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Closing Heaven's Door: Evidence from the 1920s U.S. Immigration Quota Acts

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# Closing Heaven's Door: Evidence from the 1920s U.S. Immigration Quota Acts\*

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## Abstract

The introduction of immigration quotas in the 1920s fundamentally changed U.S. immigration policy. We exploit this policy change to estimate the economic consequences of immigration restrictions for the U.S. economy. The implementation of the quota system led to a long-lasting relative decline in population growth in areas with larger pre-existing immigrant communities of affected nationalities. This effect was largely driven by the policy-restricted supply of immigrants from quota-affected nationalities and lower fertility of first- and second-generation immigrant women. In the more affected areas labor productivity growth in manufacturing declined substantially and native workers were pushed into lower-wage occupations. While native white workers faced sizable earnings losses, black workers benefited from the quota system and improved their relative economic status within the more affected areas.

**Keywords:** Immigration restrictions, productivity growth, local labor markets, racial wage gap

**JEL Codes:** J31, J61, N31, O15

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# 1 Introduction

There is an ongoing debate among policy-makers on how rich countries should respond to higher immigration. Recent events such as the European migrant crises or the discussion about increased border security in the United States have further stimulated the immigration debate. The issue of how immigration influences labor market conditions is also a central question in labor economics. Economic theory provides no ultimate answer to this question, as the predictions depend on, for example, the assumptions made about returns to scale, the degree of substitutability in production between native and immigrant labor, and the skill levels of these groups (Borjas, 2014). Empirical studies have investigated the effect of immigration on a range of economic outcomes, such as native wages, employment, and productivity, without reaching any consensus (e.g., Card, 2001, 2009; Borjas, 2003, 2006; Peri, 2012; Lewis and Peri, 2015; Cadena and Kovak, 2016).

This paper contributes to the understanding of how immigration restrictions have influenced the U.S. economy from a historical perspective. The debate on imposing immigration restrictions to prevent entry of certain ethnic groups into the United States is not a new one (Hutchinson, 1981; Higham, 2002). In the 1920s, the United States changed its open door policy for European immigrants by introducing immigration quotas based on national origins (King, 2000). While about 30 million immigrants from Europe arrived in the United States between 1850 and 1914 (Abramitzky et al., 2014), the implementation of the quota system curtailed European immigration to the United States from 4.5 million between 1910 and 1914 to less than 800,000 between 1925 and 1929 (U.S. Department of Commerce, 1931, Table 99). This fundamental change in immigration policy in the 1920s is unprecedented in U.S. history and can potentially be used to evaluate the causal effects of immigration restrictions on the U.S. economy (Abramitzky and Boustan, forthcoming).

We exploit this regime change in U.S. immigration policy—the passage of the Emergency Quota Act of 1921 and the Immigration Act of 1924, which abruptly ended the era of unrestricted immigration from Europe to the United States (Goldin, 1994)—to study the economic consequences of immigration restrictions for the U.S. economy during the period 1900-1940.<sup>1</sup> The implementation of the quota system provides two sources of variation which we exploit in our empirical analysis: First, the timing of the quota system permits a before-and-after quota comparison of the outcomes of interest. Second, the quota system restricted immigration from some nations more than from others. Our differences-in-differences strategy combines these two sources of variation with an established

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<sup>1</sup>We refer to Section 2 for a detailed description of the immigration quotas.

observation in the migration literature that newly arrived immigrants tend to settle in areas where previous immigrants of the same nationality live to measure the local exposure to the quota system (Bartel, 1989; Card, 2001; Munshi, 2003). We measure these nationality networks by the initial (pre-quota) spatial distribution of foreign-born individuals by countries of origin across the United States. Thus, we estimate the effects of a quota supply-driven decrease in immigration, which is likely to be unequal across local economies due to variation in the spatial distribution of foreign-born from different countries of origin. In other words, areas with larger pre-existing immigrant communities of affected nationalities would expect to receive fewer immigrants after the introduction of the quota acts in the 1920s.<sup>2</sup> We implement our differences-in-differences strategy using three different samples: U.S. Census county level data (1900-1940), a repeated cross-section of U.S. Census microdata (1900-1940), and hand-collected data from the U.S. Census of Manufactures at the city level (1909-1929).

We first establish that the quota system led to a long-lasting relative decline in population growth in counties with larger pre-existing communities of quota-affected nationalities. Our results reveal that counties with a one-standard-deviation greater quota exposure experienced a decline in annual population growth of 0.2 percentage points after the implementation of the immigration quotas, which corresponds to approximately 15 percent of a standard deviation in population growth in the post-quota period. This effect is quantitatively larger for urban counties, which is consistent with the fact that most immigrants at this time lived in large urban areas.<sup>3</sup> A simple accounting exercise demonstrates that the quota-induced decline in the immigration rate explains about 33 percent of the total population growth effect. We also show that first- and second-generation immigrant women were less likely to marry and had fewer children after the introduction of the quota acts, which further contributed to the decline in population growth. Our results on fertility and marriage are broadly consistent with Angrist (2002), who finds that high immigrant sex ratios during the 1910-1940 period increased the likelihood of second-generation women of the same ethnic group to marry, while it reduced their likelihood of participating in the labor market.

In the more affected urban areas, the quota system also led to a substantial decline in labor productivity growth in manufacturing. A one-standard-deviation increase in quota exposure reduced annual

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<sup>2</sup>While this strategy has similarities to the more traditional Bartik instrumental variable approach (Bartik, 1991), we focus on isolating policy-driven variation in the immigration flow to the United States; see also Section 3.2 for more details.

<sup>3</sup>For example, the share of foreign-born living in cities with more than 10,000 inhabitants in 1910 is around 25 percent, while it is around 8 percent in rural areas (own calculations based on the 1-percent national random sample from the Integrated Public Use Microdata Series (IPUMS) in 1910).

labor productivity growth by around 1 percentage point in urban counties and cities. The decline in labor productivity growth might be a result of agglomeration externalities, which are stronger in urban areas where a substantial part of the quota-affected immigrants at that time lived. Since we find no robust effects on capital deepening in the more affected cities, the productivity losses in manufacturing could also have occurred because firms did not adjust their capital intensity in response to the implementation of the quota acts.<sup>4</sup>

We also find that the regime change in U.S. immigration policy had a sizable effect on the earnings of native workers.<sup>5</sup> Our results reveal that native workers living in areas that were more exposed to the quota system were pushed into lower-wage occupations. This effect, however, differs substantially by race. While the implementation of the quota system led to substantial earnings losses for native white workers, black workers benefited from it in the more affected areas. A one-standard-deviation increase in quota exposure reduced the earnings of native white workers by 1.4 percent and increased the earnings of black workers by 1 percent. In order to evaluate to what extent the implementation of the quota system reduced the black-white earnings gap, we additionally exploit the within-county level variation of the quota exposure by race. We find a stronger decline of the black-white earnings gap in the more affected areas. For the average level of quota exposure, the earnings of black workers increased up to 2 percent relative to white native workers in the post-quota period (1930-1940), suggesting that European immigrant labor at that time had a lower elasticity of substitution to white native workers compared to black native workers. The insight that there are “winners and losers from immigration” relates to a recent paper by Borjas (forthcoming), who reassesses the wage impact of the Mariel boatlift and shows that the increased number of low-skilled immigrants from Cuba lowered the wage of native low-skilled groups. Our finding that the quota acts increased the relative economic status of black workers in more affected areas before World War II also contributes to the debate on the evolution of black-white income differences throughout U.S. history (e.g., Myrdal, 1944; Smith and Welch, 1989; Margo 2016).<sup>6</sup> While our estimates indicate that black workers improved their *relative* economic status within the affected areas, it is important to note that based on our differences-in-

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<sup>4</sup>See Goldin and Katz (1998), Atack et al. (2004), Katz and Margo (2014), and Lafortune et al. (2015) for a detailed discussion about the historical evolution of capital-skill complementarity in the United States.

<sup>5</sup>Due to the lack of individual wage data before 1940 we use occupation and industry-based earnings scores in the empirical analysis to evaluate the effect of the immigration quota acts on the earnings of native workers. For further details we refer to Section 3.1.

<sup>6</sup>For a linked sample of black men, Collins and Wanamaker (2014) show that the Great Migration contributed substantially to the decline in the black-white earnings gap before World War II. Other studies primarily focus on the evolution of black economic progress during the period 1940-1970 (e.g., Donohue and Heckman, 1991; Malony, 1994; Margo, 1995; Collins, 2000; Boustan, 2009; Carruthers and Wanamaker, 2017).

differences estimation strategy we cannot evaluate whether the quota system contributed to an overall decline in the black-white earnings gap during the period 1900-1940, which was a period of some but weaker black-white income convergence compared to after the 1940s (Margo, 2016).

The results of this paper advance the understanding of the economic consequences of immigration restrictions for the U.S. economy. Clemens et al. (2017) is one of the relatively few studies that investigates the labor market effects of a change in U.S. immigration policy—the exclusion of the so-called bracero workers on December 31, 1964. Using a differences-in-differences approach, Clemens et al. show that despite half a million seasonally employed farm workers from Mexico being excluded from the labor force, domestic farm laborers did not experience a rise in real wages or employment. They argue that this null finding is the result of employers adopting less labor-intensive technologies and changes in crop production. Chen (2015) investigates how the Chinese Exclusion Act of 1882 affected the average occupational standing of Chinese immigrants and finds that the average occupational income score of Chinese compared to Japanese immigrants significantly declined after the Exclusion Act of 1882. Greenwood and Ward (2015) and Massey (2016) examine how the U.S. quota laws during the 1920s changed migration behavior. While Greenwood and Ward (2015) show that emigration rates declined significantly after the introduction of immigration quotas, especially from unskilled occupations and farming, Massey (2016) examines how the enactment of the Emergency Immigration Act of 1921 affected migrant selection and finds that the average skill level of immigrants increased after the Emergency Immigration Act.<sup>7</sup> While our paper complements these studies, our focus is a different one, since we are interested in the macroeconomic implications of immigration restrictions for local economies and show that the implementation of the quota system led in more affected areas to a substantial decline in population and labor productivity growth and pushed native workers into lower-wage occupations.<sup>8</sup>

Our analysis of the 1920s immigration quota acts further adds to the immigration debate in the United States from a historical perspective (e.g., Borjas, 1994, 1999; Card, 1990, 2005; Cortes, 2008; Saiz, 2003, 2007). Many American economic historians have evaluated how immigration affected the U.S. economy during the 19th and early 20th centuries, although ours is the first study to evaluate the broader macroeconomic implications of the quota system for the U.S. economy with a focus on local economies.<sup>9</sup> We provide estimates of the effect of immigration restrictions on population

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<sup>7</sup>More information on classical immigration topics such as immigrant selection and assimilation can be found in the surveys of Borjas (1994), Kerr and Kerr (2011), and Abramitzky and Boustan (forthcoming), for example.

<sup>8</sup>Related work of Xie (2017) shows that the quota system decreased manufacturing wages and increased black immigration for the period 1920-1930.

<sup>9</sup>See, for example, Abramitzky et al. (2012, 2013, 2014, 2016); Bandiera et al. (2013); Collins (1997); Dunlevy and

growth, labor productivity growth, and earnings of native workers from this unprecedented change in US immigration policy that are well-identified. In particular, our differences-in-differences approach exploits only within-county or within-city variation of the data, and we are able to provide indirect support for the main identifying assumption by showing the existence of parallel trends prior to the implementation of the quota system. Our findings are also robust to a number of sensitivity checks; most importantly, we demonstrate that our results are not driven by shocks to immigration due to World War I and the Literacy Act of 1917.

This research also complements empirical studies that have investigated the long-term impact of (historical) immigration flows on local economies in the United States.<sup>10</sup> A recent county-level study by Nunn et al. (2017) finds positive long-term effects of immigration from Europe to the United States during the Age of Mass Migration.<sup>11</sup> Counties today still benefit from the historical inflow of European migrants in terms of productivity gains in the agricultural and manufacturing sector, increased urbanization, and higher rates of patenting during the Age of Mass Migration.<sup>12</sup> Peri (2012) uses state-level panel data to analyze the long-run effects of immigrants on employment and productivity for the period 1960-2006. Peri finds no evidence that immigrants crowded out employment, but he reports significant productivity gains from the net inflows of immigrants for the receiving states. Our paper adds to the findings of this literature by providing evidence that the immigration restrictions established during the 1920s led in the more affected areas to a significant decline of productivity growth in the manufacturing sector and to sizable earnings losses for native (white) workers.

