction in July 1921 produced a salient wave of anti-Italian sentiment (Frankfurter, 1927). The Palmer Raids and the postwar Red Scare associated immigrants from Russia and Eastern Europe with Bolshevism (Higham, 2002). Also, the second Ku Klux Klan expanded rapidly between 1921 and 1925, directing its nativism primarily at Catholics and Jews (Gordon, 2017). To assess the sensitivity of our estimates to these concurrent sources of anti-immigrant hostility, Figure A5 re-estimates the baseline specification in Equation (9), while excluding households from Southern, Central, and Eastern Europe in turn, where Italy and Russia account for 82.1% and 75.2% of households from Southern Europe and Eastern Europe, respectively. The estimates remain robust. Considering that Southern and Central European origins are predominantly Catholic while Eastern European origins account for most Jewish immigrants in our sample, these results also mitigate concerns that the expansion of the Klan confounds our findings.

Exclusion of Western Europe Interpreting our estimates as the effect of the Quota Act on households from targeted countries requires that naming choices in the comparison group were unaffected by the Quota Act. This assumption may be less plausible for Western European households, whose origin countries were also subject to quotas, albeit generally less binding (Figure 4). In addition, several of these origin countries (notably Ireland, Belgium, France, and Luxembourg) were predominantly Catholic and therefore exposed to the hostility that the second Klan directed at Catholics (Gordon, 2017). Figure A6 addresses this concern by dropping households from Western Europe (the UK, Ireland, Germany, the Netherlands, Switzerland, Belgium, France, and Luxembourg) from the comparison group, based on the birth country of the father, the mother, or either parent. The estimates are close to zero before 1921 and rise afterward across the three definitions, consistent with the baseline results.

Expected Duration of Residence The Quota Act may have impacted assimilation through alternative channels outside of the community-size and hostility channels outlined in our framework. One such channel is the impact of the Quota Act on immigrants' expected duration of residence in the United States. Repeated transatlantic migration was common before World War I. Keeling (2011) estimates that 19% of westward migrant crossings between 1900 and 1914 involved individuals who had previously traveled to the United States. Restrictions on subsequent re-entry could therefore discourage departures and increase the value of choosing American names for children expected to grow up in the United States. Consistent with the possibility that quotas altered settlement decisions, Greenwood and Ward (2015) find that tighter immigration restrictions during the 1920s reduced emigration and lengthened immigrants' stays in the United States. By increasing immigrants' expected duration of stay in the United States, the Act may therefore have strengthened their incentives to assimilate, independent of community-size and hostility.

Two considerations suggest that this channel is unlikely to explain our findings. First, the 1921 Quota Act preserved a route to re-entry for existing residents, allowing otherwise-admissible aliens returning from temporary visits abroad to enter even after their nationality's monthly or annual quota had been reached (42 Stat. 5–6, §2(d)). Second, if existing immigrants were unaware of such provisions in the Quota Act, then the channel implies a weaker response among households whose head had already naturalized, since their re-entry was not subject to the quota and their expected permanence in the country was therefore unaffected by the Quota Act. We test for this channel in Figure A7 by splitting the treated households into three groups based on the father's citizenship status: naturalized citizens, non-citizens who had filed a declaration of intention, and other non-citizens. The estimates for naturalized households are no weaker than the baseline, providing evidence against expected duration of residence as the principal explanation for our findings.

Estimates by Occupation The restrictions on immigrants from targeted countries were largely based on their portrayal as a source of unskilled labor (Jenks and Lauck, 1912). In our analysis sample, 54% of fathers in targeted households are semi-skilled manual and production workers (operatives) or non-farm laborers, compared with 36% in non-targeted households. This raises the concern that our estimates may capture a differential response to the Quota Act by skill group rather than by country of origin. Considering that name Americanization may carry labor market returns (Goldstein and Stecklov, 2016; Biavaschi et al., 2017), any shock around 1921 that worsened the prospects of low-skill workers (such as the 1920–1921 recession) could have strengthened their incentive to give their children American names. To address this possibility, we split targeted households at the median of paternal occupational income scores and re-estimate Equation (9) separately for each group. Figure A8 presents nearly identical estimates for the two groups, indicating that the assimilation response is not tied to parental occupational status.

Estimates by Industry A related concern involves industry composition. Immigrants from Southern, Central, and Eastern Europe were heavily concentrated in mining and manufacturing (Jenks and Lauck, 1912; Hourwich, 1912). Our sample shows a consistent pattern, with 47% of fathers in targeted households employed in these two sectors compared with 28% in non-targeted households. These sectors were subject to distinct shocks during our sample period. The 1920–1921 recession reduced industrial employment, postwar labor conflict in steel and coal was widely attributed to foreign-born workers, and large industrial employers ran Americanization programs for their immigrant workforce (Higham, 2002). Labor market adjustments to the Quota Act also varied across industries (Abramitzky et al., 2023). If naming choices responded to these conditions specific to mining and manufacturing, our estimates would be more pronounced for targeted households employed in these sectors. To test this possibility, we re-estimate Equation (9) separately for targeted households with fathers in mining or manufacturing and for those in other industries. Figure A9 shows that the results are similar for both groups, suggesting that sector-specific shocks do not drive our findings.

Alternative Outcome Variables Two potential concerns may arise regarding the ANI, our baseline outcome variable. First, because the ANI is a continuous index, the estimated effects may be sensitive to its distribution. For example, the results could be driven by shifts among a small number of names in the tails rather than a broad change in naming behavior. Second, the baseline ANI is calculated from name frequencies among the 21 birth cohorts preceding each child. This reference window includes recent birth years, some of which fall within our sample period. Naming patterns in these adjacent cohorts could be partly shaped by the same period-specific factors that affect the children we study, and this overlap might confound the interpretation of our results. To address

these concerns, Figure A10 presents estimates using three alternative outcome variables. The first is the percentile rank of the ANI, which depends only on the relative position of a name in the distribution. The second is a binary indicator for whether the ANI of a child is greater than the sample median. The indicator is scaled to take values of 0 or 100 so that coefficient magnitudes are comparable across outcomes. The third is an ANI constructed from name frequencies in a fixed reference period (1890–1910). As this window ends eight years before the earliest birth year in our sample, it excludes cohorts adjacent to the children we study. The results remain robust across all specifications.

Standard Errors Our baseline inference uses standard errors clustered at the father's country of birth, the level at which treatment status is defined. Figure A11 reports our event-study coefficients with confidence intervals from three alternative methods. The first reports confidence intervals computed from two-way clustered standard errors by birth country and year of immigration, which allows for arbitrary correlation in assimilation trajectories among households that arrived in the same year across all origins. The second uses two-way clustering by birth country and county of residence, which allows for arbitrary within-county correlation across origins. The third uses the wild cluster bootstrap, clustering by birth country across 57 clusters and using Webb weights with 9,999 replications. This provides inference that is robust to the potential imbalance in cluster sizes across birth countries (MacKinnon and Webb, 2017; Webb, 2023). The confidence intervals are nearly identical across these methods, indicating that our inference is robust to alternative approaches to accounting for dependence in the errors.

6 Mechanisms

This section presents two sets of empirical analyses that shed light on the mechanisms underlying our findings. First, based on the model predictions in Section 3, we examine heterogeneity in the effects of the Quota shock by the pre-existing prevalence of the immigrant community. Second, we investigate patterns of selective migration and their association with our findings, which provide supplementary evidence on the strategic behavior of immigrants.

6.1 Heterogeneity Analysis

Prediction 2 of our conceptual framework offers a way to distinguish the two mechanisms through which the Quota Act may have influenced assimilation effort. If the community-size channel dominates, the effect of the Quota Act should attenuate with the local prevalence of the Quota-origin population, since a larger resident stock buffers the decline in new arrivals and decreases the relative returns to assimilation. If the hostility channel dominates, the effect of the Quota Act should instead

increase with the prevalence of the local Quota-origin population, as greater visibility would amplify the consequences of retaining one's ethnic identity and increase the relative returns to assimilation.

To evaluate this prediction, Table 4 examines whether our findings vary with the prevalence of the local Quota-origin population. Specifically, we calculate the county-level share of the population born in Quota-affected countries (the county-level Quota-origin share) using the 1920 Census and split the baseline sample at the median share. Column 1 shows the baseline estimate for comparison, and Columns 2 and 3 report the estimates for the below- and above-median samples, respectively.[23] The results indicate that the increase in name Americanization following the Quota Act is more pronounced in counties that had a *lower* prevalence of the Quota-origin population. In contrast, the effect is smaller in magnitude and statistically insignificant in the above-median counties. This pattern suggests a primary role for the community-size channel: individuals embedded in a larger Quota-origin population were more insulated from the Quota Act.

Table 4: Heterogeneity by Quota-Origin Prevalence

Dep. var.: American name index			
	(1)	(2)	(3)
	All	Low	High
Quota × Post	0.885***	1.088***	0.400
	(0.324)	(0.322)	(0.386)
R-squared	0.52	0.53	0.50
Observations	820741	427895	392846
Child gender	✓	✓	✓
Household fixed effects	✓	✓	✓
Birth year fixed effects	✓	✓	✓
Birth order fixed effects	✓	✓	✓

Notes: Each column estimates Equation (10). Column 1 reports the baseline estimate, while Columns 2 and 3 split the sample at the median 1920 county population share of immigrants from targeted countries, with Low and High denoting below- and above-median counties, respectively. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. The sample consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

[23] The sample size and coefficient in Column 1 differ slightly from those of the baseline specification in Column 3 of Table 2 because counties not observed in the 1920 census are excluded from the estimation.

While these results are consistent with the community-size channel, their interpretation warrants further scrutiny in two respects. First, the county-level share of Quota-origin immigrants is correlated with the overall immigrant share. The community-size mechanism predicts that a larger existing population buffers the decline in new arrivals from origins whose inflows were restricted by the Quota Act. A larger immigrant population in general need not provide the same buffer, since inflows from non-targeted countries were already below their quotas and therefore were not constrained by the Act. The heterogeneity in Table 4 could therefore reflect broader features of immigrant-dense counties, such as fewer interactions with natives and consequently weaker incentives to respond through assimilation. We thus examine whether the attenuation of the naming response is specifically associated with the local prevalence of targeted immigrants, rather than immigrant prevalence more generally.

Second, the relevant reference group requires careful interpretation. In our framework, assimilation effort depends on the benefits of integrating into the mainstream community, relative to those of retaining an immigrant identity, and the latter increases with the size of the group to which an individual belongs. This link to group size may reflect either (i) the greater benefits that a larger co-ethnic network offers to those who retain their identity, or (ii) the broader social environment sustained by a larger targeted-group population, in which retaining a non-mainstream identity remains common and viable. In this view, the reference group can be defined either narrowly, as people from the same country of origin, or more broadly, as immigrants from restricted origins as a whole. The two need not coincide, as counties with similar shares of immigrants from Quota-affected countries may differ in their composition of origin countries. Distinguishing between the two would help identify the dimensions of the immigrant community that shaped immigrants' responses.

To examine which margin of the immigrant community drives the heterogeneity, we estimate the following triple-interaction specification:

$$y_{ihct} = \alpha_h + \alpha_{ct} + \beta \left(Quota_h \times Post_t \right) + \gamma \left(Quota_h \times Post_t \times Z_c \right) + \Gamma X_i + \epsilon_{ihct}, \qquad (11)$$

where y_{ihct} additionally indexes the county c in which child i is observed, and α_{ct} denotes county-by-birth-year fixed effects. Z_c denotes a county-level measure of the local immigrant community constructed using the 1920 Census. We separately consider the immigrant share of the population (Immigrant), the share of immigrants from countries targeted by the Quota (Treated), and the share of the Quota-origin population from an individual's own country of origin (Co-national). The first two variables (Immigrant and Treated) are measured at the county level, so their interactions with $Post_t$ are absorbed by the county-by-birth-year fixed effects. In contrast, because Co-national is measured at the origin-by-county level, we control for its interaction with $Post_t$ in the corresponding specification. The coefficient of interest on the triple-interaction (γ) captures how the effect of the

Quota Act varies with our measures of the local immigrant community size.

