Discussion Papers
Department of Economics
University of Copenhagen

No. 16-11

National Immigration Quotas and Local Economic Growth

Philipp Ager

Casper Worm Hansen

Øster Farimagsgade 5, Building 26, DK-1353 Copenhagen K., Denmark
Tel.: +45 35 32 30 01 – Fax: +45 35 32 30 00
http://www.econ.ku.dk

ISSN: 1601-2461 (E)

National Immigration Quotas and Local Economic Growth*

Philipp Ager Casper Worm Hansen

[This version: November 2016]

Abstract

The introduction of immigration quotas in the 1920s fundamentally changed US migration policy. We exploit this policy change to estimate the effect of immigration on local economic growth and industry development. Our analysis demonstrates that areas with larger pre-existing communities of immigrants of nationalities restricted by the quota system experienced larger population declines in the subsequent decades as the quotas reduced the supply of immigrants to these areas. We then show that the quotas led to negative agglomeration effects in the manufacturing sector, while productivity losses are only visible in urban counties, cities, and immigrant dependent industries. We also find that the quota system pushed native workers into low-wage occupations.

Key Words: Immigration restrictions, National quota acts, Economic growth

JEL: J11; J61; N12; O11; O47.

*Acknowledgements: We would like to thank: Carl-Johan Dalgaard, Stephan Heblich, Peter Sandholt Jensen, Petra Moser, Nathan Nunn, Asger Moll Wingender, Daniel Sturm and seminar participants at the University of Copenhagen. This research is funded by the Danish Research Council reference No. DFF – 4182-00043.

1 Introduction

In light of the recent European migrant crises, there is a lively debate on how rich countries should respond to higher immigration.[1] Following these debates, many policy makers and researchers are interested in evaluating how immigration influences the economic performance of receiving countries. Economic theory provides no ultimate answer to this question, as the implications depend, for example, on the assumptions made about returns to scale, the degree of substitutability in production between native and foreign-born workers, and immigrants' skills. Many scholars have also investigated empirically the effect of immigration on a range of economic outcomes, such as native wages, employment, and productivity without reaching any consensus.[2]

This paper contributes to the understanding of how immigration influences the economic performance of receiving countries by studying the consequences of implementing immigration restrictions based on a quota system for local economies. We exploit a regime change in US immigration policy—the passage of the Emergency Quota Act in 1921—which abruptly ended an unprecedented era of unrestricted immigration to the United States (Goldin, 1994).[3] The introduction of nationality based quotas curtailed annual immigration in the United States by more than 80 percent. This unique episode in history can potentially be used to evaluate the causal effect of immigration on the US economy (Abramitzky and Boustan, 2016).

The quota system provides two sources of variation which our empirical strategy exploits. First, the timing of the quota system permits a before-and-after quota comparison of the outcomes of interest. Second, the quota system was constructed such that immigration from some nations was restricted more than from others, while immigration from the Western Hemisphere remained completely unaffected.[4] To construct treatment-control differences, we combine this variation with an established observation in the migration literature that newly arriving immigrants tend to settle in areas where previous immigrants of the same nationality live (Bartel, 1989; Card, 2001; Munshi, 2003). We measure these nationality networks by the initial (pre-quota) spatial distribution of foreign-born from different countries of origin across the United

[1] According to the United Nations more than 240 million people live permanently outside their birth countries in 2015, and this number is expected to increase steadily the coming decades (UN, 2015).

[2] See, for example, Card (2001, 2007, 2009), Borjas (2003, 2006), Peri and Sparber (2009), or Peri (2012).

[3] More than 30 million European immigrants arrived in the United States during the Age of Mass Migration (1850-1920), resulting in an average annual immigration inflow rate of about 0.75% of the total US population at that time (Hatton and Williamson,1998).

[4] We refer to section 2 for a detailed description of the quota system.

States. Thus, we estimate the effects of a quota supply-driven decrease in immigration which is likely to be unequal across areas due to variation in the spatial distribution of foreign-born from different countries of origin.[5] We implement our strategy using three different samples: counties, cities, and industries at the national level. While the counties in our sample are measured only every decade from 1900 to 1940, our newly constructed quinquennial city level manufacturing dataset from 1909 to 1929 and the biennial industry data from 1919 to 1931 provide us with sharper time variation related to the introduction of the quotas.

Our first finding is that the quota system led to a relative decline in population, which to a large extent is accounted for by the direct effect of the quota system on restricting the inflow of immigrants in some areas more than in others. There are three main results: First, counties and cities that were relatively more affected by the quotas experienced negative agglomeration effects in the manufacturing sector, as measured by the number of workers, establishments, total wages, and total value added. For example, a one-mean-deviation increase in treatment intensity (at the county level) reduces total wage income and value added by about 3-6 percent in the following decades. These effects are significantly larger in urban counties and cities. We also find a negative effect on the number of workers, wages, and value added for industries that depended more heavily on the inflow of immigrants affected by the quota. Second, we only find negative productivity effects in urban counties, cities, and for immigration dependent industries. Our baseline estimates suggest a decrease in manufacturing value added per worker of 13 percent in 1940 for urban counties, and 9 percent in 1929 at the city level, for example.[6] The second finding could be explained by agglomeration externalities being stronger in urban areas, which were generally more affected by the quota system since most of the immigrants lived there. Third, our analysis shows that the quotas pushed native workers into low-wage occupations, which resonates with the findings of Foged and Peri (2016).

In terms of identification, our baseline specification is quite demanding, and exploits only within-county/city variation of the data, and we are able to provide indirect support of the main identifying assumption by showing the existence of parallel trends prior to the implementation of the quota system.[7] The findings are also robust to a number of sensitivity checks; most

[5] This approach assumes that native workers are not fully mobile.