## 2 Background

Opposition to immigration has a long history in the United States (Hutchinson, 1981; Higham, 2002). The rise of the Know-Nothing party—an anti-immigration movement—during the 1850s as a response to increased Catholic immigration from Germany and Ireland is such an example. The first laws that

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Hutchinson (1999); Hatton and Williamson (1998); Ferrie (1994, 1997, 1999); Greenwood (2008); Kim (2007); Timmer and Williamson (1998). For further details and references we refer to the surveys on immigration in American history of Abramitzky and Boustan (forthcoming), Carter and Sutch (1997), and Hatton and Williamson (1998).

<sup>10</sup>A growing literature in cultural economics investigates the long-term consequences of immigration for the U.S. economy (e.g., Fernández and Fogli, 2009; Grosjean, 2014; Bandiera et al., 2016), in particular showing that the cultural composition of US counties matters for economic development (e.g., Ager and Brueckner, 2013, 2016); Burchardi et al., 2016; Fulford et al., 2016).

<sup>11</sup>Likewise, Rodriguez-Pose and von Berlepsch (2014, 2015) also find that historical settlement patterns of migrants still matter for local economic development in the United States today.

<sup>12</sup>Further evidence on immigrants' positive impact on science and innovation in the United States is from Akcigit et al. (2017), Moser et al. (2014), and Hunt and Gauthier-Loiselle (2010).

restricted immigration to the United States at a larger scale were the Page Act of 1875 and the Chinese Exclusion Act of 1882. While the Page Act of 1875 forbade indentured labor from “China, Japan, or any Oriental countries”, immigration of alien convicts, and of women for the purpose of prostitution, the Exclusion Act of 1882 barred immigration from China to the United States in general. European immigration remained virtually unrestricted until the passage of the Immigration Act of 1917 (also referred to as the Literacy Act).<sup>13</sup> The 1917 act required from immigrants over sixteen years of age to pass a literacy test by reading 30-40 lines in any language of their own choice. It further banned immigration from the so-called Asiatic Barred Zone, which included most parts of Asia and the Pacific Islands.<sup>14</sup> The official statistics document that the total number of annual immigrants admitted to the United States dropped from circa 300,000 in the years just preceding the Immigration Act of 1917 act to around 100,000 immediately afterwards. Yet, the 1917 act effectively failed to reduce immigration from Europe to the United States on a larger scale, because literacy rates in Europe were rising rapidly during this time period. Already in 1920 annual immigration rose above 400,000 and exceeded 800,000 in 1921 (U.S. Department of Commerce, 1924, Table 65).

The first immigration policy effectively restricting the number of immigrants from Europe to the United States was the Emergency Quota Act of 1921.<sup>15</sup> This law restricted the annual number of aliens of any nationality to 3 percent of the number of foreign-born persons of such nationality listed in the U.S. Census of 1910. The implementation of nationality quotas reduced the total number of immigrants from 805,228 in 1921 to 309,556 in 1922 (U.S. Department of Commerce, 1929, Table 100).<sup>16</sup> Exempted from the quota system were immigrants born in Canada, Mexico, and South America (Massey, 2016). The 1921 act asymmetrically affected immigration from different European regions: Immigration from the southeastern part of Europe was now severely restricted, while immigration from the northwestern part of Europe was still considered desirable (King, 2000). For example, the number of Italian immigrants was reduced from 222,260 in 1921 to the 1922-quota

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<sup>13</sup>Various interest groups, such as the American Federation of Labor, were behind the passage of the Immigration Act of 1917. Goldin (1994) argues that some labor unions and native-born rural Americans were against free immigration and nearly succeeded in implementing immigration restrictions in the 1890s. However, what changed from 1890 to 1917 and became decisive for the vote in favor of the 1917 act was the position on free immigration among the older immigration groups living predominantly in the Midwestern areas, and rural-natives living in the American South, who initially had a more liberal view on immigration. We refer the reader to Hutchinson (1981), Goldin (1994), and Higham (2002) for more details on the political economy behind the passage of the Immigration Act of 1917.

<sup>14</sup>The Asiatic Barred Zone did not include Japan because the Gentlemen’s Agreement of 1907—an informal agreement between the governments of the United States and Japan—already limited immigration of Japanese workers to the United States.

<sup>15</sup>There were no serious considerations in the United States Congress to implement quotas or any other blanket restrictions to restrict immigration from Europe to the United States before 1920 (Goldin, 1994).

<sup>16</sup>Total immigration was limited to 357,000 per annum (King, 2000, p.200).



number of 42,057, while the number of Swedish immigrants in 1921 was 9,171 and the 1922-quota number was 20,042 (U.S. Department of Commerce, 1924, Tables 71, 79).

The Immigration Act of 1924 (also known as the Johnson-Reed Act) replaced the Emergency Quota Act and made the quota system permanent (King, 2000). The 1924 act involved two significant changes: First, the ceiling was reduced from 3 percent to 2 percent of the foreign-born stock. Second, the modified formula for the national origins quotas was based on the foreign-born stock of 1890 instead of 1910, which meant that the annual quota number was reduced from 357,083 in 1924 to 164,667 in 1925 (U.S. Department of Commerce, 1929, Table 111). This change almost completely prevented immigration from Southern and Eastern Europe. For example, the annual quota for Russia dropped from 24,405 to 2,248 immigrants only (U.S. Department of Commerce, 1929, Table 111). The 1924 act further completely banned immigration from Asia.

Until July 1, 1927 the nationality quotas were based on the 1890 Census. After July 1, 1927 the annual quota was fixed to a total of 150,000 immigrants and the 1924 act was substituted for a national origins plan, which based the quota allocation by country on the national origins of the white population in the United States in the 1920 Census (King 2000, pp.204-212).<sup>17</sup> The national-origins-based quota system became effective, with some delay, on July 1, 1929 and remained in place, apart from some minor modifications, until 1965 when it was replaced by the Immigration and Nationality Act.

### **3 Data and Estimation Strategy**

#### **3.1 Data**

Our empirical analysis evaluates how population growth, manufacturing activity, and individual outcomes, such as native earnings and the decision to migrate, responded to the introduction of the quota acts in the 1920s. The U.S. Census collected county-level data on demographic, economic, and social variables for every decade since 1790. These data are retrieved from the Inter-University Consortium for Political and Social Research (ICPSR) 2896 data file for the sample period 1900-1940 (Haines, 2010).<sup>18</sup> We use county-level population, urban population (i.e., population in areas with more than

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<sup>17</sup>The 1927 immigration quotas by country are depicted in King (2000, Table 7.1). On top of limiting immigration from southeastern European countries, the national origins system shifted the quotas towards immigration from Britain while reducing quotas from other northwestern European countries, such as Germany, Ireland, or Sweden.

<sup>18</sup>More information about the data set, such as scope of study, data collection, and data source can be found at <http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/02896>.

25,000 inhabitants), and value added per worker to measure labor productivity in the manufacturing sector as outcome variables for the empirical analysis.<sup>19</sup>

The essential ingredients for constructing the quota-exposure measure are the county-level population data by country of origin from the U.S. Census (see the following subsection for more details). For this measure, we draw on a well-established fact in the migration literature that newly arriving immigrants tend to settle in areas where previous immigrants of the same nationality live and that settlement patterns of immigrants varied by nationalities across the United States during the Age of Mass Migration (e.g., Ager and Brueckner, 2013). The basic intuition is that areas with larger pre-existing immigrant communities of affected nationalities would expect to receive fewer immigrants after the introduction of the quota acts in the 1920s.

For the city-level analysis we digitized data from the Census of Manufactures for the years 1909, 1914, 1919, 1925, and 1929. The manufacturing outcome variables are value added per worker to measure labor productivity and horsepower per worker (or horsepower per value added) to proxy for capital intensity.<sup>20</sup> Because of data constraints, we ended up with a balanced panel of 431 cities with more than 10,000 inhabitants in 1909. We use the 1-percent random sample of the micro-level census data in 1910 from the Integrated Public Use Microdata Series (IPUMS) to calculate nationality shares at the city level (Ruggles et al., 2015). The quota exposure measure is then constructed analogously to the county-level analysis. As can be seen from Table 1, the average foreign-born share affected by the quota system (i.e., quota exposure) is around 4 percent at the county level and 7 percent in the city sample.

The individual analysis is based on U.S. Census microdata from IPUMS. Our data consist of a repeated cross-section of individuals based on the 1-percent national random IPUMS samples of the population for the Census years 1900 to 1940. We use the following data sets for the empirical analysis: (a) a sample of 15-65-year-old workers to study how the quota acts affected the earnings of native workers and internal population movements; (b) a sample of women aged 15-49 to study fertility behavior and marriage. One limitation of the individual analysis is that the U.S. Census did not collect information on individual income before 1940. Since individual earnings are not reported, economic historians rely on so-called “earning scores” based on occupations, industries, and race

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<sup>19</sup>Since there are no value added data available for manufacturing in 1910 at the county level, these values had to be imputed. We use a linear interpolation to obtain the 1910 manufacturing value added data. Data on manufacturing workers are obtained from the Integrated Public Use Microdata Series (IPUMS) for the Census years 1900 to 1940 (Ruggles et al., 2015). Information on manufacturing workers is based on the imputed IPUMS variable “ind1950” codes 306-499.

<sup>20</sup>Note that information on horsepower for the 1925 Census of Manufacturers is not available. We use a linear interpolation to obtain the 1925 values for horsepower.

to study, for example, the returns to migration in the United States before 1940 (e.g., Abramitzky et al., 2012; Collins and Wanamaker, 2014). We follow this literature and use two measures of occupation-based earnings scores in our empirical analysis: The first measure is based on the annual earnings data reported in Lebergott (1964, Table A-18), which are available for a broad category of industries over the period 1900-1960.<sup>21</sup> We assign the annual earnings for a given census year (1900, . . . , 1940) and industry category to any individual reporting an industry code in IPUMS (“ind1950”), which corresponds to the broader industry classification.<sup>22</sup> Since the Lebergott data refer to annual earnings of full time employees, we adjust the earnings of workers by race, gender, and region of residence (South/non-South) for each industry category based on their actual earnings in the 1940 Census as described in Collins and Wanamaker (2014, Appendix A2). The adjustment factor in 1940,  $Adfactor_{irgl;1940}$ , is obtained by dividing the earnings,  $Earnings_{irgl}$ , of race  $r$ , gender  $g$ , in region  $l$  for every industry,  $i$ , in 1940 by the average earnings in the 1940 Census in that industry,  $Earnings_i$ .<sup>23</sup> For every worker in our sample, we adjust the “Lebergott earnings score” with the 1940 adjustment factor,  $Adfactor_{irgl;1940}$ , to obtain a race, gender, and location specific earnings score by industry. As in Collins and Wanamaker (2014, p.246), we also modify the earnings reported in Lebergott for the agricultural sector to account for differences between hired farm labor and farm operators’ income.<sup>24</sup> The Lebergott earnings score varies across occupations but, compared to the more frequently used IPUMS occupation score (“occscore”),<sup>25</sup> it also captures variation within occupations over time. The Lebergott earnings score is denoted in constant prices.<sup>26</sup>

The second measure follows the spirit of the IPUMS occupation score, but using the informa-

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<sup>21</sup>These broader industry categories are agriculture; manufacturing; mining (including subcategories); construction; transportation (including subcategories); communication and utilities (including subcategories); trade; service (including subcategories); government (including subcategories); and finance, insurance and real estate.

<sup>22</sup>For example, individuals reporting “ind1950” codes 306-499 in IPUMS work in manufacturing and obtain the corresponding manufacturing earnings value from Lebergott (1964). We base the assignment on the “ind1950” code from IPUMS, as it is considered to be comparable across different Census years.

<sup>23</sup>The average 1940 earnings in industry  $i$  are based on 15-65-year-old workers reporting an occupation in that industry.

<sup>24</sup>Farm operators’ net income is retrieved from the Major Statistical Series of the U.S. Department of Agriculture Volume 3 (U.S. Department of Agriculture, 1957). This volume contains information about farm operators’ net income for the years 1910-1956 (U.S. Department of Agriculture 1957; Table 17). There is no information on farm operators’ income for the Census year 1900. We obtain the 1900 value of farm operators’ income by multiplying the ratio of farm operators’ income to earnings of hired labor in agriculture in 1910 with the earnings of hired labor in agriculture in 1900 reported in Lebergott (1964).