Table 5: Sources of Heterogeneity: Triple Difference Estimates

Dep. var.: American name index				
	(1)	(2)	(3)	(4)
Quota × Post	0.957***	1.537***	0.879***	1.537***
	(0.301)	(0.377)	(0.281)	(0.417)
Quota × Post × Immigrant	-0.293			0.050
	(0.837)			(0.861)
Quota × Post × Treated		-1.183***		-1.192***
		(0.335)		(0.330)
Quota × Post × Co-national			-0.122	-0.182
			(0.508)	(0.545)
R-squared	0.52	0.52	0.52	0.52
Observations	818383	818383	818383	818383
Post × Co-national			✓	✓
Child gender	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓
County×Birth-year fixed effects	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (11). The Immigrant variable indicates the immigrant-to-population ratio, while the Treated variable measures the share of the population born in Quota-targeted countries among immigrants. The Co-national variable is the share of the population from the same origin country among Quota-targeted immigrants. All three measures are constructed using the 1920 Census. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. The sample consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. All specifications include child gender, household fixed effects, birth-order fixed effects, and county-by-birth-year fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table 5 reports the estimates of Equation (11). Column 1 shows that the coefficient on the triple interaction with Immigrant is small and statistically insignificant. In contrast, Column 2 reports a strong negative coefficient of the interaction with the local prevalence of the treated group. Columns 1 and 2 suggest that the differential assimilation responses in Table 4 reflect the concentration of immigrants from targeted countries within the immigrant community at large, rather than the prevalence of immigrants in general. Column 3 examines the narrower margin of

national origin. Given that the Co-national variable is defined only for households from countries targeted by the Quota Act, the coefficient on Quota×Post×Co-national captures differences in treatment effects across Quota-affected families depending on their co-national concentration in the locality. The coefficient is again small and statistically insignificant, providing little evidence that co-national concentration plays an additional role in shaping the assimilation response. This pattern is consistent with the broader Quota-origin population serving as the relevant reference group, rather than narrowly defined co-ethnic enclaves.[24] The horse-race specification in Column 4 reinforces this interpretation, as the interaction with the targeted immigrant share remains negative and statistically significant, whereas the other interactions are statistically insignificant.[25]

The boundary of the operative reference group is informative about the nature of the community-size channel. Among immigrants from Quota-targeted countries, the heterogeneity occurs at the level of restricted origins rather than at the co-national margin. This suggests that the value of a larger immigrant community was not primarily origin-specific. Had the buffering operated mainly through co-ethnic networks, such as shared language, kinship ties, and origin-country institutions, the response would have varied with local co-national concentration. Instead, the heterogeneity depends on the local concentration of the restricted population as a whole. This pattern is more consistent with community values shared across restricted origins than with the benefits of a specific co-ethnic network. These values would arise from the local environment Quota-origin immigrants held in common, such as immigrant neighborhoods and workplaces, or from the social norms sustained by their collective presence. Where they made up a larger share of the local population, maintaining a non-mainstream identity remained viable despite the decline in new arrivals, weakening the incentive to assimilate. Building on the extensive literature on the returns to co-ethnic networks (Munshi, 2003; Damm, 2009; Carneiro et al., 2020; Schüller and Chakraborty, 2022), this indicates that the community relevant to assimilation decisions can extend beyond ethnic boundaries, so that a policy targeting specific origins can shape the responses of immigrants at the level of the targeted population as a whole.

6.2 Selective Migration

The heterogeneity analysis indicates that the assimilation response varied with the local prevalence of the targeted immigrant population. If immigrants considered the size of the local Quota-origin

[24] Since co-nationals are counted within the Treated variable, this does not imply that co-national ties were irrelevant, but rather that they carried no additional weight beyond inclusion in the targeted group.

[25] A remaining concern is that the local concentration of the Quota-origin population may have been correlated with other county characteristics, such as the prevalence of low-skill occupations or employment in mining and manufacturing. These factors could have directly influenced assimilation decisions through differences in the socioeconomic environment faced by Quota-origin immigrants. Table A5 addresses this concern, showing that the results are robust to controlling for heterogeneity by these alternative characteristics.

community an important factor in their responses, relocation would provide another margin along which they could adjust to the shock. A large literature documents that mobility serves as a margin of adjustment to changes in demographic, political, and socioeconomic conditions (Boustan, 2010; Abramitzky et al., 2019), with immigrants in particular exhibiting stronger migration responses to changes in these conditions (Bartel, 1989; Edin et al., 2003; Stuart and Taylor, 2021; Jia et al., 2023). The destinations chosen by immigrants who moved after 1921 may therefore provide additional evidence on the mechanisms underlying their assimilation responses to the Quota Act. Under the community-size channel, households from Quota-affected origins would favor destinations with larger existing Quota-origin populations, where the benefits of retaining an immigrant identity are better sustained despite the decline in new arrivals. Under the hostility channel, these households would instead favor destinations with less nativist hostility, where retaining a distinct identity entails lower costs. Migration may thus provide an alternative to name Americanization, either by preserving access to an immigrant community or by reducing exposure to anti-immigrant hostility.

To evaluate these predictions, we examine the destination choices of immigrant households that relocated after the Quota Act. Since we do not observe the date of migration in the census data, we define movers through linkages between the full-count 1920 and 1930 Censuses from the IPUMS Multigenerational Longitudinal Panel (Ruggles et al., 2025) as immigrants whose county of residence changed between the two census years. This definition is imperfect in two respects: it includes households that moved before the legislation (1920), and households that may have moved for reasons unrelated to the Quota Act. Nevertheless, unless their destinations were systematically associated with the prevalence of Quota-origin immigrants or nativist hostility, their inclusion attenuates the estimated effects rather than generating spurious patterns. For consistency, we only examine immigrant households from the baseline analysis sample.[26]

Table 6 presents the results on migration selectivity, using the following household-level cross-sectional specification:

$$y_h = \alpha_c + \beta Quota_h + X_h'\lambda + \epsilon_h. \tag{12}$$

We examine two dependent variables (y_h) measured at the destination county (county of residence in 1930) to test for the prevalence of our hypothesized channels. To test for the community-size channel, we use the 1920 population share of immigrants from Quota-affected countries (Columns 1–2). To test for the hostility channel, we use the yea status of the local legislator's roll-call vote on the Emergency Quota Act (Columns 3–4) as a proxy for anti-immigrant sentiment in the locality.[27]

[26]The results on migration selectivity are robust to extending the sample to all migrating households (Table A9).

[27]We use the House roll-call vote from Lewis et al. (2019) on H.R. 4075, the bill enacted as the Emergency Quota Act, and code both yea and paired-yea votes as support for the Quota Act. While roll-call votes may imperfectly capture the hostility immigrant households encountered, prior evidence links restrictionist voting to local economic and demographic conditions (Goldin, 1994) and immigration-induced backlash (Tabellini, 2020), supporting their

Because the vote is recorded at the congressional district level, we assign votes to counties using an area-based crosswalk between counties and the districts of the 67th Congress, assigning each county the area-weighted average of the yea indicators of the overlapping districts. A county is coded as supporting the Act if this average exceeds one half. The explanatory variable, $Quota_h$, is an indicator for whether the household head was born in a country targeted by the Quota Act. We include a full set of origin (1920) county fixed effects α_c, and a vector of household-level controls X_h that includes the birth year, race, naturalization status, and year of immigration for the mother and father. The coefficient β therefore captures the differential pattern of destination choices between immigrant households from countries targeted by the Quota and households from non-targeted countries. By including origin-county fixed effects, we compare destination choices across households that lived in the same county in 1920, which allows us to interpret the estimates as differences in the direction of migration.

Table 6: Migration Selectivity by Quota Status of Origin Country

Dep. var.: Destination county characteristics				
	(1)	(2)	(3)	(4)
	Prevalence of Quota-immigrants		Anti-immigration sentiment	
Quota	0.021***	0.022***	-0.017	-0.017
	(0.003)	(0.003)	(0.021)	(0.019)
Outcome mean	0.085	0.085	0.605	0.605
R-squared	0.41	0.42	0.22	0.22
Observations	38726	38711	31973	31959
Origin-county fixed effects	✓	✓	✓	✓
Household controls		✓		✓

Notes: Each column reports estimates from Equation (12). The sample consists of immigrant households that changed counties between 1920 and 1930. The dependent variables are characteristics of the household's destination county. In Columns 1–2, the outcome is the 1920 share of immigrants from targeted countries; in Columns 3–4, the outcome is an indicator equal to one if the county supported the Emergency Quota Act, based on the local legislators' roll-call votes. Quota is an indicator for the household head being born in a country targeted by the Quota Act. All specifications include 1920 origin-county fixed effects. Household controls include race, birth year, citizenship status, and year of immigration, all measured for both the father and the mother. Standard errors are clustered by father's country of birth. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The estimates in Columns 1–2 are positive and statistically significant. Immigrant households

use as a proxy for local nativist sentiment. Because counties supporting and opposing the Act may also differ along other dimensions, we interpret the contrast between yea and nay counties as descriptive, acknowledging these potential limitations.

from countries targeted by the Quota Act moved to counties where the Quota-targeted population share was 2.1–2.2 percentage points higher than in the destinations chosen by non-Quota-targeted immigrant movers, against a mean of 8.5%. In contrast, we do not find a similar pattern related to anti-immigrant sentiment in the destination counties. While the estimates in Columns 3–4 are negative, they are not statistically significant and the magnitudes are small: a 1.7 percentage point decrease relative to a mean of about 60.5%. Taken together, these patterns suggest that immigrant households that moved in response to the Quota Act selected destinations based on the prevalence of the targeted population, rather than on the intensity of local nativist hostility, reinforcing the role of the community-size channel in determining assimilation effort.[28]

Table 7: Effects of the Emergency Quota Act by Migration Status

Dep. var.: American name index		
	(1)	(2)
	Non-migrants	Migrants
Quota × Post	1.162***	0.658*
	(0.365)	(0.364)
R-squared	0.54	0.58
Observations	430649	233853
Child gender	✓	✓
Household fixed effects	✓	✓
Birth year fixed effects	✓	✓
Birth order fixed effects	✓	✓

Notes: Each column estimates Equation (10) separately by household migration status. Column 1 restricts the sample to households whose county of residence did not change between 1920 and 1930. Column 2 restricts the sample to households whose county of residence changed. Both samples consist of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

An implication of migration being an alternative strategic response is that parents who moved to areas with larger Quota-origin communities would have exhibited a smaller shift toward American

[28] Beyond destination choice, the extensive margin of migration also supports the community-size channel. Although Quota-origin households were not more likely to move than other immigrant households on average, they were significantly more (less) likely to relocate when the Quota-origin population share in their 1920 county of residence was lower (higher). Table A10 summarizes the results.

names for their children. To test this implication, in Table 7 we estimate the baseline specification from Section 5.1, separately for non-migrant and migrant households. Column 1 reports an estimated coefficient on name Americanization of 1.162 for non-migrants, which exceeds the baseline estimate in magnitude (0.882 in Table 2). In contrast, Column 2 reports an estimate of 0.658 for migrants, both smaller in magnitude and statistically weaker. This pattern is consistent with migration being a substitute for name Americanization. Households that moved toward larger concentrations of the targeted population had less need to adjust along the cultural margin of names given to children.[29] One possible concern is that the contrasting coefficients in Table 7 reflect pre-existing differences in naming trends between migrant and non-migrant households. To assess this possibility, Table A11 repeats the exercise using 1919 as a placebo shock year. This exercise examines whether a similar contrast was already present before the Act, as would be expected if the post-1921 pattern reflected a continuation of earlier differential trends. Neither the migrant nor the non-migrant sample exhibits an increase in name Americanization, and no contrast between the two groups appears. The placebo test results suggest that the post-Quota contrast was not simply an extension of earlier differences in naming trends.

Taken together, these findings provide additional support for the community-size channel as a key driver of assimilation effort in response to the Quota Act. Immigrants who moved after the Act sorted on the prevalence of the targeted population rather than on local hostility, and those who moved exhibited a weaker adjustment in naming practices. In addition, the larger estimated naming response among non-migrant households mitigates the concern that the baseline findings are driven by the selective migration of immigrant families.

7 Long-Term Consequences

The evidence so far indicates that immigrant parents changed their attitudes toward assimilation after the Quota Act, as reflected in the names given to their children. A natural question is whether these naming differences translated into differences in the children's later outcomes. Differences in naming do not necessarily imply that pre- and post-Quota siblings later assimilated to different degrees. Siblings grew up in the same household, and the shift in parental attitudes around 1921 shaped the upbringing of both for most of their childhood. Names may nonetheless matter in their own right. A name may shape perceived identity (Amit and Dolberg, 2023) or signal to the host society about whether its bearer belongs to the in-group (Zhao and Biernat, 2017), which may in turn affect socioeconomic outcomes (Goldstein and Stecklov, 2016; Biavaschi et al., 2017; Abramitzky

[29]The substitution between spatial and cultural adjustment aligns with theoretical models in which the cost of retaining a distinct identity depends on the local composition of the population (Bisin and Verdier, 2000; Bisin et al., 2016), and with empirical findings that migration decisions and investment in location-specific capital are jointly determined (Bauer et al., 2005; Adda et al., 2022).

et al., 2020). This section examines whether siblings born before and after the Act, who received differently Americanized names, differed in their adult outcomes.

Our estimation follows Equation (10), the static DiD specification from Section 4, but uses outcomes of child i observed later in life. Identification relies on the assumption that adult outcomes would have followed parallel trends between targeted and non-targeted households across birth cohorts in the absence of the Act, conditional on the included individual-level controls and household fixed effects. We interpret these estimates as suggestive evidence on whether the naming response was accompanied by differences in later-life outcomes, while remaining cautious about a causal interpretation, as linkage to later-life records reduces the sample size and limits the precision of pre-trend assessments.[30]

7.1 Economic Outcomes

We first examine differences in adult economic outcomes. American-sounding names may affect economic outcomes through two main channels. Externally, a name shapes how others in the host society perceive and treat individuals, which in turn affects their access to economic and social opportunities. Internally, a name shapes how individuals perceive themselves, strengthening their identification with the receiving country and shaping the choices that govern their integration into mainstream society. It should be noted that, because our estimates reflect the reduced form effects of the shift in parental attitudes toward assimilation, the evidence below speaks to which channel better fits the data rather than isolating the causal role of name Americanization.

We measure long-run outcomes using the 1950 Census, by which point all siblings in our sample had reached adulthood (aged 27 to 32). Because the 1950 Census collected income and education only for individuals on every fifth line of the enumeration form, both are rarely observed for more than one sibling in the same household. We therefore measure adult socioeconomic status using four occupation-based proxies: the occupational income score (OCCSCORE), the occupational education score (EDSCOR50), the Duncan socioeconomic index (SEI), and the Siegel prestige score (PRESGL) (Ruggles et al., 2024).[31] The sample consists of all sibling groups from the baseline sample with at least one sibling born pre-Quota Act and one born post, with non-missing occupations in 1950. Siblings are linked from the full-count 1930 Census to the full-count 1950

[30] For each long-term outcome of interest, the estimation sample requires a household to have at least one child born before and one born after the Quota Act, both successfully linked between the 1930 and 1950 Censuses and both with non-missing values for that outcome.