<sup>25</sup>The IPUMS occupation score is based on a 1956 special report from the Census on occupational characteristics. It reports median incomes by occupations, which reflect the relative economic standing of occupations in 1950. The IPUMS then assigned to every individual in the Census reporting an occupation (“occ1950”) the respective value of their occupation.

<sup>26</sup>We used <https://www.measuringworth.com/uscompare/> to convert the Lebergott earnings score into constant prices. We use 1900 as the reference year.

tion about wages and salaries of workers from the IPUMS 1940 full count sample. Compared to the IPUMS occupation score, our measure has the advantage that we can construct an earnings score that differs by gender, race, census region (Northeast, Midwest, South, and West), and occupation.<sup>27</sup> The “1940 earnings score” captures variation in individuals’ wage income that arose from the implementation of the quota acts in the 1920s if individuals changed their occupation or kept their occupation but moved to a different region. However, compared to the Lebergott earnings score this measure is time invariant and the ranking of occupations is based on the terminal year of the sample. A further drawback is that the 1940 Census did not report any business and farm income. To overcome this issue in part, we assign farm operators (occ1950 codes 100 and 123) their net income in 1940 from the Lebergott earnings score adjusted by race, gender, and location (South/Non-South); however, we do not have such income information on other businesses than farming. We further need to assume that the relative standing of occupations was constant over time. Because of these limitations, we consider the Lebergott earnings score to be our preferred measure. The microdata are then merged with the county level quota-exposure measure. Further descriptive statistics are shown in Table 1.

[Table 1 about here]

### 3.2 Estimation strategy

The quota-exposure variable captures the extent to which a given county was affected by the quota system in terms of restricting the (potential) supply of immigrant labor. We construct *Quota exposure* as:

$$Quota\ exposure_c = \sum_{n=1}^N FB_{nc} \times Quota_n, \quad (1)$$

where  $FB_{nc}$  is the share of foreign-born of nationality  $n$  in county  $c$  in 1910,<sup>28</sup> and  $Quota_n$  measures the restrictiveness of the quota system for nationality  $n$  as:

$$Quota_n = \max \left( \frac{\overline{IM}_{n,10-14} - \overline{Q}_{n,22-30}}{\overline{IM}_{n,10-14}}, 0 \right), \quad (2)$$

<sup>27</sup>We use the following broad occupation categories from IPUMS variable “occ1950”: professional, technicals; farmers; managers, officials, and proprietors; clerical and kindred; sales workers; craftsmen; operatives; service workers (private households); service workers (not household); farm laborers; and laborers.

<sup>28</sup>We have information from the Census on the following foreign birthplaces: Albania; Armenia; Austria; Belgium; Bulgaria; Czech Republic; Denmark; Finland; France; Germany; Great Britain; Greece; Hungary; Ireland; Italy; Luxembourg; Netherlands; Norway; Poland; Portugal; Romania; Russia; Spain; Sweden; Switzerland; Turkey; Yugoslavia; Asia; Africa; Australia; Canada; Mexico; Cuba; Central and South America; and Hawaii.

where  $\overline{IM}_{n,10-14}$  is the average annual number of immigrants of nationality  $n$  admitted to the United States during the pre-WWI years 1910–1914, and  $\overline{Q}_{n,22-30}$  is the average annual quota number for nationality  $n$  during the post-quota years 1922–1930.<sup>29</sup>  $Quota_n$  is bounded between zero and one: For example, if one nationality is completely excluded from immigrating to the United States (i.e.,  $\overline{Q}_{n,22-30} = 0$ ),  $Quota_n$  takes on the value one and, in the other extreme, if the quota number exceeds the pre-WWI immigration flows (i.e.,  $\overline{IM}_{n,10-14} < \overline{Q}_{n,22-30}$ ), the restriction is non-binding and  $Quota_n$  becomes zero.<sup>30</sup> Data on the annual quota number for the years 1922–1930 and the actual number of pre-WWI immigrants admitted over the period 1910–1914 are collected from the Statistical Abstract of the United States (U.S. Department of Commerce, 1924, Table, 79; 1931, Tables 99 and 104).

Panel A of Figure 1 visualizes *Quota exposure<sub>c</sub>*: We see that the well-known immigrant clusters on the Northeastern coast, the Great Lakes region, and the West coast are generally more affected by the quota system compared to counties located in the American South and the southern parts of the Midwest. Since our empirical strategy exploits only county-level variation within the same state and not across such broad areas, Panel B of Figure 1 displays *Quota exposure* conditional on state fixed effects and reveals significant variation in quota exposure within states as well.

[Figure 1 about here]

The econometric model follows a differences-in-differences (DiD) approach to identify the effects of the quota system. Our variable of interest, *Quota exposure<sub>c</sub>*, measures the differential impact of the quota system in terms of restricting (potential) future immigration across counties by comparing the development of the outcome variables before and after the implementation of the quota system. More affected areas are expected to experience a stronger decline in the number of future immigrants relative to an area less affected by the quota system. Our estimation strategy resembles the more classical shift-share instrument-variable approach (e.g., Bartik, 1991) in the sense that we rely on local past settlement locations of the affected immigrant groups and aggregate shocks to migration flows. In contrast to the classical shift-share approach, which would exploit all immigration shocks (domestic and abroad) during the period 1900-1940, our estimation strategy isolates the policy-driven variation in the aggregate flow of immigrants to the United States from the implementation of the

<sup>29</sup>The official immigration year ended on June, 30, such that the immigration year of 1922 refers to immigration inflows between July 1, 1921 and June 30, 1922, for example. Nationalities exempted from the quota, such as Canadian and Mexican immigrants, were assigned the value zero (i.e.,  $Quota_n = 0$ ).

<sup>30</sup>This way of calculating the intensity of the quota system follows Greenwood and Ward (2015).

quota system.<sup>31</sup>

The DiD strategy naturally allows for falsification tests, where we can evaluate if *Quota exposure* is correlated with trends in outcomes before the implementation of the quota system. In particular, we begin the empirical analysis by estimating a so-called “flexible” model for population growth, which allows us to investigate the main identifying DiD assumption about common pre-treatment trends (or in this case common trends in growth rates) along with the dynamic effects on population growth in the post-quota period:

$$\Delta \ln pop_{ct} = \sum_{j=1910}^{1940} \alpha_j Quota\ exposure_c \times I_t^j + \sum_{j=1910}^{1940} \mathbf{X}'_c \times \mathbf{I}'_t \Gamma_j + \lambda_c + \mu_t + \phi_{st} + \varepsilon_{ct}, \quad (3)$$

where  $\Delta \ln pop_{ct}$  is the approximate 10-year population growth rate,  $Quota\ exposure_c$ , defined in equation (1), is interacted with a full set of time fixed effects ( $I_t^j$ ), where the omitted time period of comparison is 1920. In order to take into account potential mean reversion due to initial level or trend differences, the vector  $\mathbf{X}'_c$  includes (log) county population sizes in 1900 and 1910 interacted with a full set of time fixed effects. All our specifications control for county fixed effects ( $\lambda_c$ ) and time fixed effects ( $\mu_t$ ). The baseline specification also controls for state-by-time fixed effects ( $\phi_{st}$ ), implying a comparison of outcomes between more and less (or non-) quota affected counties within the same state. We cluster the error term,  $\varepsilon_{ct}$ , at the county level. The sample spans the period 1910-1940.<sup>32</sup> Our data support the assumption of common trends in growth rates if  $\hat{\alpha}_{1910} \approx 0$ . The post-quota effects are denoted by  $\hat{\alpha}_{1930}$  and  $\hat{\alpha}_{1940}$  and reflect the impact of the introduction of the quota system on population growth in 1930 and 1940 relative to the omitted time period 1920 (i.e., the change in log population from 1910 to 1920).

We also estimate the following standard (non-flexible) DiD model:

$$\Delta \ln y_{ct} = \beta Quota\ exposure_c \times I_t^{post} + \sum_{j=1910}^{1940} \mathbf{X}'_c \times \mathbf{I}'_t \Psi_j + \lambda_c + \mu_t + \phi_{st} + \epsilon_{ct}, \quad (4)$$

where  $y_{ct}$  is population size or manufacturing value added per worker, and  $I_t^{post}$  is an indicator equal to one after 1920 (i.e., in the post-quota period). In addition to the above-mentioned variables,  $\mathbf{X}'_c$  also contains controls for (potential) immigration shocks caused by World War I and the Literacy Act of 1917. These results are reported in the robustness analysis. The remaining variables are defined

<sup>31</sup>See, for example, the discussion on empirical approaches to identify the causal effects of immigration on local economies in Lewis and Peri (2015).

<sup>32</sup>Since the outcome variable is a growth rate, we use data starting in 1900, and so  $t = 1910$  corresponds to the 10-year period 1900-1910, and  $t = 1920$  corresponds to the 10-year period 1910-1920, etc.

as in equation (3). This model estimates the average effect of implementing the quota system and is therefore more suited in terms of interpreting our results.

A similar model is used at the city level for the years 1909, 1914, 1919, 1925, and 1929:

$$\Delta \ln y_{vt} = \gamma Quota\ exposure_v \times I_t^{post} + \sum_{j=1909}^{1929} \mathbf{X}'_v \times \mathbf{I}_t^j \Phi_j + \lambda_v + \mu_t + \phi_{st} + \zeta_{vt}, \quad (5)$$

where  $y_{vt}$  is manufacturing value added per worker or horsepower per worker (or per value added) in city  $v$  in year  $t$ . The variable  $Quota\ exposure_v$  is constructed as described in equation (1), but using the foreign-born share of the different nationalities living in city  $v$  in 1910 from IPUMS.  $Quota\ exposure_v$  is interacted with  $I_t^{post}$ , an indicator equal to one after 1919.  $\mathbf{X}'_v$  contains (log) manufacturing value added per worker (or horsepower per worker/value added) measured in 1909 and 1914, and (log) city population size in 1910, interacted by time. The parameter  $\lambda_v$  denotes city fixed effects, and  $\zeta_{vt}$  is the error term clustered at the city level. The remaining variables are defined as in equation (3). Because of data restrictions, we only consider cities with more than 10,000 inhabitants in 1909. One important advantage of the city-level data is that these are available for the years 1909, 1914, 1919, 1925, and 1929, and we can consequently mitigate the concern of omitted time-varying shocks during the post-quota period, such as the Great Depression. In addition, since most immigrants at that time lived in cities, a second advantage of using these data is that we can evaluate the impact of the quota system on the manufacturing sector in cities where one expects it would matter the most.

For the individual-level analysis we estimate the following model:

$$y_{ict} = \delta Quota\ exposure_c \times I_t^{post} + \mathbf{M}'_i \Omega + \lambda_c + \mu_t + \phi_{st} + \eta_{ict}, \quad (6)$$

where  $y_{ict}$  is the outcome of interest (see Section 5) for individual  $i$  living in county  $c$  and sampled in Census year  $t$ .  $Quota\ exposure_c \times I_t^{post}$  is defined as in equation (4).  $\mathbf{M}'_i$  denotes a set of individual-level controls, such as dummies for race, place of residence (rural/urban), marital status, fixed effects for age and birthplace, and  $\eta_{ict}$  is the error term clustered at the county level. The remaining variables are defined as in equation (3). Equation (6) is estimated using a repeated cross-section of individuals for the years 1900, 1910, ..., 1940.

## 4 County and City Results

### 4.1 Population growth

This subsection documents how the quota system influenced population growth at the county level. We first provide evidence that more affected areas in fact experienced a (relative) decline in the inflow of immigrants, which can be viewed as a consistency check. If our strategy based on nationality networks of previous immigrants captures the effect of the quota acts, we would expect a reduced inflow of immigrants into the more quota affected counties, whereas the effect on population growth can be mitigated or amplified via in-and-out migration and changes in fertility behavior (e.g., Peri, 2016). To check whether our exposure measure actually captures reductions in immigration we derived a retrospective measure on the number of immigrants at the county-by-year level based on immigrants' year of arrival and their current place of residence from the IPUMS full-count Census data in 1920 and 1930. This information is then used to construct a location-based measure of immigration flows aggregated by quota exposure for every year from 1900 to 1930.<sup>33</sup>

Figure 2 graphs the annual average inflow of immigrants by *Quota exposure<sub>c</sub>*: We group counties such that if their quota-exposure value is above/below the sample median, they are assigned to the treatment/control group, respectively. The reported average inflow of immigrants in Panel A is conditional on county fixed effects, while Panel B and Panel C also condition on World War I and the Immigration Act of 1917.<sup>34</sup> Before World War I, counties in the treatment group received on average more immigrants, but the time trend is relatively similar to counties in the control group.<sup>35</sup> At the outbreak of World War I, both treatment and control counties experienced a significant decline in immigrant inflows, but the average decline seems to be stronger among counties in the treatment group.

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<sup>33</sup>We note that the use of the variable YRIMMIG from IPUMS for this purpose is not without problems. One main concern is the so-called “year heaping”, since some immigrants might not exactly remember the date of arrival at the time of the Census enumeration and just use the nearest round number as year of arrival (e.g., 1900, 1910, ...). Since we measure year of arrival retrospectively (i.e., in 1920 and 1930), another concern is that the immigration flows are downward biased due to death and emigration before the year of Census enumeration. Appendix Figure 1 compares the year of arrival measure with the Ferenczi and Willcox (1929, Table 3) immigration time series which is based on the admission of immigrant aliens by year and country of last residence for a selected group of nationalities to check whether measurement issues might confound our findings. While the two time series move very much in the same direction, it is important to note that the Ferenczi/Willcox immigration flows are larger than the immigration flows derived from IPUMS, which is expected, as the latter measure reflects immigrants who stayed in the United States and did not die or return to their home country before the Census enumeration (see Appendix B for a further discussion).

<sup>34</sup>We refer the reader to Section 4.3 on how we construct the controls for World War I and the Immigration Act of 1917.

<sup>35</sup>There is a steep decline in the average inflow of immigrants from 1900 to the following years, which may be evidence of “year heaping”. This decline, however, is similar between treatment and control counties, suggesting that this type of measurement error is less of a concern in our case.



There is an immediate reversion in immigration inflows in the aftermath of World War I close to pre-war levels. Following the Immigration Act of 1924, the average flow of immigrants into treatment counties reduces significantly compared to the control counties. This reveals that the more affected areas in fact experienced a relative decline in the number of immigrants after the implementation of the quota acts, compared, in particular, to the pre-war years.<sup>36</sup> Panel B and Panel C in Figure 2 demonstrate that this conclusion is robust to controlling for the possible confounding effects of the immigration shocks caused by World War I and the Immigration Act of 1917.

*[Figure 2 about here]*

Panel A of Table 2 presents our findings for population growth. The estimating equation is (3) and the method of estimation is least squares. Columns 1-3 summarize the results for all counties, and columns 4-6 restrict the sample to urban counties (with at least 25,000 inhabitants in urban areas in 1900). While column 1 reveals that the decline in population growth was stronger between 1910 and 1920 in more affected counties, this pre-quota growth difference is reversed when we control for (log) population sizes in 1900 and 1910 interacted with time fixed effects (column 2). The initial population-interaction controls take out possible concerns that the quota-exposure measure is mechanically linked to past population growth. Our baseline specification (column 3) controls on top of that for state-by-time fixed effects and shows that counties with a higher quota exposure experienced a reversal in the trend of the population growth rate after 1920, since both post-quota estimates are negative and statistically significant at the 1-percent level. The point estimate for 1930, for example, suggests that a one-standard-deviation increase in quota exposure is associated with a reduction in the annual population growth rate of about 0.3 percentage points. For urban counties we do not find any evidence of significant differences in the pre-quota period, whereas the post-quota estimates are both negative and statistically significant at the conventional levels. Generally, our results reveal that the decline in population growth was stronger in urban counties. The baseline specification indicates that a one-standard-deviation increase in quota exposure is associated with a decrease in the annual population growth rate of about 0.7 percentage points in 1930 (column 6).

Panel B of Table 2 reports the corresponding estimates for the non-flexible DiD model using estimating equation (4). These estimates confirm the insights from the flexible model reported in Panel A. In particular, we find that the effect of the quota system on population growth reduces

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<sup>36</sup>Appendix Table 1 reports the results from estimating a model similar to estimating equation (4), but using decadal immigration rates as outcome variable. These results show that the more affected counties experienced a relative decline in immigration rates following the implementation of the quota system.

in numerical magnitude from column 1 to column 3. This is most likely driven by the downward trend in population growth in the pre-quota period, which is eliminated in column 3 as we control for initial population sizes (interacted with time fixed effects) and state-by-time fixed effects. The magnitude of the estimate reported in column 3 is such that counties with a one-standard-deviation higher quota exposure experienced a decline in the annual population growth rate by 0.2 percentage points from 1920 to 1940. This change corresponds to about 15 percent of a standard deviation in the population growth rate in the post-quota period. The parallel finding for the population growth rate in urban counties (column 6) is a decline of 0.4 percentage points per annum, which means that a one-standard-deviation difference in quota exposure is able to explain about 24 percent of the observed decrease in population growth in urban counties between the pre- and post-quota periods.

The findings of Table 2 suggest that the implementation of the quota system had an overall negative effect on population growth, which was unequal across the United States. Counties with larger pre-existing immigrant communities of quota-affected nationalities experienced larger declines in population growth. Appendix A outlines a simple accounting exercise, which evaluates how much of the population-growth effect is directly attributable to declining immigration rates. We find that the direct effect of restricting the number of immigrants through the quota system explains about 33 percent of the total population-growth effect for the whole sample and 53 percent in urban counties. Given statistical uncertainty, these effects are sizable. In Section 5.3 we show that the implementation of the quota system also reduced fertility among first and second-generation immigrant women, which further contributed to the negative effect on population growth in the post-quota period.

*[Table 2 about here]*

## **4.2 Labor productivity growth**

Having established that the quota system substantially reduced population growth in more affected counties relative to less affected counties, we study in this subsection how this fundamental change in U.S. immigration policy influenced labor productivity growth (measured by value added per worker) and the capital intensity (measured by horsepower per worker or per value added) in manufacturing.

In more affected areas the implementation of immigration quotas in the 1920s resulted in a statistically significant decline in labor productivity growth and no robust effect on capital intensity. Columns 1-3 of Table 3 summarize the county level results from estimating equation (4). The method of estimation is least squares, and all three specifications include the baseline controls (county fixed

effects, state-by-time fixed effects, initial outcomes and log population size in 1910 both interacted with time fixed effects). Column 1 shows the results for all counties observed throughout the sample period 1900-1940. The effect of quota exposure on labor productivity growth is negative and statistically significant at the 10-percent level. A one-standard-deviation increase in quota exposure led to a 0.3 percentage points decline in labor productivity growth per annum. This effect almost doubles in (numerical) magnitude and is now statistically significant at the 1-percent level when we restrict the sample to urban counties in column 2. A one-standard-deviation increase in quota exposure reduced labor productivity growth in urban counties by around 0.8 percentage points. Column 3 shows that the negative productivity growth effect is mainly driven by manufacturing establishments located in urban counties of the Northern states. Columns 4-7 of Table 3 report the city level results from estimating equation (5), controlling for city fixed effects, state-by-time fixed effects, and the initial outcomes and log city population size in 1910, both interacted with time fixed effects. Note that only cities with more than 10,000 inhabitants in 1909 are included in this sample, which spans the years 1909, 1914, 1919, 1925, and 1929. The estimate in column 4 reveals that a one-standard-deviation increase in quota exposure led to a statistically significant decrease in productivity growth of around 1 percentage point per annum. Consistent with the county level results, the negative productivity growth effect is mainly driven by manufacturing establishments located in the Northern cities of the United States (see column 5).<sup>37</sup> Columns 6 and 7 summarize the results on whether the implementation of the quota system led firms to adjust their capital intensity. We proxy this adjustment process by the growth rate in machine horsepower per worker (column 6) or machine horsepower per value added (column 7). The estimates indicate that, if anything, manufacturing establishments in the more affected cities decreased capital intensity, but these results are not very precisely estimated; see section 4.3, Table 4, columns 6-7.

Overall, our findings suggest that the implementation of the quota system led to substantial labor productivity losses in more affected areas. For example, since the annual average productivity growth rate in manufacturing between 1920 and 1930 in all (urban) counties was 1.3 (2.7) percent, the above calculations suggest that a one-standard-deviation difference in quota exposure is associated with a decline in productivity growth of around 0.3 (0.8) percentage points. Likewise, the annual average productivity growth rate in the post-quota period in larger cities (above 10,000 inhabitants in 1909) was 2.7 percent with associated losses in productivity growth due to a one-standard-deviation

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<sup>37</sup>We obtain qualitatively similar results when using the average wage growth in manufacturing instead of value added per worker as a proxy for labor productivity growth (not reported).

difference in quota exposure of around 1 percentage point.

[Table 3 about here]

### 4.3 Robustness

In this subsection, we demonstrate that the main findings of Tables 2 and 3 do not depend exclusively on our preferred estimation strategy. Appendix Table 2 replicates the main specifications of Tables 2 and 3 based on a so-called Bartik-style approach (we refer to Appendix C for the details). Reassuringly, these “Bartik-style” estimates confirm the main findings of Tables 2 and 3.

One important threat to identification would be if our results are driven by the immigration shocks caused by World War I and the Literacy Act of 1917. To address this concern, we follow a similar approach as in equation (1) and construct a local exposure measure of these two aggregate immigration shocks. The World War I exposure measure replaces the quota number (i.e.,  $\bar{Q}_{n,22-30}$ ) with the flow of immigrants of nationality  $n$  during World War I in equation (2).<sup>38</sup> Using this approach, we find that World War I (almost) completely restricted immigration for all nationalities not coming from the Americas. This measure is interacted with the foreign-born share of nationality  $n$  and finally summed over all nationalities as in equation (1). The Literacy Act of 1917 is constructed in a similar fashion. Instead of  $Quota_n$ , we use data from Lee and Lee (2016) on the percentage of the 1915 population in a given sending country,  $n$ , with no schooling in equation (1). The main idea is that the percentage of the population with no schooling is a good proxy for (national) illiteracy rates. It is important to note that the raw correlations between the quota exposure measure and the exposure to World War I and the Literacy Act of 1917 are 0.90 and 0.83, respectively. The three exposure measures are relatively highly correlated, as they all exploit the spatial variation of the foreign-born share in 1910. In particular, exposure to the quota system and to World War I are highly correlated, as both shocks affected many of the same nationalities while keeping migration from the Americas unrestricted.

Table 4 reports the main specifications at the county and city level when controlling for the exposure to World War I and the Literacy Act of 1917. We see that the estimates for population growth at the county level increase in numerical magnitude (columns 1 and 3). The same is true for the productivity growth regressions for urban counties and cities (columns 4 and 5); however, the estimate for the whole sample turns insignificant (column 2). The effect on capital intensity at the city level is now statistically insignificant for both measures. Thus, our baseline conclusions about the impacts of

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<sup>38</sup>The World War I immigration years cover the period 1915-1918.

the quota system on population growth and labor productivity growth in the manufacturing sector are robust to controlling for the possible confounding effects of these two pre-quota immigration shocks. Finally, it is important to note that we take a conservative view when considering the immigration shock due to World War I as being permanent, since the data indicate that it was only temporary of nature (i.e., the flow of immigrants immediately increased to pre-WWI levels at the end of the war; see Figure 2 or Ferenczi and Willcox, 1929).

*[Table 4 about here]*

## 5 Individual Level Results

### 5.1 Economic status of native workers

Table 5 examines how the quota policy-driven reduction of immigrant labor influenced native earnings. The estimating equation is (6) and the estimation method is least squares. We use the Lebergott earnings score in Panel A of Table 5 and the 1940 earnings score based on the IPUMS full count sample in Panel B as proxy for the economic status of native workers. All specifications of Table 5 control for marital status, place of residence (rural/urban), race, sex, literacy, fixed effects for age and birthplace, time fixed effects, state-by-time fixed effects, county fixed effects, and 1910 (log) county population interacted by time fixed effects. As in Table 4, we also account for the exposure to World War I and the Literacy Act of 1917.<sup>39</sup> The sample spans the period 1900-1940 and is restricted to 15-65-year-old U.S.-born workers reporting an occupation in the Census.<sup>40</sup>

Panel A of Table 5 summarizes the results for the Lebergott earnings score. As described in Section 3.1, this measure varies across broad industry categories and over time. Overall, native workers experienced a substantial decline in their earnings after the implementation of the quota system in more affected counties, but this effect varies substantially by gender and race. The estimates in column 1 indicate that native workers on average experienced a 1.85 percent decline in earnings after the quota system was implemented, compared to native workers living in an county with a one-standard deviation lower exposure to the quota system, but with otherwise similar observable characteristics. In column 2, we exclude workers born outside the current state of residence in order to understand the importance of population sorting as a response to the shock. For example, one could argue that

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<sup>39</sup>Results remain unaffected when we omit exposure to World War I and the Literacy Act of 1917 in all specifications.