[31] EDSCOR50 is defined as the share of individuals in a respondent's occupational category who had completed at least one year of college. SEI assigns each occupation a score from a model relating occupational prestige ratings to proxies of occupational income and education, estimated using the 1947 National Opinion Research Center (NORC) survey and the 1950 Census. PRESGL is based on subjective prestige ratings from NORC surveys conducted during the 1960s. Their values are assigned based on the 1950 occupational classification and therefore represent alternative scalings of the identical underlying occupations rather than independent outcomes.

Census using the IPUMS Multigenerational Longitudinal Panel (Ruggles et al., 2025).

Table 8: Long-Term Effects on Occupational Socioeconomic Status

Dep. var.: occupational socioeconomic status scores	(1)	(2)	(3)	(4)	(5)
	Income score	Education score	Duncan SEI	Siegel prestige score	Mean
Quota × Post	0.021*	0.026**	0.018	0.027*	0.024*
	(0.012)	(0.012)	(0.013)	(0.015)	(0.012)
R-squared	0.63	0.64	0.66	0.60	0.63
Observations	91292	90316	91292	90316	90316
Individual controls	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10). The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. The dependent variables are z-standardized values of the occupational income score (OCCSCORE), occupational education score (EDSCOR50), Duncan socioeconomic index (SEI), and Siegel prestige score (PRESGL) in Columns 1–4, respectively, and their mean in Column 5. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. Individual controls include gender and one-digit industry fixed effects. All specifications include household, birth-year, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table 8 reports estimates using occupation-based scores as outcomes, standardized for ease of interpretation. Individual controls include gender and one-digit industry dummies, so that comparisons are made within sectors.[32] As shown in Columns 1–4, the coefficients are positive for all four measures, but they are small in magnitude and only marginally significant. Given that the dependent variables are standardized, the estimates imply that post-Quota siblings from Quota-origin households had 0.018–0.027 standard deviations higher occupational socioeconomic status than their pre-Quota siblings. Column 5 shows a similar pattern using the average of the standardized values of the four measures (i.e., average of the z-scores of OCCSCORE, EDSCOR50, SEI, and PRESGL). These modest magnitudes are consistent with the baseline estimates. In standardized terms, the long-run estimates are about one-half to three-quarters as large as the effect of the Quota Act on the American name index, likely reflecting the long time lag and other factors that shape

[32]Table A4 shows that our findings are robust to excluding the industry dummies, which addresses bad control concerns.

occupational outcomes. These results are also consistent with Abramitzky et al. (2020), who find few differences in the adult outcomes of brothers with more and less foreign-sounding names among the children of immigrants in the early twentieth century, suggesting that the negative association between ethnic names and adult outcomes in this era likely reflects household-level differences.

Table 9: Long-Term Effects on Occupational Socioeconomic Status: Right-Tail Outcomes

Dep. var.: Indicator for occupational category	(1)	(2)	(3)	(4)	(5)	(6)
		Top 10% SES index				
	Professional and manager	Income score	Education score	Duncan SEI	Siegel prestige score	Mean
Quota × Post	0.018***	0.013***	0.019***	0.016***	0.016***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
R-squared	0.59	0.57	0.60	0.59	0.58	0.59
Observations	91292	91292	90316	91292	90316	90316
Individual controls	✓	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10). The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. The dependent variables are: indicator for professional or managerial occupations (Column 1); indicator for the top 10 percent of the estimation sample distribution for each SES measure (Columns 2–5); and indicator for the top 10 percent of a composite SES index defined as the standardized average of the four standardized measures (Column 6). Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. Individual controls include gender and one-digit industry fixed effects. All specifications include household, birth-year, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

However, small average effects do not rule out economic consequences. If an American name mattered primarily by easing entry into positions from which immigrants were otherwise excluded, its effects could be concentrated in the upper tail of the occupational distribution. Table 9 examines this possibility by using indicators for holding a professional or managerial occupation and for ranking in the top decile of each occupation-based score as the outcome variables. According to Column 1, siblings born after the Quota Act in targeted households were more likely to hold professional or managerial occupations, while Columns 2 through 5 show that they were also more likely to rank in the top decile of each occupation-based score. Column 6 yields a similar pattern using the top decile indicator of the average of the z-scores of OCCSCORE, EDSCOR50, SEI, and

PRESGL.[33]

Because the base rate is 10% by construction, the estimates of 1.3 to 1.9 percentage points imply a 13 to 19% increase in the probability of reaching the top decile of socioeconomic status. These effects are several times larger than those implied by the mean estimates in Table 8. Assuming that the occupation-based scores are normally distributed, a shift of their distribution by 0.018–0.027 standard deviations would raise the probability of reaching the top decile by 0.3–0.5 percentage points.[34] Therefore, the results in Table 9 suggest that the gains were disproportionately concentrated in the upper tail. Consistent with this interpretation, Table A2 reports that the effects on raw occupation-based scores disappear when the sample excludes individuals at or above the 90th percentile of each outcome. Similarly, Table A1 shows that the effects are close to zero at the top 30 to 50 percent thresholds of the mean SES index but become positive at thresholds near the top of the distribution.

These results suggest a more nuanced reading of the conclusions in Abramitzky et al. (2020). Consistent with their evidence, differences between siblings are small at the mean of the occupational distribution. In the upper tail, however, we find that post-Quota siblings in targeted households, who on average received more Americanized names, were more likely to reach high-ranking occupations relative to their pre-Quota siblings. This pattern is consistent with names serving as a marker that restricts entry into a narrow set of positions, where entry may depend more heavily on perceptions of ethnic background and cultural fit. Such exclusion would be largely masked in mean outcomes.[35]

Taken together, these results help distinguish between the external and internal channels. If the internal channel were the main driver, a stronger identification with the receiving country would

[33]Figure A12 presents a dynamic version of these results based on Equation (9). Although the period-specific estimates are noisier than the baseline results based on the American name index, the patterns are comparable. The pre-Quota coefficients are statistically insignificant, whereas the post-Quota coefficients become positive and significant. This result alleviates the concern that differences of a few years in exposure to the shift in parental attitudes toward assimilation could affect the adult economic outcomes of pre- and post-Quota siblings. If such differences mattered, we would expect increasing pre-trends, because siblings born farther from (closer to) 1921 would have been less (more) exposed to the changed parental attitudes.

[34]Let z denote a standardized occupation-based score, with $z \sim N(0,1)$. The top-decile threshold is then $z^* \approx 1.28$. If the entire distribution shifts upward by Δ standard deviations, the probability of exceeding z^* becomes $1-\Phi(z^*-\Delta)$, where $\Phi(\cdot)$ is the standard normal cumulative distribution function. The resulting increase in this probability is therefore $\Phi(z^*) - \Phi(z^* - \Delta) \approx \phi(z^*)\Delta \approx 0.175\Delta$, where $\phi(\cdot)$ is the standard normal density. Substituting the mean effects of $\Delta = 0.018$ and 0.027 from Table 8 yields increases of about 0.3 and 0.5 percentage points, respectively.

[35]A sharper test of the exclusion account would compare the occupational outcomes of siblings with similar educational attainment. This comparison is not feasible in our setting. The 1950 Census recorded education only for persons on every fifth line of the population schedule, so it is rarely observed for more than one sibling in the same household. Although the 1940 Census records years of schooling for all individuals, siblings in our sample were aged 17 to 22 in 1940. Some siblings were still in school, which prevents the observation of completed educational attainment, particularly for younger siblings. Nevertheless, existing evidence suggests that differences in schooling are unlikely to explain the upper-tail gap. Comparing brothers among the sons of immigrants linked between the 1920 and 1940 Censuses, Abramitzky et al. (2020) find that a 20-point increase in name foreignness is associated with about two fewer weeks of schooling. According to this estimate, the 0.88-point increase in the American name index in our baseline specification corresponds to a difference of less than one day of schooling.

shape a broad range of choices governing integration, and its effects would be expected to appear across the occupational distribution. The estimates do not show such a pattern. The mean effects are small and only marginally significant.[36] In contrast, the results suggest that Americanized names raise the likelihood of reaching the top of the occupational distribution. Given the weaker effects on mean outcomes, this pattern is consistent with the exclusion account, in which access to higher-ranked positions depends on selection and screening. In this sense, our findings are more consistent with an external channel, in which Americanized names influence how individuals are perceived and treated by others, than with an internal channel operating through changes in self-identification. To reinforce this interpretation, we next examine the long-term assimilation outcomes of the siblings.

7.2 Assimilation Outcomes

If the differences in socioeconomic outcomes operated mainly through the external channel, post-Quota siblings would not be expected to differ from their older siblings in the formation of American identity. To evaluate this prediction, we examine three proxies for adulthood assimilation. For each outcome, the sample consists of all sibling groups from the baseline sample with at least one sibling born pre-Quota Act and one born post, with the outcome of interest non-missing.

Table 10 summarizes the results. First, we examine spousal characteristics. Intermarriage is widely used as a measure of immigrant assimilation and ethnic identity (Bleakley and Chin, 2010; Fouka et al., 2022), as spousal choice reveals preferences over the cultural traits transmitted to the next generation through children (Bisin and Verdier, 2000; Bisin et al., 2004). Column 1 uses an indicator for a foreign-born spouse, and Column 2 uses the American name index of the spouse. Both estimates are close to zero and statistically insignificant, providing no evidence that post-Quota siblings differed from their pre-Quota siblings in spousal choice.

Second, Column 3 exploits the names that siblings gave to their own children. As described in Section 4.1.2, names chosen for children can be a marker of the parents' ethnic identity. If post-Quota siblings had formed a stronger American identity, it may be reflected in more American names for their children. However, the estimate indicates no significant difference.

Third, Column 4 examines voluntary enlistment in World War II, restricting the sample to

[36] The estimates in Table 8 fall within the range implied by the upper-tail shift in Table 9. The coefficient in Column 6 of Table 9 indicates that the Quota shock moved 1.9 percent of the sample into the top decile of the index. The implied effect on the mean is the product of this share and the size of the shift these individuals experienced. Because this shift is not observed, we bracket it by two extremes. The upper bound assumes that these individuals came from the average of the remaining ninety percent of the distribution, a distance of $\bar{z}_{>p_{90}} - \bar{z}_{\leq p_{90}}$ standard deviations, implying a mean effect of 0.042 standard deviations. The lower bound assumes that they were located immediately below the ninetieth percentile, a distance of $\bar{z}_{>p_{90}} - z_{90}$, indicating a mean effect of 0.008 standard deviations. The estimate of 0.024 in Column 5 of Table 8 lies between these bounds, suggesting that the observed mean effects are consistent with the right-tail results.

male siblings. The siblings in our sample were born between 1918 and 1923 and were therefore of military age during the war. Voluntary enlistment during World War II has been used as a measure of allegiance to the United States and of support for the government (Qian and Tabellini, 2026). If post-Quota siblings perceived themselves more strongly as American, they may have been more likely to enlist voluntarily. The estimate, however, is again close to zero and statistically insignificant.[37]

Table 10: Long-Term Effects: Adulthood Assimilation Outcomes

Dep. var.: long-term assimilation proxies				
	(1)	(2)	(3)	(4)
	Spouse		Child	WWII
	immigrant	ANI	ANI	volunteer
Quota × Post	0.006	0.192	0.110	-0.001
	(0.007)	(0.610)	(0.452)	(0.005)
R-squared	0.49	0.53	0.38	0.51
Observations	41068	22223	34837	71845
Gender	✓	✓	✓	
Household fixed effects	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10). The sample consists of siblings from the baseline sample linked to the 1950 Census, restricted to households with both pre- and post-Quota siblings linked and non-missing outcomes. The dependent variables are: an indicator for having a foreign-born spouse (Column 1); the American name index (ANI) of the spouse (Column 2); the ANI of the siblings' children (Column 3); and an indicator for voluntary enlistment in World War II, estimated for male siblings only (Column 4). ANI ranges from 0 to 100, with higher values indicating more American names. Quota indicates that the father was born in a country targeted by the Emergency Quota Act, and Post indicates births in 1922–1923. All specifications include household fixed effects, birth-year fixed effects, and birth-order fixed effects. Controls for gender are included in columns 1 to 3. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

These estimates require linking both pre- and post-Quota siblings to observed outcomes later in life, raising concerns about selection into the estimation sample. If individuals with a weaker naming response were more likely to be linked to the 1950 Census (for instance, because of more distinctive names), the absence of long-run effects on assimilation outcomes may simply reflect a

[37]The dependent variable is an indicator equal to one for voluntary enlistment, with both drafted service and non-service coded as zero. Restricting the sample to siblings who both served in World War II does not alter the results.

lack of naming differences between siblings in the linked sample. Table 11 addresses this concern by re-estimating the baseline specification on the estimation sample of each column in Table 10. The estimates range from 0.98 to 1.34 and are statistically significant in all columns, comparable to or larger than the baseline estimate in Table 2.

Table 11: Baseline Estimates Using the Long-Term Estimation Samples

Dep. var.: American name index				
	(1)	(2)	(3)	(4)
Sample	Spouse		Child	WWII
	immigrant	ANI	ANI	volunteer
Quota × Post	1.179***	1.338**	0.979*	1.306***
	(0.349)	(0.529)	(0.567)	(0.455)
R-squared	0.59	0.60	0.61	0.59
Observations	41068	22223	20564	71845
Gender	✓	✓	✓	
Household fixed effects	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10), restricting the sample to households in which both pre- and post-Quota siblings are linked to the 1950 Census and have non-missing values for the corresponding outcome. The dependent variable in all columns is the individual's ANI measured in the 1930 Census, ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include household fixed effects, birth-year fixed effects, and birth-order fixed effects. Controls for child gender are included in columns 1 to 3. Standard errors are clustered by father's country of birth. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Overall, we find no statistically significant differences between siblings in observable proxies for assimilation in adulthood. Because these proxies capture only a limited set of assimilation dimensions, the results cannot rule out broader unobserved effects. Nevertheless, the absence of detectable differences across three distinct dimensions—marriage, the naming of the next generation, and military service—provides little supporting evidence for the view that differences in adult assimilation account for the occupational results. This pattern is more consistent with an external channel, in which an American name mattered through the perceptions of others rather than through the self-identification of its bearer.