<sup>40</sup>The sample includes only native white and black workers, who comprise 99 percent of all native workers over the period 1900-1940.

part of the baseline estimate is driven by in-migration of lower-skilled native workers from less quota-exposed counties who replaced immigrant labor in the more quota-exposed counties. The result presented in column 2 suggests that interstate migration is not driving our finding, since the coefficient of interest remains statistically significant at the 1-percent level and similar in magnitude. Still, we need to acknowledge that with the data at hand we cannot rule out whether native workers relocated within the same state as a response to the implementation of the quota system. Column 3 shows that interstate migrants in more affected counties also experienced earning losses relative to interstate migrants in less affected areas, albeit the coefficient is smaller in absolute magnitude compared to the baseline estimate.<sup>41</sup> Columns 4 and 5 summarize our results by gender. Only male workers experienced significant earnings losses after the implementation of the quota system in more affected counties: A one-standard-deviation increase in quota exposure reduced the earnings score for native male workers by 1.94 percent, which seems plausible since European immigration was strongly male biased (Angrist, 2001).

The estimates presented in columns 6 and 7 reveal heterogeneous effects by race. For these specifications, we add a triple-interaction term,  $Quota\ exposure_c \times I_t^{post} \times black_i$ , to the baseline estimating equation (6), where  $black_i$  is an indicator variable whether individual  $i$ 's race is black. The specification in column 6 further includes race-by-time and race-by-county fixed effects, which absorb any race specific trends and location specific effects by race that occurred during our sample period. Quota exposure and the triple-interaction term are statistically significant at the 1-percent level with opposite signs. The shortage of immigrant labor substantially decreased the earnings of white native workers in the more affected counties, whereas there is no statistically significant difference in the earnings of black workers between more and less affected counties. The effect on black workers, however, becomes positive and statistically significant once the sample is split by gender. In particular, Appendix Table 3 (Panel A) columns 1 and 2 show that there is a statistically significant increase in black earnings for men and women after the implementation of the quota system in the more affected counties. Both earning losses of white native workers and the economic gains of black workers contributed to a narrowing down of the black-white earnings gap in the more affected counties over the period 1900-1940. This can be seen in column 7 of Panel A, which only exploits the within-county variation of the data (i.e., compared to column 6 this specification also includes county-by-time fixed effects). Conceptually, this specification compares the earnings score between black and white workers living within the same county who are equally exposed to the quota system. The estimated coefficient on

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<sup>41</sup>We note that it is not possible to infer from the Census whether interstate migrants moved because of the quota system.

the triple-interaction term,  $Quota\ exposure_c \times I_t^{post} \times black_i$ , is positive and statistically significant at the 1-percent level (note, that the main effect is absorbed by the county-by-time fixed effects). For the average level of quota exposure, the earnings score of black workers increased up to 2 percent relative to white native workers over the sample period. Reassuringly, we obtain the same insights using the 1940 earnings score in Panel B of Table 5 and in Panel B of Appendix Table 3.

Overall, the evidence presented in Table 5 demonstrates that the quota system pushed the average native white worker into lower paid occupations suggesting some degree of complementarity between native white and immigrant labor. Black workers, on the other hand, were to a large extent substitutes for (unskilled) immigrant workers and filled up some of the vacant jobs due to the reduced supply of immigrant labor in the more quota-affected counties. These findings are also consistent with the conditional earnings distribution in the pre-quota period (1900-1920), where white native workers were at the top, immigrants in the middle, and black workers at the bottom of the earnings distribution (available upon request). Our results indicate that the average black worker benefited from the implementation of the quota system in the more affected counties suggesting that this fundamental change in U.S. immigration policy contributed to a black-white income convergence in the more affected counties over the 1900-1940 period.

*[Table 5 and Appendix Table 3 about here]*

## 5.2 Interstate migration

Table 6 documents whether the implementation of the quota system triggered interstate migration. The outcome variable is an indicator variable for whether a 15-65-year-old U.S.-born worker lives outside his/her state of birth. The estimating equation is (6) and the method of estimation is least squares. The result presented in column 1 shows no evidence of interstate migration being systematically related to quota exposure. This is also the case when we add in columns 2 and 3 the triple-interaction term  $Quota\ exposure_c \times I_t^{post} \times black_i$  to the baseline estimating equation (6) in order to capture differences in internal population movements by race. Overall, the evidence presented in Table 6 indicates that the quota system was not an important trigger of interstate population movements. Having said that, there are two important caveats to this non-finding: First, all specifications in Table 6 control for immigration shocks related to World War I and the Literacy Act of 1917, and consistent with the hypothesis that European immigration delayed the Great Migration (Collins, 1997) the World War I

shock variable is positively related to black interstate migration (not reported).<sup>42</sup> Second, the interstate migration indicator-variable used for Table 6 is not an ideal measure of in-migration, since it does not allow us to check for intrastate migration. In fact, exploiting the linked sample (1910-1930) of Collins and Wanamaker (2014) reveals that black migrants were more likely to opt for places exposed to the quota system, however, this relationship is not robust to the WWI migration-shock variable, which generally supports our findings reported in this section (not reported). We finally note that the results on the earnings score of black workers in Table 5 are not driven by positively selected black workers born outside the current state of residence. Appendix Table 3, columns 3 and 4 reveal that the positive effect on the black earnings score in the more affected counties is exclusively driven by black workers born in the current state of residence, while the Lebergott earnings score of black workers born outside the state of residence is not statistically different from that of their white counterparts.

*[Table 6 about here]*

### **5.3 Marital status and fertility**

Table 7 provides evidence that the implementation of the quota system decreased the likelihood of marriage and reduced fertility. The basic argument put forward in this subsection is that the quota system worsened marriage-market conditions among first- and second-generation immigrant women, because their marriages were mostly endogamous and immigration streams from Europe to the United States were male-biased (e.g., Angrist 2002; Lafortune, 2013). Since these immigrant groups accounted for a relatively large share of the total U.S. reproductive population at that time, changes in their fertility behavior were likely to be an important component of aggregate fertility changes during this time period.

The first three columns of Table 7 summarize the results on the likelihood of sampling a married woman over the period 1900-1940. The sample includes white and black women aged 15-49.<sup>43</sup> The estimating equation is (6) and the method of estimation is least squares. All specifications of Table 7 control for place of residence (rural/urban), race, sex, fixed effects for age and birthplace, time fixed effects, state-by-time fixed effects, county fixed effects, 1910 (log) county population interacted by time, and exposure to World War I and the Literacy Act of 1917. Column 1 shows that after the

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<sup>42</sup>Using the more traditional Bartik-style approach reveals that (European) immigration in fact crowded out black migration, which is perhaps not surprising given that the identification strategy of the Bartik-style approach includes both the World War I shock and the quota shock altogether with all other immigration shocks happening during this time period (not reported).

<sup>43</sup>Results remain unchanged if we include women from other races to the sample (not reported).



implementation of the quota system there was a substantial decline of marriage rates in more affected counties relative to less affected counties. A one-standard-deviation increase in quota exposure reduced the likelihood of sampling a married woman by 0.74 percentage points. In column 2, we add the triple-interaction term,  $Quota\ exposure_c \times I_t^{post} \times FGSG_i$ , to the baseline estimating equation (6), where  $FGSG_i$  is an indicator variable for being a first- or second-generation immigrant women. The triple-interaction term captures differences in the likelihood of being married by immigration status. The specification in column 2 further includes interaction terms of the immigration status indicator  $FGSG_i$  by time and by county. Consistent with the above-mentioned argument and the evidence in Angrist (2002) and Lafortune (2013), we find that the negative marriage effect is driven by first- and second-generation immigrant women who already lived in the United States prior to the quota system (not reported). While there is no statistically significant effect of quota exposure on the likelihood of U.S.-born women being married, the joint effect on  $Quota\ exposure_c \times I_t^{post}$  and  $Quota\ exposure_c \times I_t^{post} \times FGSG_i$  is negative and statistically significant at the 1-percent level. Column 3 summarizes the result when we only exploit the within-county variation of the data. Once county-by-time fixed effects are added to the specification we find no statistically significant differences in the likelihood of being married by immigration status.

The three remaining columns of Table 7 paint a coherent picture suggesting that the quota-induced decline in marriage rates is associated with reduced fertility. Our measure of fertility is the number of own children below the age of five living in the household. Since the sample is restricted to include women of age 15-49, this fertility outcome corresponds to the general fertility rate measured over a five-year period.<sup>44</sup> Column 4 reports a negative and statistically significant effect of quota exposure on fertility at the 1-percent level. The magnitude of the estimated coefficient implies that a one-standard deviation increase in quota exposure is associated with a decrease of 0.03 children per woman aged 15-49. This can be compared to an aggregate decline in fertility from the pre- to the post-quota period of 0.12 children per woman aged 15-49 (in our sample), suggesting an important role of the quota system for the overall fertility decline. The remaining columns of Table 6 demonstrate that the negative effect on fertility is mainly driven by first- and second-generation immigrant women.

Our findings are generally consistent with Angrist (2002), who shows that a decrease in the sex ratio has negative effects on marriages and the number of children (of all ages) living in the household. Angrist's estimation strategy is based on national-ethnic marriage markets, while our estimation strategy builds on the existence of local ethnic marriage markets which allows us to control for a tighter

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<sup>44</sup>This fertility measure has, for example, previously been used by Bleakley and Lange (2009).

set of fixed effects. We also apply a DiD strategy with the purpose of isolating the effect of introducing the quota system on marriage and fertility, whereas Angrist’s inference is based on a Bartik style instrumental variable approach.<sup>45</sup> Overall, our results indicate that the implementation of the quota system led to a decline in population growth through the reduced supply of immigrants from affected nationalities as well as lower fertility of first- and second-generation immigrant women in part due to changes in the local marriage market conditions over the sample period.

*[Table 7 about here]*

Finally, it is worthwhile to note that the main findings of Section 5 do not depend exclusively on our preferred estimation strategy. Appendix Table 4 replicates the main specifications of Tables 5-7 based on the Bartik-style approach (we refer to Appendix C for the details). Reassuringly, these estimates confirm the main findings of Section 5.

## 6 Conclusion

The 1920s were marked by a fundamental change in U.S. immigration policy. The passage of the Emergency Quota Act of 1921 and the Immigration Act of 1924 ended the era of unrestricted immigration from Europe to the United States (Goldin, 1994). While economic historians have investigated the political economy of immigration restrictions in the United States (Goldin, 1994; Timmer and Williamson, 1996; 1998) and explored how the quota acts affected migrant selection and return migration (Greenwood and Ward, 2015; Massey 2016), a rigorous quantitative assessment of the macroeconomic consequences of these immigration restrictions for the U.S. economy focusing on local economies has been missing so far. This paper aims to fill this gap in the literature. The passage of the quota acts in the 1920s implied that some nationalities—mainly from Europe—were to a different extent affected by these laws, while for other nationalities, such as Canadians or Mexicans, immigration to the United States still remained open without any restrictions. Our empirical analysis exploited this variation along with the spatial distribution of different pre-existing nationality networks across the United States to evaluate how key drivers of economic growth, such as population and labor productivity growth, responded to the implementation of the quota system.

We found that the implementation of the quota system had a negative effect on population growth.

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<sup>45</sup>Our analysis also draws on larger Census samples from 1900 to 1940, while Angrist, due to limited data availability some 20 years ago, was only able to use 1-percent or even smaller random samples for 1910, 1920, and 1940.

More affected counties experienced a long-lasting decline in population growth relative to less affected counties. We demonstrated that this decline is mainly due to the asymmetric effect of reduced immigration inflows from quota-affected nationalities across counties. First- and second generation immigrant women also reduced fertility in the more affected counties, which reinforced the negative effect of the quota system on population growth. The manufacturing sector experienced in the more affected urban areas a substantial decline in labor productivity growth. Since immigrants predominantly settled in urban areas and thereby increased the density of economic activity (e.g., Ciccone and Hall, 1996) the decline in labor productivity in the more affected urban areas might be a result of agglomeration externalities due to a shrinking of the manufacturing sector or because firms did not significantly adjust their capital intensity after the introduction of the quota system.

The shutdown of large-scale immigration from Europe to the United States during the 1920s also significantly reduced the earnings of an average U.S.-born worker. The overall negative effect on native earnings turned out to substantially differ by race. After the implementation of the quota system white native workers experienced sizable earnings losses in the more affected counties. This finding could indicate that white U.S.-born workers and immigrant labor were to some extent complements in the production process at that time. On the other hand, black workers, who were closer substitutes to (unskilled) immigrant labor benefited from the immigration restrictions and increased their relative economic status in the more affected counties during the post-quota period. Our finding suggests that the quota system, which had a differential impact on the supply of immigrant labor across counties in the United States, might have triggered some black-white income convergence before the 1940s. Overall, we conclude that such a fundamental change in immigration policy generates winners and losers depending on how much they stand in competition with immigrant workers.

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# Appendix

## A. Accounting for the effect on population via immigration

This section outlines an accounting exercise for the effect of the quota system on population growth via immigration rates, using the so-called fundamental demographic equation. The fundamental demographic equation is given by:

$$P_{ct+1} = P_{ct} + B_{ct} - D_{ct} + I_{ct} - X_{ct}, \quad (7)$$

where  $P_{ct+1}$  is the population size in year  $t + 1$  in county  $c$ ,  $B_{ct}$  is the total number of births,  $D_{ct}$  is the total number of deaths,  $I_{ct}$  is the total number of immigrants (and in-migrants), and  $X_{ct}$  is the total number of emigrants (and out-migrants). Equation (7) can be rearranged such that:

$$\hat{p}_{ct+1} = b_{ct} - d_{ct} + i_{ct} - x_{ct}, \quad (8)$$

where  $\hat{p}_{t+1} \equiv (P_{ct+1} - P_{ct})/P_{ct}$  is the population growth rate in county  $c$ ,  $b_{ct} \equiv B_{ct}/P_{ct}$  is the crude birth rate,  $d_{ct} \equiv D_{ct}/P_{ct}$  is the crude death rate,  $i_{ct} \equiv I_{ct}/P_{ct}$  is the immigration rate, and  $x_{ct} \equiv X_{ct}/P_{ct}$  is the emigration rate. We argue that—among other things—all the rates in equation (8) are functions of the quota system  $Q_t$ :

$$\hat{p}_{ct+1}(Q_{ct}, b_{ct}, d_{ct}, i_{ct}, x_{ct}) = b_{ct}(Q_{ct}) - d_{ct}(Q_{ct}) + i_{ct}(Q_{ct}) - x_{ct}(Q_{ct}). \quad (9)$$

Differentiating this equation with respect to the quota system yields the following expression:

$$\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}} = \frac{\partial b_{ct}}{\partial Q_{ct}} - \frac{\partial d_{ct}}{\partial Q_{ct}} + \frac{\partial i_{ct}}{\partial Q_{ct}} - \frac{\partial x_{ct}}{\partial Q_{ct}}, \quad (10)$$

where the arguments have been suppressed for simplicity. Table 2 reports estimates for  $\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}}$ , which, taken at face value, give the total effect of all the changes in the demographic components due to the implementation of the quota system. Using the annual county data on immigration inflows (used to construct Figure 2), we obtain immigration rates at the county-by-decade level and estimate a model similar to equation (4) in the paper. The results are reported in Appendix Table 1. For the total sample, we find that  $\frac{\partial i_{ct}}{\partial Q_t} = -0.35$  (standard error = 0.05), while  $\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}} = -1.05$  (standard error = 0.16), which is smaller compared to the DiD estimates reported in Panel B of Table 2. However, in order to facilitate the comparison, Appendix Table 1 reports estimates from specifications that restrict

the samples to include the same counties and the same controls.<sup>46</sup> Comparing the two point estimates, we see that the effect on population growth is significantly stronger than the effect on immigration rates, but the direct effect on immigration still explains about 33 percent of total population growth effect. Taking uncertainty into account, this suggests that much of the effect on population growth can be explained by the direct effect of the quota system on immigration. In addition, Table 6 provided evidence suggesting that  $\frac{\partial b_{ct}}{\partial Q_{ct}} < 0$ , so we do not need large effects on in- and out-migration and mortality rates to explain our findings for population growth.

[Appendix Table 1 about here]

## **B. Immigrant-admitted data vs. census-immigrant data**

Appendix Figure 1 compares data on the flow of immigrants from Ferenczi and Willcox (1929) to the flow of immigrants derived using micro data from IPUMS for selected nationalities (i.e., Germans, Dutch, Danes, Swedes, Italians, Romanians, and Greeks). The immigrant data from IPUMS are derived from the full count 1920 and 1930 censuses, using the *YRIMMIG* and *BPL* variables. The *YRIMMIG* variable reports the year in which a foreign-born person (*BPL* codes 150-950) emigrated to the United States. We then collapsed the individual-level data by birthplace and immigration year in order to obtain the IPUMS data series shown in Appendix Figure 1. Notice that in order to avoid any double counting, the 1920 Census is used for the years 1900 to 1919, while the 1930 Census is used for the remaining years, 1920 to 1929. Any differences between the two data series might reflect 1) measurement error, most likely in the micro data due to, e.g., age heaping and 2) return migration and death, which should become more pronounced for the years further away from the census years 1920 and 1930.

For 1900, there is clear evidence of age heaping since the inflow measure based on IPUMS shows an immediate drop the following years for all nationalities, whereas the Ferenczi and Willcox series shows that the inflow was actually increasing at the start of the 20th century. However, we generally find that the two data series are trending in very similar ways for all nationalities, suggesting that the IPUMS based measure is actually capturing the flows of immigrants, which we believe validates our approach in Figure 1. It is important to keep in mind that we are only using the IPUMS inflow

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<sup>46</sup>In a model with no initial controls, the relative sizes of the estimates are similar to the ones reported in Appendix Table 1, for example.

measure in Figure 1 (and Appendix Table 1) and not in the remainder of our empirical analysis.

*[Appendix Figure 1 about here]*

### **C. Bartik-style approach**

As mentioned in Section 3, our estimation strategy has similarities to the classical shift-share instrument also known as the so-called Bartik-style approach (Bartik, 1991). However, it is not the same since we focus on isolating the immigration shock coming from the introduction of the quota system. Appendix Table 2 reports the results from regressing our county outcomes on changes in the predicted foreign-born share, which then corresponds to the reduced-form estimates in a Bartik-style instrumental variable approach. To facilitate a comparison to our DiD strategy, we exploit the county foreign-born share in 1910. We then predict the foreign-born share backwards and forwards using the aggregate immigration flows by nationality from the Ferenczi and Willcox series.

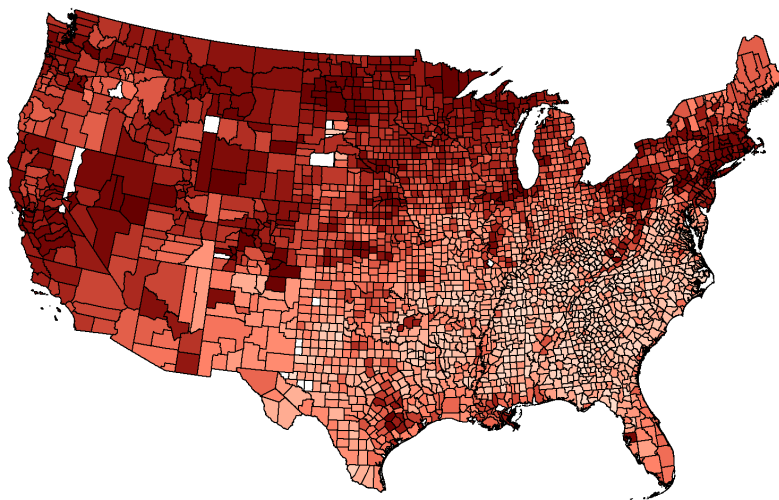
Overall, these “Bartik-style” estimates largely confirm our DiD findings of Tables 2 and 3. Columns 1-4 of Appendix Table 2 summarize the results for all counties, while columns 5-8 present the results for urban counties. As a point of reference, columns 1 and 5 show that areas more exposed to the quota system experienced large declines in the change of the actual foreign-born share. In other words, our DiD strategy, in fact, captures a decline in the foreign-born share, which we argue is caused by the introduction of the quota system. The other columns refer to the Bartik approach. The estimates in columns 2 and 6 reveal a positive and statistically significant relationship between the changes in the actual and predicted foreign-born shares. This would constitute a first-stage relationship in a Bartik-style instrumental variable approach. The remaining columns show that changes in the predicted foreign-born share are statistically significant and positively related to population growth and to labor productivity growth in urban counties.

*[Appendix Table 2 about here]*

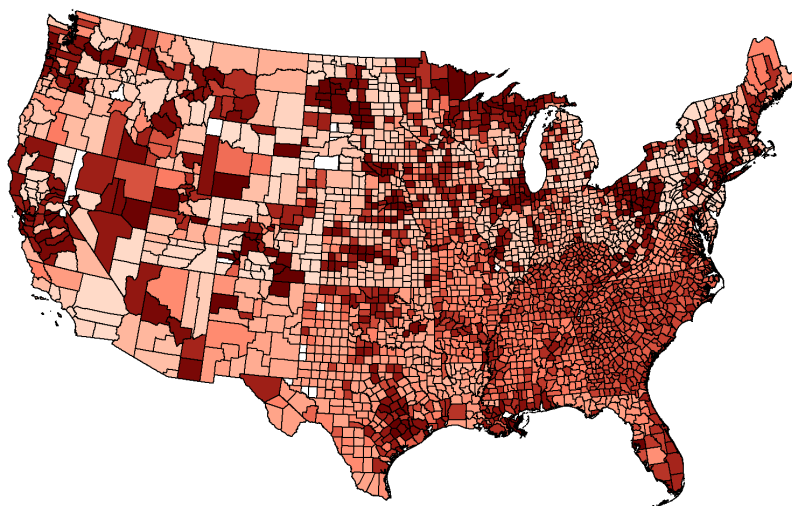
Appendix Table 4 presents our results for the main variables of interest in Section 5 based on the Bartik-style approach. Again, these “Bartik-style” estimates are in line with our DiD findings of Tables 5 to 7.

*[Appendix Table 4 about here]*

**Figure 1: Quota exposure**



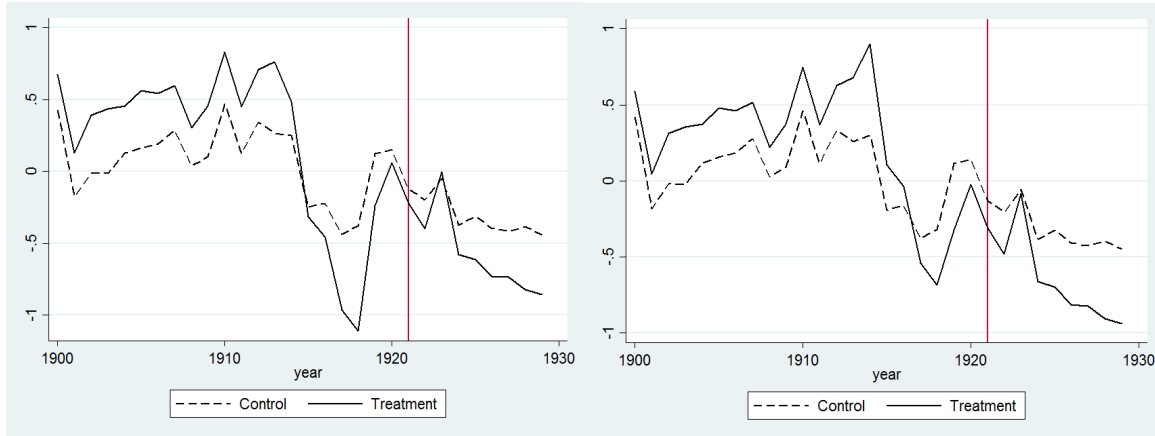
Panel A: Quota exposure.