8 Conclusion

Immigration restrictions are designed to shape the number and composition of new arrivals. Yet they can also alter the incentives of immigrants who have already settled in the host country. Our study of the 1921 Emergency Quota Act highlights this less visible consequence of immigration restrictions. Although the Act did not directly regulate the cultural practices of existing immigrants, families from origin countries targeted by the Act began giving their children more Americanized names following its passage. Our exploration of mechanisms points to reduced expected growth of immigrant communities, rather than heightened anti-immigrant hostility, as the primary explanation for this response. Families were less responsive where immigrants from targeted origins were already more prevalent, while those who relocated tended to move toward larger communities and exhibited smaller changes in naming practices. The prospect of fewer future arrivals thus changed the cultural choices of immigrants already living in the United States.

In addition to their role as a proxy for parental assimilation attitudes, we investigate whether the changes in naming practices following the Quota Act were accompanied by differences in children's adult outcomes. We find that siblings born after the Act were more likely to reach the upper tail of the occupational distribution, despite showing little difference in their own assimilation outcomes. These long-run findings are more suggestive, given the limitations imposed by historical record linkages. Nevertheless, the results suggest that the economic advantages associated with more Americanized names need not reflect greater assimilation by their bearers. Instead, the concentration of gains in high-status occupations, without corresponding differences in adulthood assimilation, is more consistent with names shaping how individuals are perceived and treated by natives in the host country.

More broadly, our findings suggest that immigration restrictions can operate as an indirect form of cultural policy. By changing the expected growth of immigrant communities, governments may influence the transmission of cultural identity even without explicitly seeking to do so. However, whether such adjustments translate into broader integration depends not only on the assimilation choices of immigrants but also on how the receiving society responds to them. An important direction for future research is therefore to examine how the attitudes and behavior of natives evolve alongside those of incumbent immigrants, particularly amid renewed anti-immigrant sentiment across major destination countries.

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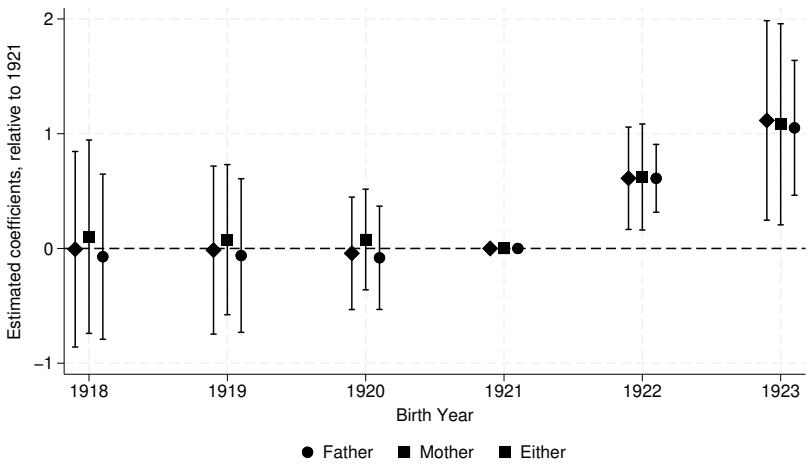
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Appendices

A Robustness Checks: Baseline Findings

Figure A1: Alternative Definitions of Treatment Status



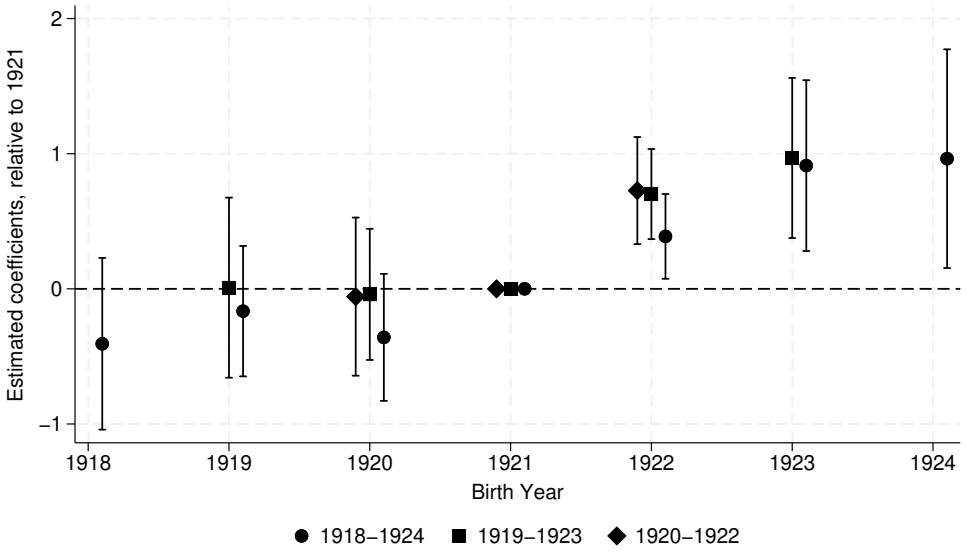
Notes: This figure plots estimates from Equation (9) using alternative definitions of treatment status. Treatment is defined based on whether the father, mother, or either parent was born in a country targeted by the Emergency Quota Act, as indicated. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by paternal and maternal country of birth in the first and second specification, respectively, and two-way clustered by both in the last specification.

Figure A2: Group-Specific Birth-Order Fixed Effects



Notes: This figure plots estimates from Equation (9), controlling for Quota-by-birth-order and origin-country-by-birth-order fixed effects, respectively, instead of birth-order fixed effects as in the baseline specification. All specifications include household and birth-year fixed effects and child gender. Standard errors are clustered by father's country of birth.

Figure A3: Alternative Sample Windows



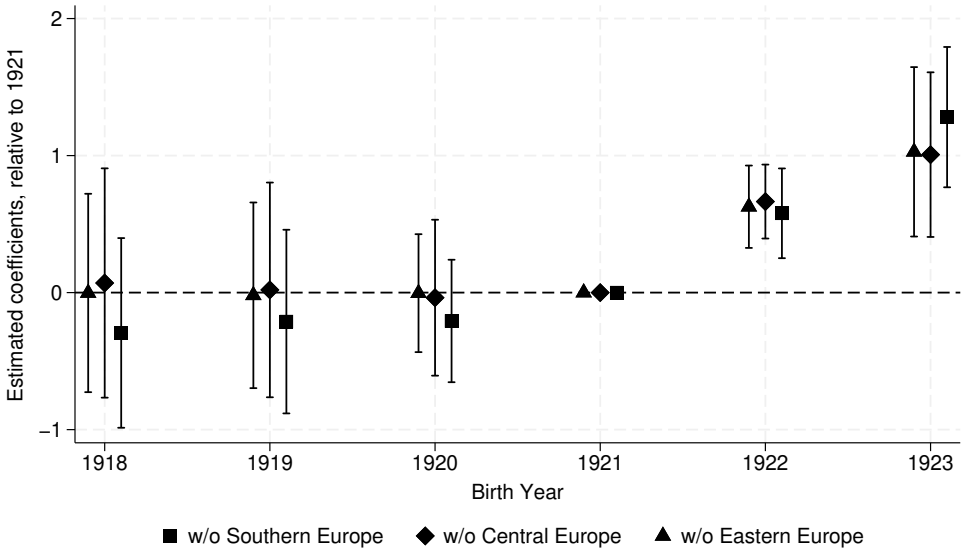
Notes: This figure plots estimates from Equation (9) using alternative sample windows around the Emergency Quota Act. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A4: Exclusion of Germany and Its World War I Allies



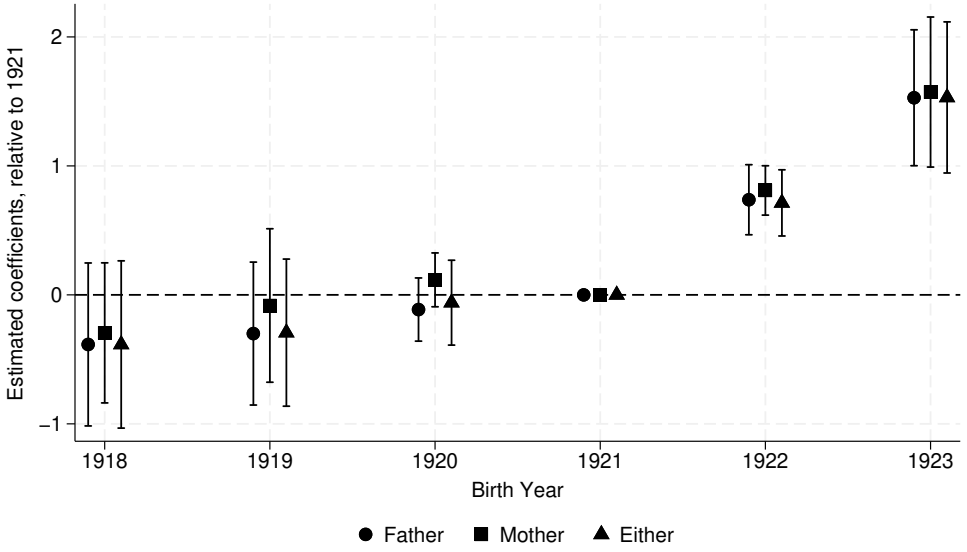
Notes: This figure plots estimates from Equation (9), first excluding Germany, and then excluding Germany, Austria, and Hungary from the sample. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A5: Exclusion of Southern, Central, and Eastern European Countries



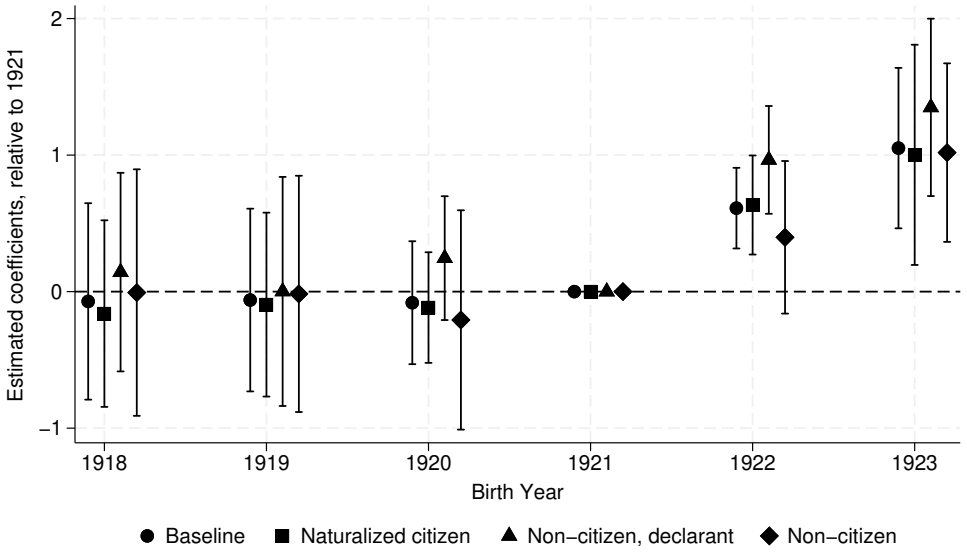
Notes: This figure plots estimates from Equation (9), separately excluding households whose fathers were born in Southern Europe, Central Europe, and Eastern Europe from the sample. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Confidence intervals are based on standard errors clustered by father's country of birth.

Figure A6: Exclusion of Western Europe



Notes: This figure plots estimates from Equation (9), excluding households with parents born in Western Europe (the UK, Ireland, Germany, the Netherlands, Switzerland, Belgium, France, and Luxembourg), based on the birth country of the father, the mother, or either parent, respectively. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A7: Estimates by Citizenship Status



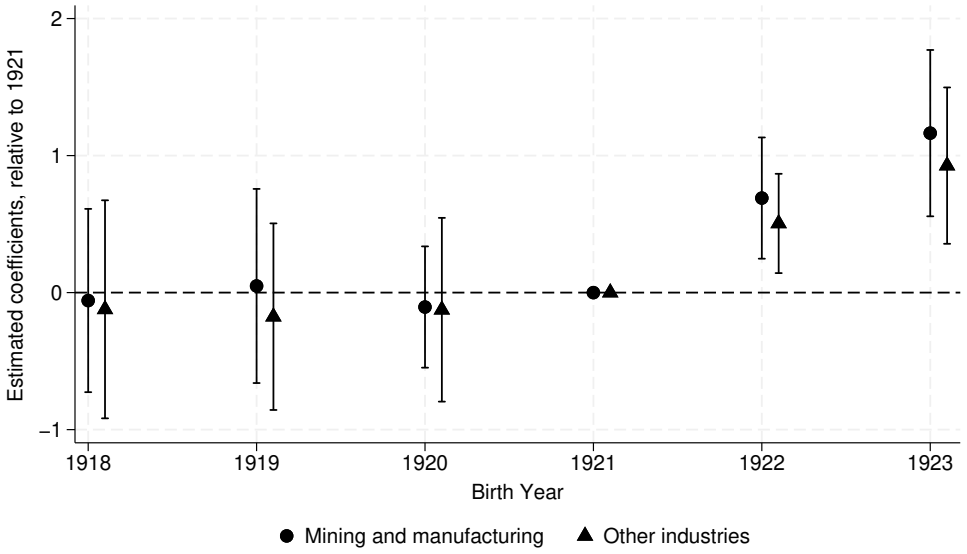
Notes: This figure plots estimates from Equation (9), separately restricting targeted households to those whose fathers are naturalized citizens, non-citizens who had filed a declaration of intention, and other non-citizens. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A8: Estimates by Father's Occupational Income Score



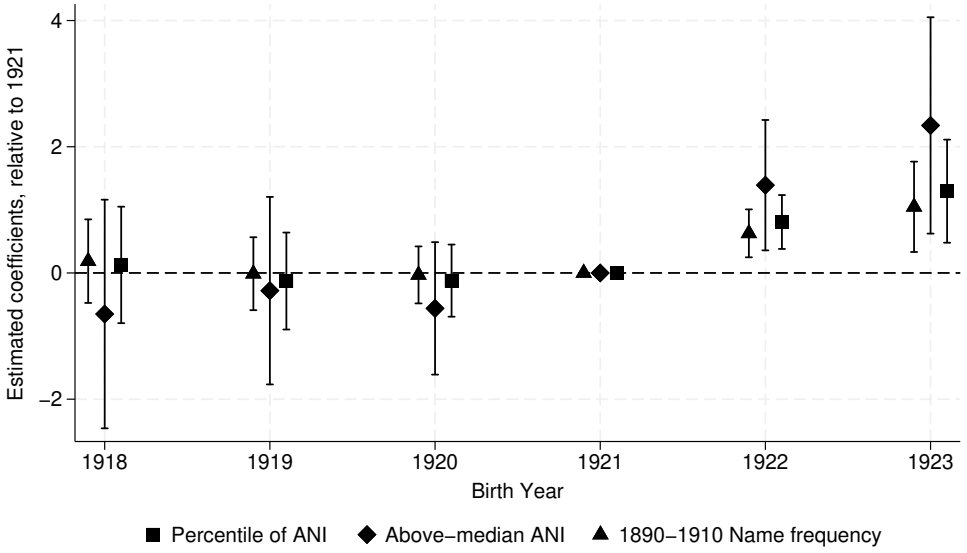
Notes: This figure plots estimates from Equation (9), separately for targeted households with paternal occupational income scores (OCCSCORE) at or below the median and above the median. The median is computed among fathers in targeted households. Targeted households whose fathers have no classified occupation are excluded. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A9: Estimates by Father's Industry



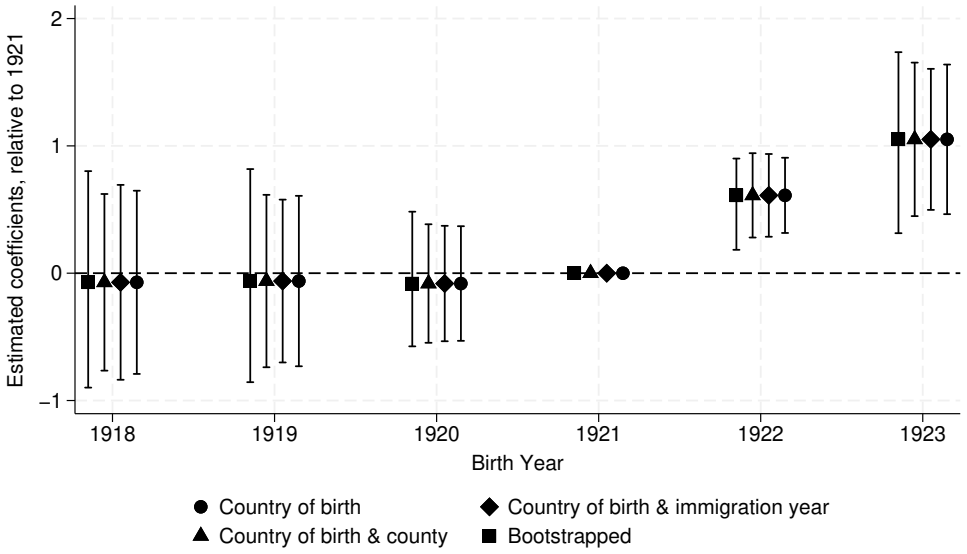
Notes: This figure plots estimates from Equation (9), separately restricting targeted households to those whose fathers work in mining or manufacturing and those whose fathers work in any other industry. Among households in which the father reports an industry, 47 percent of targeted fathers work in mining or manufacturing, compared with 28 percent of non-targeted fathers. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A10: Alternative Outcome Variables



Notes: This figure plots estimates from Equation (9) using three alternative measures of name Americanization. Percentile of ANI is the percentile rank of a child's ANI. Above-median ANI is an indicator equal to 100 if the ANI of the child exceeds the median and 0 otherwise. 1890–1910 Name frequency is the ANI constructed using name frequencies from the 1890–1910 birth cohorts. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Figure A11: Alternative Clustering Methods



Notes: This figure plots estimates from Equation (9) using alternative methods for statistical inference: clustering by father's country of birth; two-way clustering by father's country of birth and immigration year; two-way clustering by father's country of birth and county; and a wild cluster bootstrap clustered by father's country of birth using Webb weights and 9,999 replications. Immigration year and county are measured for the father. All specifications include household and birth-year fixed effects, child gender, and birth-order fixed effects.

B Robustness Checks: Long-Term Outcomes

Alternative Cutoffs Section 7.1 presents a significant effect of the Quota shock on the probability of reaching the top decile of the occupational distribution. Combined with small and only marginally significant effects on raw measures of occupational socioeconomic status, we interpret this pattern as consistent with an exclusion account, in which an American name and its downstream consequences fostered entry into a narrow set of positions from which immigrants were otherwise barred.

Table A1: Long-Term Effects on Occupational Socioeconomic Status: Alternative Cutoffs

Dep. var.: Indicator for occupational category	(1)	(2)	(3)	(4)	(5)
	Average of the SES index				
	Top 50%	Top 40%	Top 30%	Top 20%	Top 10%
Quota × Post	-0.001	0.002	0.000	0.009**	0.018***
	(0.007)	(0.007)	(0.009)	(0.004)	(0.005)
R-squared	0.62	0.62	0.60	0.58	0.59
Observations	91292	91292	91292	91292	91292
Individual controls	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10). The dependent variables are indicators for being in the top 50, 40, 30, 20, and 10 percent, respectively, of a composite SES index defined as the standardized average of the four standardized occupational SES measures. The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. Individual controls include gender and one-digit industry fixed effects. All specifications include household, birth-year, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table A1 provides supporting evidence by repeating the estimation with indicators for placement in the top 50, 40, 30, 20, and 10 percent of occupations as dependent variables, based on the composite index used in Column 6 of Table 9. For the top 50, 40, and 30 percent indicators, the coefficients are small and statistically insignificant. In contrast, the coefficient for the top quintile turns positive, and the estimate further increases at the top decile. These results suggest that the occupational gains were concentrated near the top of the distribution, rather than reflecting a broad improvement in occupational standing.

Table A2: Long-Term Effects on Occupational Socioeconomic Status: Excluding the Top Decile

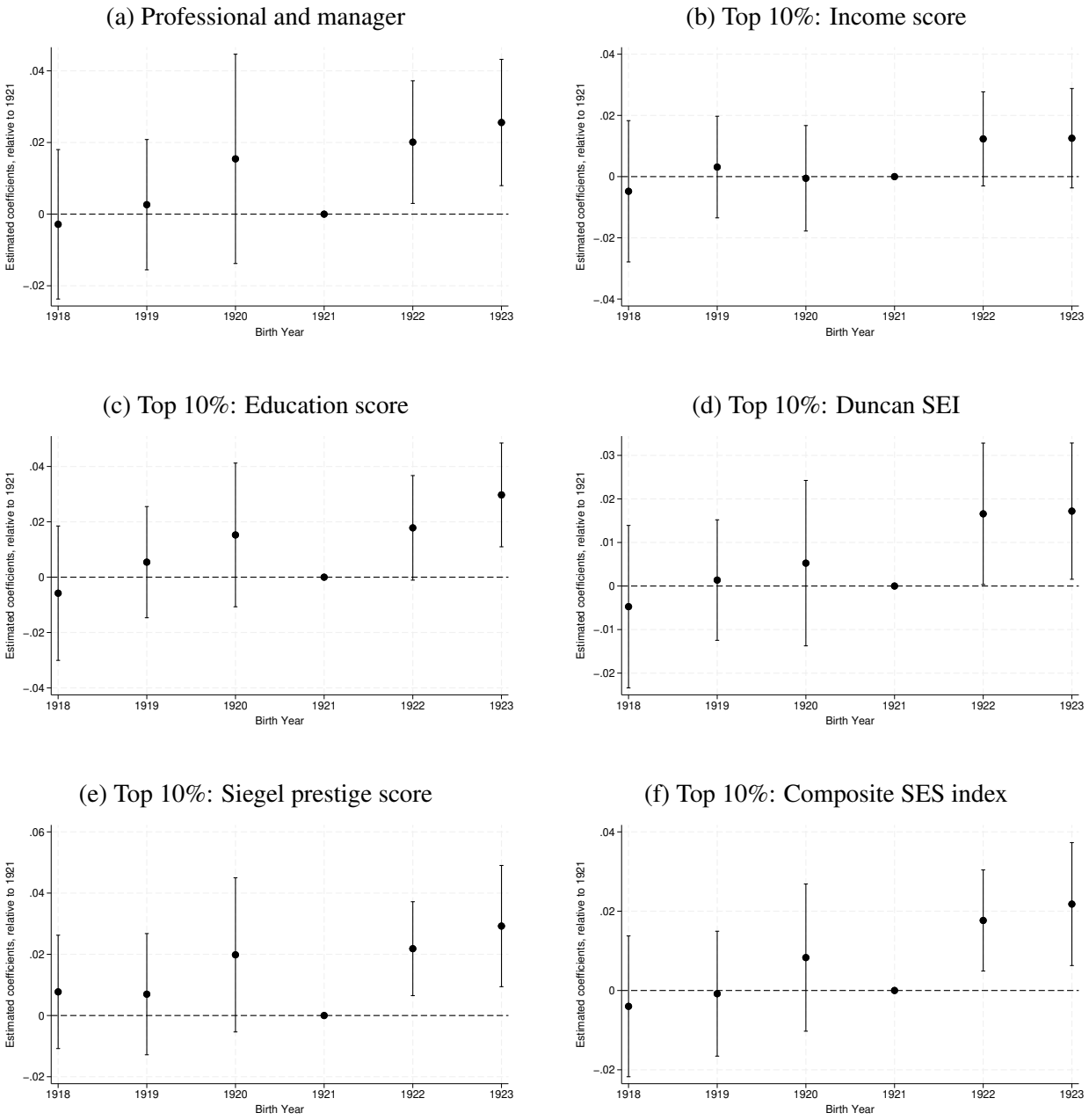
Dep. var.: occupational socioeconomic status scores	(1)	(2)	(3)	(4)	(5)
	Income score	Education score	Duncan SEI	Siegel prestige score	Mean
Quota × Post	0.003	-0.006	-0.004	0.003	-0.005
	(0.010)	(0.008)	(0.011)	(0.015)	(0.009)
R-squared	0.66	0.68	0.64	0.56	0.60
Observations	74988	70789	76350	73375	75105
Individual controls	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10), excluding individuals at or above the 90th percentile of the sample distribution of the corresponding dependent variable. The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. The dependent variables are z-standardized values of the occupational income score (OCCSCORE), occupational education score (EDSCOR50), Duncan socioeconomic index (SEI), and Siegel prestige score (PRESGL) in Columns 1–4, respectively, and their mean in Column 5. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. Individual controls include gender and one-digit industry fixed effects. All specifications include household, birth-year, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Similarly, the estimates in Table A2 indicate that the effects on raw occupational scores disappear when the sample excludes individuals at or above the 90th percentile of the corresponding dependent variable. This corroborates that the effects are disproportionately concentrated in upper-tail occupations.

Time-Varying Coefficients

Figure A12: Dynamic Effects on Occupational Socioeconomic Status: Right-Tail Outcomes



Notes: The figure plots the estimates of β_k from Equation (9) for alternative right-tail occupational SES outcomes. The dependent variables are an indicator for professional or managerial occupations (Panel a), indicators for the top 10 percent of the estimation sample distribution of the occupational income score, occupational education score, Duncan socioeconomic index, and Siegel prestige score (Panels b–e), respectively, and an indicator for the top 10 percent of a composite SES index defined as the standardized average of the four standardized measures (Panel f). The sample consists of sibling groups from the baseline sample with at least one sibling born before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act; 1921 is the omitted year. All specifications include household and birth-year fixed effects, gender, one-digit industry fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth.

Probit Estimates Given that the outcomes in Table 9 are binary, we re-estimate the specifications using a probit model. Unlike in the linear case, household fixed effects cannot be removed by a within-household transformation in a nonlinear setting. Moreover, since the sample consists of an average of two siblings per household, the incidental parameters problem prevents consistent estimation of the household intercepts. We therefore follow Mundlak (1978) and specify the household effects as a function of the household means of the regressors, which yields a correlated random effects probit model. In addition to the controls in Table 9, each specification includes the treatment indicator Quota_h and the household means of Quota$\times$Post, gender, and the birth year, birth order, and industry indicators. Because probit coefficients are not directly comparable to linear probability model estimates, we also report the average treatment effect on the treated (ATT), defined as the average change in the predicted probability from switching Quota $\times$ Post from zero to one.