Panel B: Quota exposure conditional on state fixed effects.

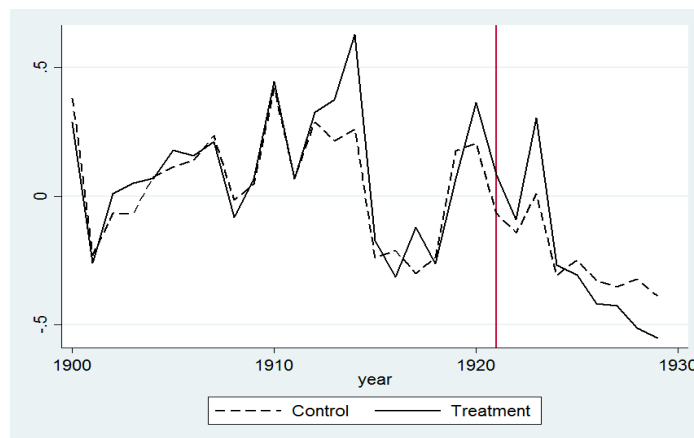
Notes: These maps show the quota-exposure variable, which is calculated as  $\sum FB_{nc} \times Quota_n$ . The quota exposure in Panel B is conditional on state fixed effects. A darker red color reflects a more exposed area in terms of restricting potential immigration.

**Figure 2:** Average annual number of immigrants by Quota exposure



Panel A: County fixed effects

Panel B: County fixed effects and WWI control



Panel C: County fixed effects, WWI control, and Literacy Act control

Notes: This figure depicts the average annual number of immigrants by quota exposure for all counties in the total sample. The treatment (control) group includes counties above (below) median quota exposure. The number of immigrants at the county level is constructed using the variable YRIMMIG for the 1920 and 1930 full-count samples from IPUMS. Panel A controls for county fixed effects; Panel B controls for county fixed effects and the effect of WWI on immigration to the US; and Panel C controls for county fixed effects and the effects of WWI and the Literacy Act in 1917 on immigration to the US.

**Table 1: Summary Statistics**

	(1) N	(2) mean	(3) sd	(4) min	(5) max
<i>County level</i>					
$\Delta(\text{Ln population})$	5,512	0.0855	0.174	-2.702	1.355
$\Delta(\text{Ln urban population})$	536	0.200	0.215	-0.285	1.240
$\Delta(\text{Ln manufacturing value added per worker})$	5,512	0.228	0.381	-1.580	1.742
Quota exposure	1,378	0.0358	0.0500	0	0.323
<i>City level</i>					
$\Delta(\text{Ln horsepower per worker})$	1,928	0.155	0.387	-2.807	4.322
$\Delta(\text{Ln horsepower per value added})$	1,922	-0.0802	0.510	-3.792	3.482
$\Delta(\text{Ln manufacturing value added per worker})$	1,922	0.236	0.396	-3.261	4.288
Quota exposure	482	0.0712	0.0737	0	0.525
<i>Individual level</i>					
Ln earnings score (Lebergott)	1,586,952	6.006	0.655	3.548	7.058
Ln occupation score (1940)	1,625,366	6.716	0.623	4.730	7.711
== 1 if Born out of state	1,629,685	0.291	0.454	0	1
== 1 if Married	1,388,127	0.614	0.487	0	1
Number of children below age 5	1,388,127	0.368	0.708	0	7

Note: Quota exposure only varies at the cross-sectional dimension. See Section 3 for further details.



**Table 2: Effect on Population Growth**

	<i>Dependent variable</i>					
	<i>Δ(Ln population)</i>			<i>Δ(Ln urban population)</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: flexible DiD estimates</i>						
Quota exposure x 1910	0.566*** (0.106)	-0.230* (0.131)	-0.321* (0.166)	0.259 (0.309)	-0.191 (0.294)	-0.508 (0.404)
Quota exposure x 1930	-0.291*** (0.0998)	-0.386*** (0.143)	-0.626*** (0.148)	-0.600** (0.238)	-0.823*** (0.289)	-1.013*** (0.379)
Quota exposure x 1940	-0.827*** (0.107)	-0.331** (0.139)	-0.545*** (0.166)	-0.510* (0.287)	-0.603** (0.292)	-0.780* (0.400)
Controls	See below	See below	See below	See below	See below	See below
Observations	5,512	5,512	5,512	536	536	480
R-squared	0.518	0.716	0.755	0.623	0.802	0.850
<i>Panel B: non-flexible DiD estimates</i>						
Quota exposure	-0.842*** (0.0856)	-0.244*** (0.0913)	-0.425*** (0.0878)	-0.685*** (0.172)	-0.618*** (0.145)	-0.642*** (0.189)
Initial population size x time FE	No	Yes	Yes	No	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-time FE	No	No	Yes	No	No	Yes
Observations	5,512	5,512	5,512	536	536	480
R-squared	0.512	0.716	0.755	0.622	0.802	0.848

Notes: The table reports "flexible" DiD estimates (Panel A) relative to the omitted year 1920 and "non-flexible" DiD estimates (Panel B). The observations are at the county level for the decades 1910 to 1940. Quota exposure is the sum over N foreign-born shares in a county  $c$  each interacted with the corresponding quota intensity. All specifications include county and time fixed effects. Additional controls are state-by-time fixed effects in columns 3 and 6. The total sample includes all counties for which there exist data for all the years, whereas the urban sample only includes counties with more than 25,000 inhabitants in 1900. In column 6, 14 counties drop out, as we here control for state-by-time fixed effects, which requires at the least two counties per state for identification. The outcome variable is the change in log population size (in columns 1-3) and the change in log urban population (in columns 4-6). We include initial ln population size in 1900 and 1910 interacted with a full set of time fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 3: Effect on the Manufacturing Sector**

	<i>Dependent variable</i>						
	<i>Δ(ln manufacturing value added per worker)</i>				<i>Δ(Horsepower per worker or per va)</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quota exposure x I <sup>post</sup>	-0.623* (0.350)	-1.201*** (0.316)	-1.184*** (0.320)	-0.589*** (0.199)	-0.568*** (0.195)	-0.482* (0.283)	-0.108 (0.312)
Sample	Counties	Urban counties	Urban counties Northern states	Cities	Cities Northern states	Cities	Cities
Initial outcome x time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	No	No	No	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	No	No	No	No
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,512	480	408	1,920	1,568	1,928	1,920
R-squared	0.589	0.869	0.876	0.589	0.557	0.583	0.620

Notes: The table reports "non-flexible" DiD estimates. The observations are at the county level for the decades 1910 to 1940 (columns 1-3) and at the city level for the years 1914, 1919, 1925, and 1929 (columns 4-7). The outcome variable is the change in log manufacturing value added per worker (in columns 1-5) and the change in horsepower per worker (columns 6) and horsepower per value added (columns 7). Quota exposure is the sum over N foreign-born shares in a county (city), c, each interacted with the corresponding quota intensity. Initial outcomes are the outcomes in levels measured in 1900 and 1910 at the county level and in 1909 and 1914 at the city level interacted with a full set of time fixed effects. All regressions also include ln population in 1910 interacted with a full set of time fixed effects. Note that linear interpolation was used for the 1910 production data at the county level and for horsepower at the city level for the year 1925. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county (city) level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 4: Robustness to WWI and Literacy Act of 1917**

	<i>Dependent variable</i>						
	$\Delta(\ln \text{ population})$	$\Delta(\ln \text{ mfg va per worker})$	$\Delta(\ln \text{ urban population})$	$\Delta(\ln \text{ mfg va per worker})$	$\Delta(\ln \text{ mfg va per worker})$	$\Delta(\text{Horsepower per worker or per va})$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quota exposure x $I^{\text{post}}$	-0.664*** (0.126)	-0.766 (0.466)	-0.913*** (0.325)	-1.434*** (0.440)	-0.762*** (0.251)	-0.624 (0.413)	-0.210 (0.412)
Sample	Counties		Urban counties		Cities		
WWI exposure x $I^{\text{post}}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Literacy act of 1917 exposure x $I^{\text{post}}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls as Table 2	Yes	No	Yes	No	No	No	No
Controls as Table 3	No	Yes	No	Yes	Yes	Yes	Yes
Observations	5,512	5,512	480	480	1,920	1,928	1,920
R-squared	0.756	0.589	0.851	0.869	0.589	0.584	0.620

Notes: The table reports "non-flexible" DiD estimates. The observations are at the county level for the decades 1910 to 1940 (columns 1-4) and at the city level for the years 1914, 1919, 1925, and 1929 (columns 5-7). The outcome variables are indicated at the top row of the table. Quota exposure is the sum over  $N$  foreign-born shares in a county (city),  $c$ , each interacted with the corresponding quota intensity. See Section 4.3 for the construction of WWI exposure and Literacy Act exposure. See columns 3 and 6 of Table 2 and Table 3 for further details on the controls. Note that linear interpolation was used for the 1910 production data at the county level and for horsepower at the city level for the year 1925. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county (city) level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 5: Effect on the Earnings Scores of Native Workers**

<i>Dependent variable: Earnings scores</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Lebergott earnings score</i>							
Quota exposure x I <sup>post</sup>	-0.244*** (0.0486)	-0.264*** (0.0497)	-0.150*** (0.0572)	-0.256*** (0.0554)	-0.0471 (0.0504)	-0.257*** (0.0488)	
Quota exposure x I <sup>post</sup> x black						0.236*** (0.0774)	0.288*** (0.0841)
Joint Effect						-0.021 (0.0962)	
Sample	All	Lives in state of birth	Lives out of state of birth	Men	Women	All	All
Controls	See below	See below	See below	See below	See below	See below	See below
Observations	1,569,014	1,113,921	455,075	1,205,260	363,734	1,568,797	1,582,488
R-squared	0.642	0.659	0.583	0.572	0.754	0.647	0.657
<i>Panel B: 1940 earnings score</i>							
Quota exposure x I <sup>post</sup>	-0.162*** (0.0332)	-0.164*** (0.0381)	-0.119*** (0.0358)	-0.195*** (0.0308)	-0.0373 (0.0746)	-0.186*** (0.0298)	
Quota exposure x I <sup>post</sup> x black						0.320*** (0.0499)	0.171*** (0.0434)
Joint Effect						0.134*** (0.0468)	
Sample	All	Lives in state of birth	Lives out of state of birth	Men	Women	All	All
WWI exposure x I <sup>post</sup>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Literacy Act of 1917 exposure x I <sup>post</sup>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,610,434	1,143,034	467,377	1,239,118	371,297	1,610,224	1,624,151
R-squared	0.651	0.668	0.592	0.592	0.712	0.660	0.667

Notes: The table reports "non-flexible" DiD estimates. The observations are at the individual level over the decades 1900 to 1940. The sample spans 15-65-year-old workers. The outcome variable is the Lebergott earnings score (Panel A) and the 1940 earnings score (Panel B); see Section 3 for further details. Quota exposure is the sum over N foreign-born shares in a county, c, each interacted with the corresponding quota intensity. See Section 4.3 for the construction of WWI exposure and exposure to the Literacy Act of 1917. All regressions include county fixed effects, time fixed effects, and state-by-time fixed effects. The set of individual controls includes the following indicator variables: marital status, place of residence (rural/urban), gender, race (black/white), literacy. We further include birthplace and age fixed effects. Columns 6 and 7 further include quota exposure interacted with a dummy for race (black), race-by-time fixed effects, and race-by-county fixed effects. County-by-time fixed effects are added to the specification in column 7. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 6: Effect on Internal Population Movements**

	<i>Dependent variable</i>		
	<i>= 1 if Born outside of state of residence</i>		
	(1)	(2)	(3)
Quota exposure x $I^{post}$	-0.0544 (0.0542)	-0.0764 (0.0516)	
Quota exposure x $I^{post}$ x black		0.0508 (0.114)	-0.110 (0.0996)
Joint Effect		-0.0255 (0.0859)	
WWI exposure x $I^{post}$	Yes	Yes	Yes
Literacy Act of 1917 exposure x $I^{post}$	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes
Observations	1,615,610	1,615,400	1,615,340
R-squared	0.346	0.362	0.371

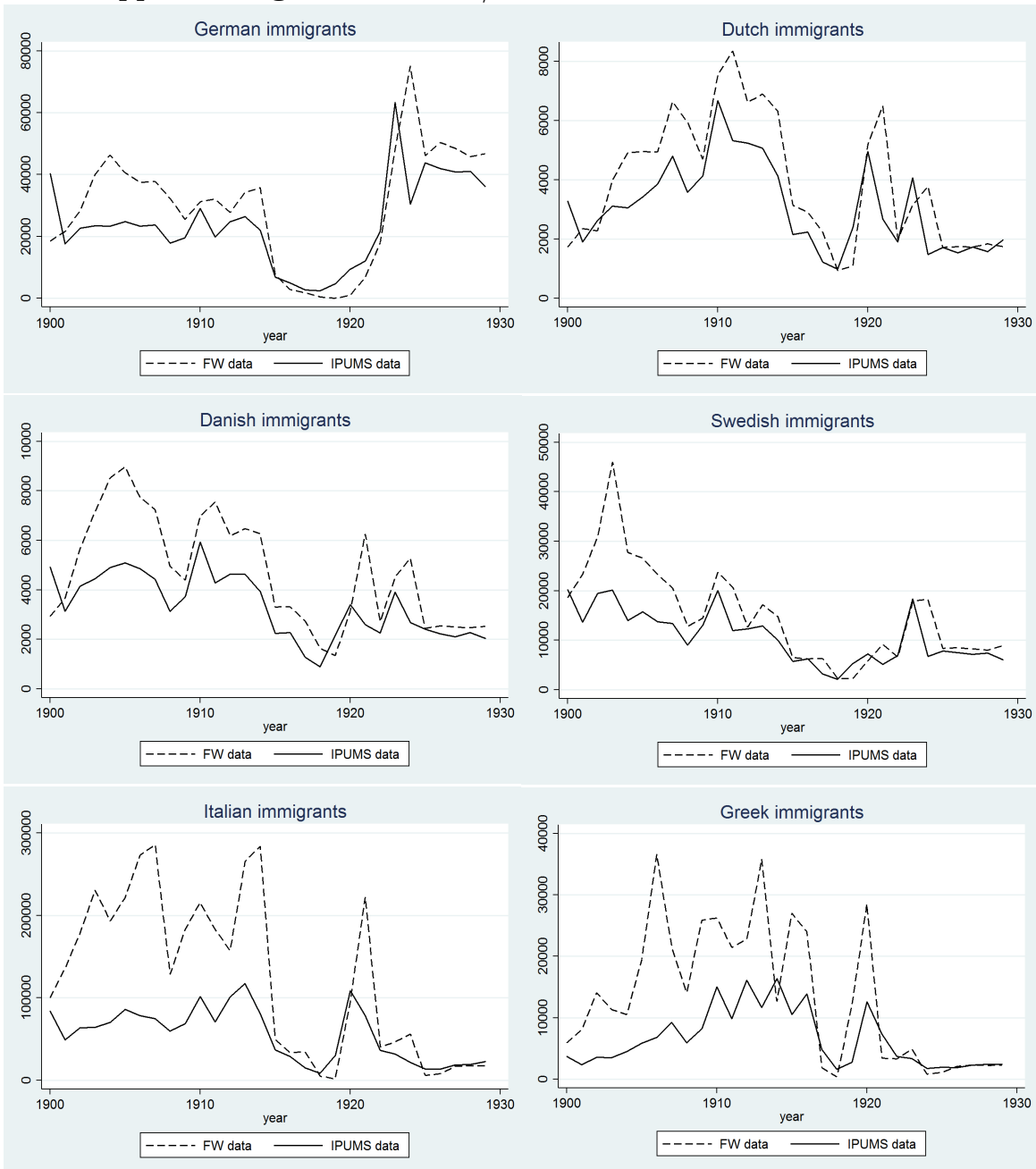
Notes: The table reports "non-flexible" DiD estimates. The observations are at the individual level for the decades 1900 to 1940. The sample spans 15-65-year-old workers. The outcome variable is the likelihood of living outside the state of birth. Quota exposure is the sum over N foreign-born shares in a county, c, each interacted with the corresponding quota intensity. See Section 4.3 for the construction of WWI exposure and exposure to the Literacy Act of 1917. All regressions include county fixed effects, time fixed effects, and state-by-time fixed effects. The set of individual controls includes the following indicator variables: marital status, place of residence (rural/urban), gender, race (black/white), literacy. We further include birthplace and age fixed effects. Columns 2 and 3 further include quota exposure interacted with a dummy for race (black), race-by-time fixed effects, and race-by-county fixed effects. County-by-time fixed effects are added to the specification in column 3. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 7: Effect on Marital Status and Fertility**

	<i>Dependent variable</i>					
	(1)	<i>=1 if Married</i> (2)	(3)	(4)	<i>Number of children below age 5</i> (5) (6)	
Quota exposure x $I^{post}$	-0.0978*** (0.0258)	-0.0478 (0.0336)		-0.390*** (0.0527)	-0.0612 (0.0373)	
Quota exposure x $I^{post}$ x FGSG		-0.0407 (0.0386)	-0.0192 (0.0360)		-0.450*** (0.0857)	-0.386*** (0.0928)
Joint Effect		-0.088*** (0.027)			-0.512*** (0.091)	
WWI exposure x $I^{post}$	Yes	Yes	Yes	Yes	Yes	Yes
Literacy Act of 1917 exposure x $I^{post}$	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,367,632	1,367,428	1,367,358	1,367,632	1,367,428	1,367,358
R-squared	0.314	0.317	0.324	0.153	0.157	0.167

Notes: The table reports "non-flexible" DiD estimates. The observations are at the individual level over the decades 1900 to 1940. The sample spans 15-49-year-old women. The outcome variable is the likelihood of being married (columns 1-3) and the number of own children below age 5 (columns 4-6). Quota exposure is the sum over N foreign-born shares in a county, c, each interacted with the corresponding quota intensity. All regressions include county fixed effects, time fixed effects, and state-by-time fixed effects. The set of individual controls includes a dummy for race (black/white), place of residence (rural/urban), age fixed effects, and birthplace fixed effects. Columns 2, 3, 5, and 6 further include quota exposure interacted with a dummy for first- and second generation women (FGSG), FGSG-by-time fixed effects, and FGSG-by-county fixed effects. County-by-time fixed effects are added to the specifications in columns 3 and 6. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Appendix Figure 1: Ferenczi/Willcox data vs. IPUMS census data**



**Appendix Table 1: Effect on Immigration Rates**

	<i>Dependent variable</i>			
	<i>Immigration rate</i>	<i>Δ(Ln population)</i>	<i>Immigration rate (urban)</i>	<i>Δ(Ln urban population)</i>
	(1)	(2)	(3)	(4)
Quota exposure x $I^{post}$	-0.349*** (0.0462)	-1.044*** (0.161)	-0.348*** (0.111)	-0.657** (0.302)
Initial population size x time FE	Yes	Yes	Yes	No
Initial number of immigrants x time FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes
Observations	4,122	4,122	360	360
R-squared	0.876	0.688	0.913	0.748

Notes: The observations are at the county level for the decades 1910 to 1930. Quota exposure is the sum over  $N$  foreign-born shares in a county,  $c$ , each interacted with the corresponding quota intensity. All regressions include county, time fixed effects, and state-by-time fixed effects. The outcome variable is the immigration rate (columns 1 and 3) and the change in log population size (in columns 2 and 4). We include initial  $\ln$  population size in 1910 and initial  $\ln$  number of immigrants in 1910 interacted with a full set of time fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



**Appendix Table 2: Bartik-style Approach (county level)**

	<i>Dependent variable</i>							
	$\Delta(\text{FB share})$	$\Delta(\text{FB share})$	$\Delta(\text{Ln pop})$	$\Delta(\text{Ln mfg va pw})$	$\Delta(\text{FB share})$	$\Delta(\text{FB share})$	$\Delta(\text{Ln urb pop})$	$\Delta(\text{Ln mfg va pw})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quota exposure x $I^{\text{post}}$	-0.312*** (0.0315)				-0.282*** (0.0443)			
$\Delta(\text{Predicted FB share})$		0.208** (0.0881)	0.604*** (0.204)	0.120 (0.266)		0.721*** (0.117)	1.108*** (0.336)	1.396*** (0.458)
Initial population size x time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,134	4,134	4,134	4,134	360	360	360	360
R-squared	0.657	0.651	0.685	0.626	0.803	0.855	0.741	0.827

Notes: The table reports "non-flexible" DiD estimates in columns 1 and 5. Columns 2-4 and 6-8 report estimates using the Bartik-style approach (see Appendix Section C for further details). The observations are at the county level for the decades 1910 to 1930. The outcome variables are indicated at the top row of the table. All specifications include county fixed effects, time fixed effects, state-by-time fixed effects, and initial ln population size in 1910 interacted with a full set of time fixed effects. Note that linear interpolation was used for the 1910 production data at the county level. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Appendix Table 3: Effect on Native Earnings Scores -- Additional Sample Splits**

<i>Dependent variable: Earnings scores</i>				
	(1)	(2)	(3)	(4)
<i>Panel A: Lebergott earnings score</i>				
Quota exposure x I <sup>post</sup>	-0.284*** (0.0547)	-0.0926* (0.0513)	-0.278*** (0.0505)	-0.137** (0.0539)
Quota exposure x I <sup>post</sup> x black	0.629*** (0.122)	0.425*** (0.0982)	0.457*** (0.105)	-0.0301 (0.0669)
Joint Effect	0.345** (0.146)	0.332*** (0.085)	0.179 (0.121)	-0.167** (0.081)
Sample	Male	Female	Born in State	Born out of State
Controls	See below	See below	See below	See below
Observations	1,205,031	363,509	1,113,724	454,763
R-squared	0.578	0.765	0.663	0.591
<i>Panel B: 1940 earnings score</i>				
Quota exposure x I <sup>post</sup>	-0.216*** (0.0300)	-0.0914 (0.0650)	-0.168*** (0.0352)	-0.164*** (0.0360)
Quota exposure x I <sup>post</sup> x black	0.423*** (0.0559)	0.387*** (0.0890)	0.409*** (0.0728)	0.231*** (0.0405)
Joint Effect	0.207*** (0.062)	0.296*** (0.066)	0.241*** (0.070)	0.067 (0.048)
Sample	Male	Female	Born in State	Born out of State
WWI exposure x I <sup>post</sup>	Yes	Yes	Yes	Yes
Literacy Act of 1917 exposure x I <sup>post</sup>	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes
Observations	1,238,898	371,075	1,142,848	467,060
R-squared	0.599	0.730	0.674	0.607

Notes: The table reports "non-flexible" DiD estimates. The observations are at the individual level over the decades 1900 to 1940. The sample spans 15-65-year-old workers. The outcome variable is the Lebergott earnings score (Panel A) and the 1940 earnings score (Panel B); see Section 3 for further details. Quota exposure is the sum over N foreign-born shares in a county, c, each interacted with the corresponding quota intensity. See Section 4.3 for the construction of WWI exposure and exposure to the Literacy Act of 1917. All regressions include county fixed effects, time fixed effects, and state-by-time fixed effects. The set of individual controls includes the following indicator variables: marital status, place of residence (rural/urban), gender, race (black/white), literacy, birthplace and age fixed effects. We further include quota exposure interacted with a dummy for race (black), race-by-time fixed effects, and race-by-county fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Appendix Table 4: Bartik-style Approach (individual level)**

	<i>Dependent variable</i>				
	<i>Lebergott earnings score</i>	<i>1940 earnings score</i>	<i>= 1 if born out of state of residence</i>	<i>=1 if married</i>	<i>Number of children below age 5</i>
	(1)	(2)	(3)	(4)	(5)
$\Delta$ (Predicted FB share)	0.150*** (0.0514)	0.0692** (0.0329)	0.00227 (0.0261)	0.119*** (0.0296)	0.276*** (0.0397)
Initial population size x time FE	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
State-by-time FE	Yes	Yes	Yes	Yes	Yes
Observations	951,603	962,346	965,656	821,595	821,595
R-squared	0.636	0.663	0.372	0.317	0.159

Notes: The table reports estimates using the Bartik-style approach (see Appendix Section C for further details). The observations are at the individual level for the decades 1900 to 1930. The outcome variables are indicated at the top row of the table. All specifications include county fixed effects, time fixed effects, state-by-time fixed effects, and initial ln population size in 1910 interacted with a full set of time fixed effects. For the set of individual controls see the corresponding footnotes tables 5-7. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the county level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.