Table A3: Long-Term Effects on Right-Tail Occupational Outcomes: Probit Estimates

Dep. var.: Indicator for occupational category	(1)	(2)	(3)	(4)	(5)	(6)
		Top 10% SES index				
	Professional and manager	Income score	Education score	Duncan SEI	Siegel prestige score	Mean
Quota × Post	0.101***	0.099***	0.102***	0.114***	0.096***	0.117***
	(0.021)	(0.025)	(0.026)	(0.032)	(0.026)	(0.027)
ATT	0.017***	0.015***	0.018***	0.016***	0.015***	0.018***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Pseudo R-squared	0.13	0.11	0.15	0.14	0.13	0.13
Observations	91292	91292	90316	91292	90316	91292
Individual controls	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓	✓
Household means (Mundlak)	✓	✓	✓	✓	✓	✓

Notes: The table reports probit estimates for right-tail occupational SES outcomes. The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. The dependent variables are indicators for professional or managerial occupations (Column 1), the top 10 percent of the estimation sample distribution of the occupational income score, occupational education score, Duncan socioeconomic index, and Siegel prestige score (Columns 2–5), respectively, and the top 10 percent of a composite SES index defined as the standardized average of the four standardized measures (Column 6). Following the correlated random effects approach of Mundlak (1978), household fixed effects are replaced by household means of all regressors. All specifications include gender, birth-year, birth-order, and one-digit industry fixed effects, as well as the household-level Quota indicator. Quota indicates that the father was born in a country targeted by the Emergency Quota Act, and Post indicates births in 1922–1923. The first row reports the probit coefficient on Quota × Post. ATT is the average over treated observations ($Quota$=1 and $Post$=1), calculated as the average difference in predicted probabilities when Quota × Post is set to one versus zero. Standard errors are clustered by father's country of birth; standard errors for the ATT are calculated using the delta method. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A3 presents the results. Both the probit coefficients and the ATTs are positive and statistically significant for all six outcomes. The ATTs range from 1.5 to 1.8 percentage points, comparable to the 1.3 to 1.9 percentage points in Table 9. The standard errors are smaller than in the linear probability specifications, which may partly reflect the additional assumptions that the correlated random effects model imposes on the unobserved household heterogeneity. Overall, the results suggest that the effects on right-tail occupational outcomes are not an artifact of the linear probability model.

Without Industry Fixed Effects The specification in Section 7 includes industry fixed effects to ensure a consistent comparison of occupation-based socioeconomic status. This may raise a bad control concern if industry itself operates as a channel for socioeconomic outcomes. To address

this concern, Table A4 presents estimates excluding these fixed effects. The results remain largely unchanged.

Table A4: Long-Term Effects on Right-Tail Occupational Outcomes: Without Industry Fixed Effects

Dep. var.: Indicator for occupational category	(1)	(2)	(3)	(4)	(5)	(6)
		Top 10% SES index				
	Professional and manager	Income score	Education score	Duncan SEI	Siegel prestige score	Mean
Quota × Post	0.018***	0.013***	0.019***	0.016***	0.016***	0.019***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
R-squared	0.57	0.56	0.57	0.57	0.57	0.57
Observations	91292	91292	90316	91292	90316	90316
Individual controls	✓	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10), excluding the one-digit industry fixed effects included in the baseline specification. The sample consists of sibling groups from the baseline sample with at least one sibling born in or before 1921 and one born after 1921, with non-missing occupations in the 1950 Census. The dependent variables are indicators for professional or managerial occupations (Column 1), the top 10 percent of the estimation sample distribution of the occupational income score, occupational education score, Duncan socioeconomic index, and Siegel prestige score (Columns 2–5), respectively, and the top 10 percent of a composite SES index defined as the standardized average of the four standardized measures (Column 6). Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

C Heterogeneity

C.1 Heterogeneity by Alternative County Characteristics

While Table 5 provides evidence of heterogeneity by the share of immigrants born in Quota-targeted countries (Treated), counties with large Quota-origin communities may differ in other characteristics that independently affected the assimilation response to the Act. To address this concern, we examine whether the results are robust to controlling for the triple interaction between Quota×Post and each of four county-level characteristics.

The first is the urban population share. Abramitzky et al. (2023) show that the shortfall of European workers in urban labor markets due to restrictive policies was filled by internal migrants. If arrivals from Quota-targeted countries were more likely than other immigrants to settle in cities,

local population composition may have independently shaped their assimilation decisions through the distinct economic roles that Quota-origin immigrants played. The second is the share of employment in manufacturing and mining, sectors that relied heavily on foreign-born labor (Collins, 1997). Counties more dependent on these sectors likely had stronger demand for workers from Quota-targeted countries. The restriction may have produced larger labor shortages in such counties, which could have affected the assimilation decisions of Quota-origin households. The third is the share of low-skilled employment.[38] Cohen and Biddle (2025) suggest that wages for low-skilled workers increased faster in local labor markets that had depended more on immigrants from restricted countries. Higher wages in these markets may have altered the economic incentives of Quota-origin households to assimilate. Finally, we include the county-level average occupational income score (OCCSCORE) as a summary measure of the local economic conditions described above.

Table A5 summarizes the results. Column 1 reports the baseline estimate, and Columns 2–5 add each triple interaction in turn. The coefficient on Quota$\times$Post$\times$Treated remains negative and statistically significant, with magnitudes similar to the baseline estimate. In contrast, none of the additional triple-interaction terms is statistically significant.

[38]Occupations with an EDSCOR50 value of 4.5 or below, a cutoff that lies just below the value for farmers, are classified as low-skill occupations. Under this definition, low-skill occupations account for 42.5% of employment in the full-count census.

Table A5: Triple Difference Estimates: Alternative County Characteristics

Dep. var.: American name index	(1)	(2)	(3)	(4)	(5)
Quota × Post	1.532***	1.700***	1.439***	1.415***	1.834**
	(0.378)	(0.430)	(0.445)	(0.488)	(0.811)
Quota × Post × Treated	-1.175***	-1.176***	-1.278***	-1.239***	-1.137***
	(0.334)	(0.335)	(0.355)	(0.382)	(0.306)
Quota × Post × Urban		-0.227			
		(0.269)			
Quota × Post × Manufacturing and mining			0.337		
			(0.567)		
Quota × Post × Low-skill				0.349	
				(0.771)	
Quota × Post × Occupational income score					-0.013
					(0.030)
R-squared	0.52	0.52	0.52	0.52	0.52
Observations	818366	818366	818366	818366	818366
Child gender	✓	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓	✓
County×Birth-year fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports heterogeneity in the effects on the American name index by the share of immigrants born in Quota-targeted countries (Treated), controlling for triple interaction terms with alternative county characteristics. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. The sample consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. All specifications include child gender, household fixed effects, birth-order fixed effects, and county-by-birth-year fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

C.2 Parental Immigration Timing

The baseline results indicate that households from Quota-restricted origins gave their children more American-sounding names after the Act. In the model, the magnitude of this response depends on κ, which captures attachment to the origin culture. If the observed change in naming patterns reflects cultural adjustment, households with stronger attachment should adjust less.

Table A6: Heterogeneity by Parental Arrival Timing and Name Americanization

Dep. var.: American name index				
	(1)	(2)	(3)	(4)
Parental arrival year	Q1	Q2	Q3	Q4
	Panel A. Quartiles across all origins			
Quota × Post	1.101***	1.082***	0.842**	0.586*
	(0.341)	(0.327)	(0.342)	(0.324)
R-squared	0.56	0.55	0.55	0.56
Observations	316734	338890	329022	327602
	Panel B. Within-origin quartiles			
Quota × Post	1.166***	0.991***	0.841**	0.609*
	(0.299)	(0.333)	(0.369)	(0.323)
R-squared	0.56	0.56	0.56	0.56
Observations	328846	327633	323575	332194
Child gender	✓	✓	✓	✓
Household fixed effects	✓	✓	✓	✓
Birth year fixed effects	✓	✓	✓	✓
Birth order fixed effects	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (10), separately restricting targeted households by quartile of parental arrival timing. Panel A defines quartiles using the average of the father's and mother's years of arrival, while Panel B first converts each parent's year of arrival into a percentile rank within their birth country and then averages the two percentile ranks. The first quartile contains households whose parents arrived earliest, either in absolute terms (Panel A) or relative to immigrants from the same countries of origin (Panel B). The sample consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

To evaluate this prediction, we use the timing of parental arrival as a proxy for the strength of attachment to the origin culture. Parents who arrived recently have spent less time in the United States and are thus more likely to retain origin-country norms. Those who arrived earlier have had longer exposure to American society, so their attachment is more likely to have weakened

(Abramitzky et al., 2020).

Table A6 re-estimates the baseline specification, splitting the treated group into quartiles of parental arrival timing while holding the control group fixed. The columns therefore report the effect of the Quota shock estimated separately for each quartile of the treated households. Panel A defines the quartiles using the average of the paternal and maternal arrival years. The estimated effect declines monotonically from the first to the fourth quartile, suggesting that parents who arrived in the United States more recently and thus retain stronger attachment to the origin culture responded less to the Quota shock.

Years of arrival, however, may also reflect differences in the timing of migration across origin countries. In this case, quartiles defined on raw immigration years may capture differences in origin-country composition across groups rather than household-level differences in arrival timing. To address this possibility, Panel B ranks the arrival year of the father and the mother separately within their own country of birth, and takes the average of the two percentile ranks. The results follow the same pattern, with both the effect size and the statistical significance declining across quartiles of the within-origin rank. Taken together, these gradients suggest that the assimilation response to the Quota Act was stronger among households with a longer presence in the United States and weaker among more recent arrivals, consistent with the prediction that attachment to the origin culture dampens the response.

C.3 Initial Local Hostility

The model in Section 3 implies that the effects of the Quota Act vary with the level of hostility that immigrant households initially faced. Regardless of the operative channel, the return to assimilation is concave in anti-immigrant sentiment. The Quota shock therefore raises the incentive to assimilate by less where baseline hostility was already high, which predicts a smaller naming response to the Act.

Table A7 tests this prediction. Following Table 6, we classify counties by whether their representative voted yea or nay on the Emergency Quota Act. Column 1 re-estimates the baseline specification using counties for which the roll-call record can be matched. The sample is smaller than in Table 2, but the estimated effect is close to the baseline estimate in both magnitude and statistical significance. Columns 2 and 3 split this sample by the vote status. The increase in the American name index is stronger in nay counties, those whose representatives opposed restriction and where baseline hostility is therefore presumed to have been lower.

Table A7: Heterogeneity by Hostility

Dep. var.: American name index			
	(1)	(2)	(3)
	All	Yea to Quota Act	Nay to Quota Act
Quota × Post	0.903***	0.613*	1.144***
	(0.317)	(0.351)	(0.346)
R-squared	0.53	0.51	0.54
Observations	618412	315758	302654
Child gender	✓	✓	✓
Household fixed effects	✓	✓	✓
Birth year fixed effects	✓	✓	✓
Birth order fixed effects	✓	✓	✓

Notes: Each column reports estimates from Equation (10). Columns 2 and 3 split the sample based on whether the county representative voted for or against the Emergency Quota Act, while Column 1 reports the estimate for the combined sample with available roll-call vote data. The number of observations in Column 1 is smaller than the baseline sample due to the limited availability of roll-call vote data. The sample consists of US-born children whose parents were both born outside the United States, restricted to households with at least one sibling born in or before 1921 and one born after 1921. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births in 1922–1923. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

D Model

D.1 Microfoundation

This section provides a brief microfoundation of the relative benefit from assimilation, $B(M, H)$. The following discussion serves only to show that the reduced form is consistent with standard theoretical premises. None of the empirical analysis depends on it.

Payoffs. The expected lifetime payoff of a child depends on the assimilation effort made by the parent. The payoffs of a non-assimilated (N) and an assimilated (A) child are defined as follows:

$$U_N(M,H) = w_0 + \phi(M)\, d(H) - \psi(H), \tag{A1}$$

$$U_A(M,H) = w_0 + \rho\, \phi(M)\, d(H) - \sigma\, \psi(H), \tag{A2}$$

w_0 is a common baseline payoff in the host economy. $\phi(M)$ is the *community value*, increasing and concave in community size ($\phi' > 0$, $\phi'' < 0$). The relevant community need not be limited to co-nationals; Section 6.1 examines its boundary. $d(H) \in (0,1]$ denotes the discount on community value arising from hostility against immigrants, a relationship that is convex ($d' < 0$, $d'' > 0$). $\psi(H)$ is a penalty from hostility, which is increasing and concave ($\psi' > 0$, $\psi'' < 0$).

In our context, a child's name is a marker that reflects both forces. A strongly foreign-sounding name signals membership in the immigrant community ($\phi(M)$) and marks the child as distinct ($\psi(H)$). In contrast, an Americanized name reduces these forces. We treat assimilation effort $a_i \in [0,1]$ as the degree of name Americanization chosen by the parent. An assimilated child retains only fractions $\rho \in [0,1)$ of community value and $\sigma \in [0,1)$ of the hostility penalty.

Derivation of $B(M,H)$. An altruistic parent values the child's expected payoff. We normalize the mapping from names to outcomes so that a child whose name has degree of Americanization a_i successfully assimilates into the mainstream with probability a_i. The child's expected payoff is then

$$\Pi(a_i; M, H) = a_i\, U_A + (1 - a_i)\, U_N. \tag{A3}$$

Parental assimilation effort bears the cultural-transmission cost $\frac{\kappa_i}{2} a_i^2$ (e.g., Bisin and Verdier, 2001). Since U_N is independent of a_i, the parent's problem coincides with (1) under

$$B(M,H) \;\equiv\; U_A(M,H) - U_N(M,H) \;=\; (1-\sigma)\,\psi(H) \;-\; (1-\rho)\,\phi(M)\,d(H) \tag{A4}$$

with optimum $a_i^* = B(M,H)/\kappa_i$, reproducing Equation (2).

It remains to check that these primitives reproduce (A1)–(A3).

Lemma. *If $\phi' > 0,\ \phi'' < 0$; $\psi' > 0,\ \psi'' < 0$; $d \in (0,1]$ with $d' < 0,\ d'' > 0$; and $\rho, \sigma \in [0,1)$, then $B(M,H)$ in Equation (A4) satisfies* (A1)–(A3).

Proof. Differentiating (A4),

$$B_M = -(1-\rho)\,\phi'\,d < 0, \qquad B_{MM} = -(1-\rho)\,\phi''\,d > 0,$$

$$B_H = (1-\sigma)\,\psi' - (1-\rho)\,\phi\,d' > 0, \qquad B_{HH} = (1-\sigma)\,\psi'' - (1-\rho)\,\phi\,d'' < 0,$$

$$B_{MH} = -(1-\rho)\,\phi'\,d' > 0, \qquad B_{HM} = B_{MH}.$$

The complementarity $B_{MH} > 0$ results from a single primitive: a larger immigrant community is worth less under hostility ($d' < 0$), so a larger community buffers the return to assimilation less where hostility is high, which is Assumption A3.

Because a_i is the degree of name Americanization, the optimal choice $a_i^* = B(M, H)/\kappa_i$ is what the American name index measures. The hostility term $(1 - \sigma)\,\psi(H)$ represents the socioeconomic returns to an Americanized name (e.g., Biavaschi et al., 2017), whereas the term $(1 - \rho)\,\phi(M)\,d(H)$ corresponds to the within-community assets that such a name forgoes. In this view, the American name index is a natural proxy for the assimilation margin. The naming choice trades off these returns against the forgone within-community assets, where parental attachment to the origin culture (κ_i) determines how strongly the choice responds to the net return $B(M, H)$.

D.2 Evidence on the Assumptions

Assumption (A3) in our model states that hostility and immigrant prevalence are complements in the return to assimilation ($B_{MH} > 0$), which plays a key role in separating the two mechanisms. However, the assumption is not self-evident. An increase in hostility may instead raise the value of the immigrant community as a source of protection and mutual insurance, in which case $B_{MH} < 0$.

Because the return to assimilation $B(M, H)$ is not directly observed, we evaluate this assumption through its implication for assimilation effort, which we measure with the American name index. Equation (2) implies that $\partial^2 a_i^*/\partial M\,\partial H = B_{MH}/\kappa_i$, which has the same sign as B_{MH}. We therefore examine how the American name index varied with county-level hostility, the local prevalence of Quota-origin immigrants, and their interaction. The estimating equation is as follows.

$$\begin{aligned} Y^b_{ihcst} = \alpha + \beta_1(\text{Hostility}_c \times \text{QuotaShr}_c) + \beta_2 \text{Hostility}_c + \beta_3 \text{QuotaShr}_c \\ + X_i'\boldsymbol{\gamma} + X_h'\boldsymbol{\eta} + \delta_{s,t,b} + \varepsilon^b_{ihcst} \end{aligned} \tag{A5}$$

Y^b_{ihcst} is the American name index of child i in household h whose father is from origin country b. t denotes birth year, and c and s indicate the county and state of residence. To capture patterns that existed before the Emergency Quota Act, the sample consists of children born to immigrant parents between 1910 and 1920. The coefficient of interest is β_1, the empirical counterpart of $\partial^2 a_i^*/\partial M\,\partial H$: assumption (A3) implies $\beta_1 > 0$, whereas $B_{MH} < 0$ implies $\beta_1 < 0$.

As in Table 6, Hostility_c measures initial hostility in each county using the roll-call vote of its representatives on immigration restriction bills. Column 1 uses the vote on H.R. 4075, the Emergency Quota Act of 1921. Columns 2 and 3 use votes on the two principal restriction bills that preceded it. H.R. 14461 is the bill reported in December 1920 to suspend immigration temporarily,

which passed the House but was not enacted. H.R. 6060, which would have imposed a literacy test on immigrants, passed both chambers of Congress but was vetoed by President Wilson in January 1915. The use of earlier votes mitigates concerns that support for the laws reflected conditions specific to 1921 rather than prior local sentiment. Column 4 uses the average of the three indicators. QuotaShr_c is the share of Quota-origin immigrants in the county population, measured from the 1910 Census.

X_i is the vector of individual controls, including gender and birth-order fixed effects. X_h is the vector of household characteristics, including fixed effects for race, birth year, citizenship, and year of immigration, each measured for both the father and the mother. $\delta_{s,t,b}$ denotes state $\times$ birth-year $\times$ origin-country fixed effects, so that β_1 is identified from variation across counties among children from the same paternal origin country who were born in the same state and year. Standard errors are clustered by father's country of birth.

Table A8: Empirical Evidence on the Model Assumptions: Based on Roll-Call Votes

Dep. var.: American name index				
	(1)	(2)	(3)	(4)
	H.R.4075	H.R.14461	H.R.6060	Average
Hostility	-0.278	-0.584	-0.241	-0.384
	(0.168)	(0.363)	(0.223)	(0.281)
Hostility × Quota share	3.908**	6.416**	6.185***	6.439**
	(1.804)	(2.873)	(2.023)	(2.656)
Quota share	-7.703***	-8.592***	-7.630***	-8.228***
	(1.548)	(1.072)	(1.527)	(1.235)
R-squared	0.08	0.08	0.08	0.08
Observations	1582298	1560452	1218847	1582298
Individual controls	✓	✓	✓	✓
Household controls	✓	✓	✓	✓
State×Birthyear×Origin-country fixed effects	✓	✓	✓	✓

Notes: Each column reports estimates from Equation (A5), using different congressional votes on immigration restrictions to measure local hostility: H.R. 4075, H.R. 14461, and H.R. 6060 in Columns 1–3, respectively, and their average in Column 4. The sample consists of children born to immigrant parents between 1910 and 1920. Quota share is the share of immigrants from countries targeted by the Emergency Quota Act in the county population, measured using the 1910 Census. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Household controls include race, birth year, citizenship status, and year of immigration, each measured for both the father and mother. Individual controls include child gender and birth-order fixed effects. All specifications include state $\times$ birth-year $\times$ origin-country fixed effects. Standard errors are clustered by father's country of birth. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A8 summarizes the results. The coefficient on the interaction between the hostility and Quota share variables is positive and statistically significant in every specification, which supports the sign restriction $B_{MH} > 0$ (A3). The results also align with the sign restrictions in (A1) and (A2). For each variable (M for the Quota share and H for the hostility measure), the sign of the marginal effect corresponds to the sign of the relevant partial derivative of B. For (A1), the marginal effect of the Quota share is largest at $H = 1$ because the interaction is positive and the hostility measure is bounded above by one. Even at this upper bound, it remains negative in every specification. The Quota share therefore enters negatively over the entire support of the hostility measure, a pattern consistent with $B_M < 0$. For (A2), the coefficient on hostility (the marginal effect at a Quota share of zero) is negative but small and statistically insignificant throughout. Even if these point estimates are taken at face value, the threshold value of the Quota share at which this derivative turns positive ranges from 0.039 to 0.091 across columns. Given these thresholds, the marginal effect of hostility is positive for 66.2–86.5 percent of observations in the baseline sample. Evaluated at the sample mean and median of the Quota share, this marginal effect is 0.452 (s.e. 0.095) and 0.384 (s.e. 0.076), respectively. It should also be noted that $B_H > 0$ is not essential to our proposed mechanism. This assumption serves only to allow for the possibility of a hostility channel and plays no role in distinguishing between the community-size and hostility channels.

D.3 Relaxing Shock Independence

Our model treats the direct consequences of the Quota—m_q and h_q—as independent of the size of the existing immigrant community, M_0. Therefore, the heterogeneity in the effects of the Quota shock by M_0 originates from the properties of $B(M, H)$ imposed by (A1)–(A3), rather than from heterogeneity rooted in the incidence of the shock itself. This is a simplification rather than a substantive restriction: allowing either m_q or h_q to depend on M_0 does not alter Prediction 2. Writing $M(q; M_0) = M_0 + m(q; M_0)$ and $H(q; M_0) = H_0 + h(q; M_0)$ and differentiating Equation (4) with respect to M_0 gives

$$\frac{\partial}{\partial M_0}\left(\frac{da_i^*}{dq}\right) = \frac{1}{\kappa_i}\left[\underbrace{B_{MM}\frac{\partial M}{\partial M_0}m_q + B_M\frac{\partial m_q}{\partial M_0}}_{\text{community-size channel}} + \underbrace{B_{MH}\frac{\partial M}{\partial M_0}h_q + B_H\frac{\partial h_q}{\partial M_0}}_{\text{hostility channel}}\right], \tag{A6}$$

which reduces to Equation (5) when $\partial m_q/\partial M_0 = \partial h_q/\partial M_0 = 0$ and $\partial M/\partial M_0 = 1$. Below, we consider whether relaxing these conditions affects Prediction 2, a key component for identifying the mechanism.

The inflow shock. While the sign of $\partial m_q/\partial M_0$ can be either positive or negative, the literature is more consistent with a positive sign. Immigrant destination choices tend to depend on pre-existing

co-ethnic networks (Bartel, 1989; Card, 2001; Munshi, 2003; Stuart and Taylor, 2021; Spitzer and Zimran, 2023), suggesting that the post-restriction inflow is disproportionately directed toward established communities. The absolute contraction would therefore be smaller in those areas ($\partial m_q/\partial M_0 > 0$). Under this condition, the community-size term in Equation (A6) becomes

$$\underbrace{B_{MM}\frac{\partial M}{\partial M_0}m_q}_{<0} + \underbrace{B_M\frac{\partial m_q}{\partial M_0}}_{<0} < 0, \tag{A7}$$

provided that $B_M < 0$ and $B_{MM} > 0$ under (A1), and $\partial M/\partial M_0 = 1 + \partial m/\partial M_0 > 0$. The community-size channel continues to predict a weaker assimilation response in larger communities.[39]

The hostility shock. Similarly, one may allow h_q to depend on M_0. If a more conspicuous immigrant presence provokes a stronger nativist reaction to the policy (Tabellini, 2020), then $\partial h_q/\partial M_0 > 0$ and the hostility term becomes

$$\underbrace{B_{MH}\frac{\partial M}{\partial M_0}h_q}_{>0} + \underbrace{B_H\frac{\partial h_q}{\partial M_0}}_{>0} > 0. \tag{A8}$$

Because we assume $B_H > 0$ (A2) and $B_{MH} > 0$ (A3), together with $\frac{\partial M}{\partial M_0} > 0$, the hostility channel also continues to predict a stronger response in areas with larger immigrant communities. Hence, the role of Prediction 2 in identifying the primary mechanism remains. This conclusion may change if $\partial h_q/\partial M_0 < 0$. For instance, if greater exposure to immigrants attenuates native anti-immigrant sentiment, the policy-induced rise in hostility may be weaker where immigrants had been more prevalent. In this case, the second term turns negative, making the sign of Condition (A8) ambiguous.

However, this possibility is not consistent with (A3). The complementarity assumption rests on the premise that a larger immigrant community is more visible, so that hostility is more salient where immigrants are more prevalent, whereas $\partial h_q/\partial M_0 < 0$ is based on the opposite premise. Although the two conditions are formally distinct—$B_{MH} > 0$ concerns the salience of a given level of hostility, while $\partial h_q/\partial M_0$ concerns the magnitude of the shock—they derive from a common view of how immigrant prevalence interacts with nativist sentiment, so the two conditions are difficult to reconcile with opposite signs. Moreover, Appendix D.2 documents pre-Quota county-level patterns supporting (A3). Maintaining (A3) while assuming $\partial h_q/\partial M_0 < 0$ would therefore require that a

[39]The latter condition indicates that a larger pre-Quota community implies a larger post-Quota community: an additional pre-Quota immigrant must not reduce the quota-era net inflow by more than one. This is not a strict assumption in that the Quota Act restricted new entry rather than removing existing residents.

larger immigrant presence simultaneously heightens and dampens hostility across the two margins, a pattern not supported by evidence.

E Migration

E.1 Migration Selectivity: Extended Sample

Table 6 examines migration selectivity among migrant households that contain the siblings in our baseline sample. To verify that the results are not specific to our sample construction, Table A9 replicates the estimation using all migrant households that include at least one child born between 1918 and 1923. The results remain robust.

Table A9: Migration Selectivity by Quota Status of Origin Country: Extended Sample

Dep. var.: Destination county characteristics				
	(1)	(2)	(3)	(4)
	Prevalence of Quota-immigrants		Anti-immigration sentiment	
Quota	0.025***	0.024***	-0.003	-0.003
	(0.003)	(0.003)	(0.020)	(0.019)
Outcome mean	0.091	0.091	0.576	0.576
R-squared	0.41	0.41	0.19	0.19
Observations	102940	102927	87460	87447
Origin-county fixed effects	✓	✓	✓	✓
Household controls		✓		✓

Notes: Each column reports estimates from Equation (12), extending the sample to all migrant households with at least one child born between 1918 and 1923. The dependent variables are characteristics of the household's 1930 destination county. In Columns 1–2, the outcome is the 1920 population share of immigrants from countries targeted by the Emergency Quota Act; in Columns 3–4, it is an indicator for local support for the Emergency Quota Act based on the House roll-call vote. Quota is an indicator for the household head being born in a country targeted by the Emergency Quota Act. All specifications include 1920 origin-county fixed effects. Household controls are included as indicated. Standard errors are clustered by father's country of birth. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

E.2 Migration Propensity

Beyond destination choice, the Quota Act may also have affected the extensive margin of migration. To examine whether households from targeted countries were more likely to relocate, and whether this difference depended on the size of the Quota-origin community in their 1920 county of residence,

we estimate

$$Migrant_{h,c} = \alpha_c + \beta Quota_h + \gamma Quota_h \times TreatShr_c + X_h'\delta + \epsilon_h, \tag{A9}$$

where $Migrant_{h,c}$ is an indicator equal to one if household h, residing in county c in 1920, changed counties of residence between 1920 and 1930. $Quota_h$ is an indicator for the father being born in a country targeted by the Emergency Quota Act, and $TreatShr_c$ is the population share of immigrants from Quota-targeted countries in county c measured in 1920. α_c denotes 1920 origin-county fixed effects, which absorb the main effect of $TreatShr_c$. X_h is a vector of household controls including race, birth year, citizenship status, and year of immigration, each measured for both the father and the mother.

Table A10: Migration Propensity by Treatment Status

Dep. var.: Indicator for migrant household				
	(1)	(2)	(3)	(4)
Quota	0.007	0.113***	-0.008	0.101***
	(0.025)	(0.024)	(0.025)	(0.023)
Quota × TreatShr		-0.847***		-0.884***
		(0.220)		(0.216)
R-squared	0.11	0.11	0.12	0.12
Observations	178968	178893	178961	178886
Origin-county fixed effects	✓	✓	✓	✓
Household controls	✓	✓	✓	✓

Notes: The table presents estimates from Equation (A9), which compares the propensity to migrate between 1920 and 1930 across households from countries targeted by the Emergency Quota Act and those from non-targeted countries. The dependent variable is an indicator for whether the household changed counties between 1920 and 1930. Quota is an indicator for the household head being born in a country targeted by the Emergency Quota Act, and TreatShr is the share of the county population born in Quota-targeted countries. All specifications include 1920 origin-county fixed effects. Household controls include race, birth year, citizenship status, and year of immigration for both the father and mother. Standard errors are clustered by father's country of birth. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A10 reports the estimates from Equation (A9). Columns 1 and 3, which exclude the interaction term, show no significant difference in migration propensity between Quota-origin and other immigrant households. This average comparison, however, masks substantial heterogeneity across origin counties. Once the interaction term is included in Columns 2 and 4, the coefficient on $Quota_h$ is positive and statistically significant (0.101–0.113), whereas the coefficient on $Quota_h \times TreatShr_c$ is negative and statistically significant (-0.884 to -0.847). Quota-origin households

were thus more likely to relocate than other immigrant households when the Quota-origin community in their county was small, and this difference declines as the Quota-origin share increases. This pattern indicates that the migration behavior of Quota-origin households depended on the local prevalence of the targeted population. Together with the destination patterns in Table 6, these results suggest that relocation among Quota-origin households was concentrated among those living in counties with small Quota-origin communities, whereas households already embedded in large Quota-origin communities had less need to move. This heterogeneity is consistent with the community-size channel.

E.3 Heterogeneity by Migration Status: Placebo Estimation

Table 7 suggests that the naming response to the Quota shock was weaker among migrant households. Combined with the migration selectivity documented in Table 6, this pattern is consistent with the substitution interpretation: households that moved toward larger concentrations of the targeted population had less need to assimilate. This interpretation, however, would lose validity if the heterogeneous association between naming patterns and origin groups had existed independently of the legislation.

Table A11: Heterogeneity by Migration Status: Placebo

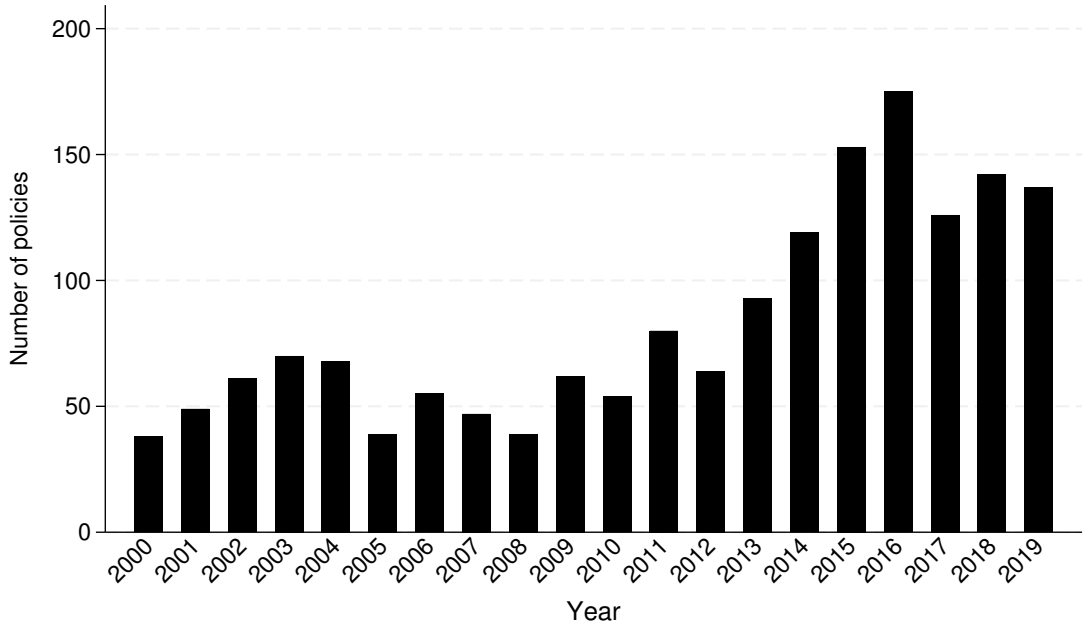
Dep. var.: American name index	(1)	(2)
	Non-migrants	Migrants
Quota × Post	0.145	0.362
	(0.369)	(0.331)
R-squared	0.53	0.56
Observations	533166	266470
Child gender	✓	✓
Household fixed effects	✓	✓
Birth year fixed effects	✓	✓
Birth order fixed effects	✓	✓

Notes: Each column reports estimates from Equation (10), replacing 1921 with 1919 as the placebo treatment year and estimating the specification separately by migration status. Migration status is defined by whether the household's county of residence changed between 1920 and 1930. The dependent variable is the American name index (ANI), ranging from 0 to 100, with higher values indicating names more common among native-born than foreign-born individuals. Quota is an indicator for the father being born in a country targeted by the Emergency Quota Act, and Post is an indicator for births after the placebo treatment year. All specifications include child gender, household fixed effects, birth-year fixed effects, and birth-order fixed effects. Standard errors are clustered by father's country of birth. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

To address this concern, Table A11 reports a placebo test that uses 1919 as the placebo treatment year, with the sample window adjusted in the same manner as in the baseline specification. The results show no significant difference, suggesting that the heterogeneity between migrant and non-migrant households is a pattern specific to the context of the Quota Act.

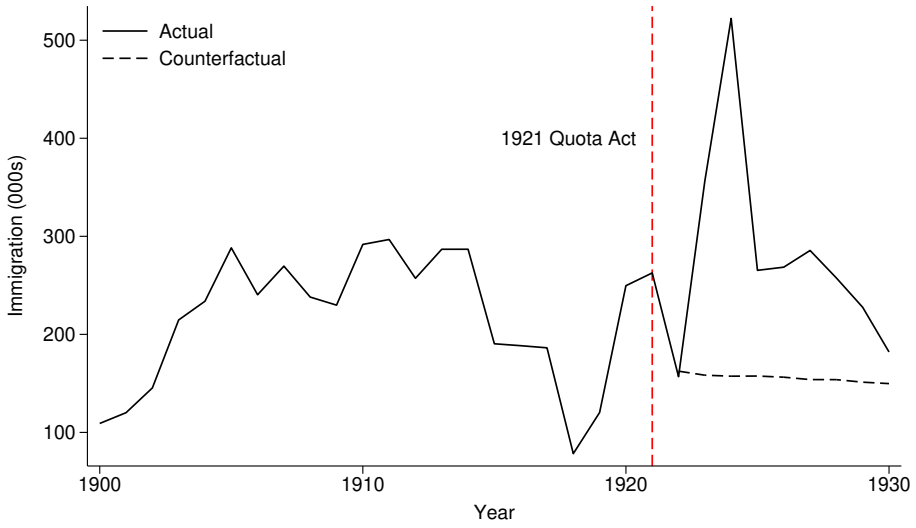
F Additional Figures and Tables

Figure A14: The Rise in Restrictive Immigration Policies, Europe, 2000–2019

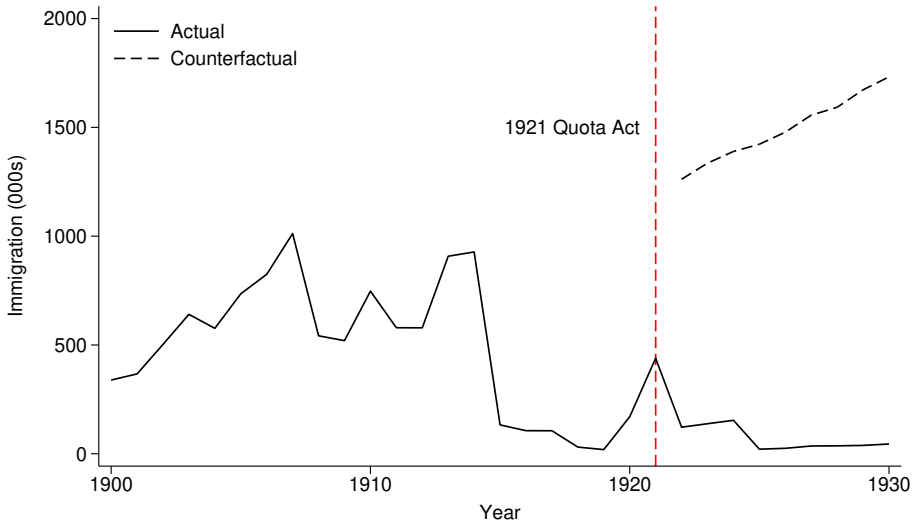


Notes: The figure plots the number of migration policy changes coded as more restrictive than the previously existing legal framework, by year of enactment, summed across 31 European countries (the European Union, the United Kingdom, Norway, Switzerland and Iceland) from the DEMIG-QuantMig Migration Policy Database (de Haas et al., 2015; Schreier et al., 2023). Years after 2013 were coded by the QuantMig extension in greater detail than earlier years, so part of the post-2013 increase may reflect more complete recording of minor changes.

Figure A15: Yearly Immigration 1900–1930



(a) Non-targeted Countries



(b) Targeted Countries

Notes: The figure plots yearly immigration flows from 1900–1930, and counterfactual immigration flows (in the absence of the Quota Act) from 1922–1930. Panel (a) plots the total flow for non-targeted regions (Canada, the Caribbean, Germany, Ireland, Latin America, Mexico, Scandinavia, the UK, and Western Europe). Panel (b) plots the total flow from targeted regions (Asia, central Europe, eastern Europe, Greece, Italy, Portugal, Russia, and Spain). Following Abramitzky et al. (2023), counterfactual immigration flows are estimated by regressing annual immigrant flows from each country group on a quadratic function of time and an indicator for war or recession years. A country's series begins in the first year its flow exceeds 2,000 arrivals, giving most countries about 100 years of unrestricted immigration flows. Immigrant flows data are from Abramitzky et al. (2023). The underlying series are based on historical immigration statistics reported in *Historical Statistics of the United States* ("Immigrants, by country of last residence—Europe: 1820–1997").

Table A12: Popular Names for Children of Immigrants, 1918–1923

Name	ANI	Share	Top 3 origin countries
Panel A. Boys			
Non-targeted			
Robert	75.1	0.03	Canada, Germany, Ireland
James	60.1	0.03	Ireland, Scotland, England
Thomas	48.6	0.03	Ireland, England, Mexico
Raymond	77.3	0.02	Canada, Mexico, Germany
Arthur	57.7	0.02	Germany, Canada, Sweden
Targeted			
Anthony	7.1	0.03	Italy, Poland, Lithuania
Michael	6.4	0.03	Italy, Poland, Czechoslovakia
Lewis	29.7	0.02	Italy, Poland, Russia/USSR
Stanley	36.4	0.02	Poland, Russia/USSR, Lithuania
Stephen	14.5	0.02	Poland, Czechoslovakia, Hungary
Panel B. Girls			
Non-targeted			
Ruth	80.8	0.02	Sweden, Germany, Norway
Alice	60.5	0.02	Ireland, Canada, Sweden
Margarita	37.1	0.01	Canada, Ireland, Mexico
Marie	42.3	0.01	Germany, Ireland, Canada
Evelyn	73.5	0.01	Sweden, Germany, Norway
Targeted			
Rose	11.7	0.03	Italy, Poland, Russia/USSR
Josephine	24.3	0.03	Italy, Poland, Czechoslovakia
Jane	30.4	0.02	Poland, Italy, Russia/USSR
Frances	39.5	0.02	Italy, Poland, Russia/USSR
Stella	37.8	0.01	Poland, Italy, Russia/USSR

Notes: This table presents popular names given to children of immigrants born in the United States between 1918 and 1923, separately by sex and by whether the father's country of birth was targeted by the Emergency Quota Act. For each group, we identify the 15 most frequently given names and exclude those appearing among the top 15 in both targeted and non-targeted groups. The table reports the five most common remaining names in each group, ranked by frequency. The American name index (ANI) measures the degree of name Americanization on a scale from 0 to 100, with higher values indicating more Americanized names. Share denotes the proportion of children within each sex and origin group given the corresponding name. The top three origin countries are the three birth countries of the father that are most associated with each name, calculated across all immigrant households regardless of targeted status.