[6] Considering all counties (urban and rural), we find some evidence that the quota system increased average productivity, however, these findings are not robust.

[7] Our empirical analysis includes county/city fixed effects, county/city specific linear time trends, and state-by-time fixed effects, while the industry analysis is at the national level and we can only control for time and industry fixed effects using this data.

importantly, we demonstrate that our findings are not contaminated by the Great Depression.[8]

Our paper contributes to a large literature on the economic consequences of immigration in the United States (Borjas, 1994, 1999, 2014; Card, 1990, 2009, 2012; Cortes, 2008; Saiz, 2003, 2007). While most of the modern literature analyzes the short-run and medium-run effects, a growing literature investigates the long-term consequences of immigration for the US economy. For example, the cultural economics literature has demonstrated that immigrants and their offspring still identify with cultural and social norms of their country of ancestry, even if they have lived in the US for a long time.[9] Banderia et al. (2016) show that US states adopted compulsory schooling laws earlier, if they hosted a higher share of European immigrants which came from countries with lower exposure to civic values (e.g., without historic exposure to compulsory schooling). Grosjean (2014) argues that the prevalence of higher homicide rates in the American South today can be traced to the historical presence of Scot-Irish settlers. These settlers transmitted a culture of violence over several generations which explains today's higher homicide rates of white offenders within the American South, however, only in areas with weak formal institutions. Fernández and Fogli (2009) find that female labor force participation and fertility rates in the country of ancestry of second-generation American women are important determinants of how much these women work and how many children they have. While the results of Abramitzky et al. (2016) indicate a cultural assimilation process of longer staying immigrants in the US, as the immigrant parents choose less foreign names for their children, they also find that, within households, brothers with more foreign names perform significantly worse in terms of educational attainment, earnings, and finding a job.

Many American economic historians have evaluated the immigrants' impact on the US economy during the Age of Mass Migration (1850-1920).[10] In contrast to much of the literature, relatively few empirical studies have investigated the impact of historical immigration flows during the Age of Mass Migration on local economies. Ager and Brueckner (2013) examine how changes in the cultural composition of US counties due to the large inflow of European

[8] We also note that this is only a concern in the county analysis, where the post-quota outcomes are measured for the first time just after the beginning of the Great Depression in 1930, while the city and industry level analysis provides post-quota estimates before that.

[9] Bisin and Verdier (2000, 2001) provide a theoretical foundation for the intergenerational transmission of cultural traits.

[10] See, for example, Abramitzky et al. (2012, 2013, 2014); Banderia et al. (2013); Collins (1997); Dunlevy and Hutchinson (1999); Hatton and Williamson (1998); Ferrie (1994; 1999, 2005); Greenwood (2008); Kim (2007); Lafortune et al. (2015); Timmer and Williamson (1998). For further details and references we refer to the recent survey on immigration in American history of Abramitzky and Boustan (2016), Carter and Sutch (1999), and Hatton and Williamson (1998).

migrants during the Age of Mass Migration affected output growth between the years 1870 to 1920. They find that increases in cultural fractionalization due to immigration increased output growth, while increases in cultural polarization led to a decline in output growth,[11] while Nunn et al. (2016) study the long-run consequences of European migration during the Age of Mass Migration for counties' economic development today. They show that counties that experienced historically a larger inflow of European migrants perform better today in terms of income levels, poverty, unemployment, and educational attainment.[12]

Nunn et al. also document that, in the short and medium run, European immigrants led to productivity gains in the agricultural and manufacturing sector, increased urbanization, and higher rates of patenting. These results resonate with Moser et al. (2014), who document a surge of patent activity of US inventors associated with the arrival of German Jewish immigrants after 1933,[13], and Peri (2012), who analyzes the long-run effects of immigrants on employment and productivity for the period 1960 to 2006. Peri finds no evidence that immigrants crowed out employment, but he documents significant productivity gains from the net inflow of immigrants for the receiving states. Our paper complements the findings of this literature by providing evidence that the immigration restrictions established during the 1920s led to a significant decline of economic activity in the manufacturing sector, productivity losses in immigrant dependent industries and in larger cities where most of the immigrants lived, and income losses of native workers that were pushed into low-wage occupations.

Our analysis contributes to a better understanding of the economic impact of immigration restrictions. Goldin (1994) explains the political process behind the introduction of barriers to immigration in the United States between 1890 and 1921 (see section 2 for a detailed discussion). While migration from Europe was virtually unrestricted during the Age of Mass Migration, Asian migration was already constrained by the Chinese Exclusion Act in 1882. Chen (2015), for example, finds that the average occupational standing of Chinese immigrants declined after the Chinese Exclusion Act. A few empirical studies have investigated how the

[11] Further studies that investigate how the ancestry composition of US counties matters for economic development are, for example, Ager and Brueckner (2016); Burchardi et al. (2016); and Fulford et al. (2016).

[12] Relatedly, using Census data for the years 1880 to 1910, Rodriguez-Pose and von Berlepsh (2014) find that historical settlement patterns of migrants still matter for local economic development in the United States today. Ager and Brueckner (2016) document that, during the Age of Mass Migration, immigrants' genetic diversity is significantly positively correlated with measures of US counties' economic development and that this positive correlation persists until the twenty-first century.

[13] Further evidence on immigrants positive impact on science and innovation in the US is from Hunt and Gauthier-Loiselle (2010), who document a positive association between high-skilled immigrants and patent activity for a panel of U.S. states from 1940–2000.

US quota laws during the 1920s changed migration behavior.[14] Greenwood and Ward (2015) show that emigration rates declined significantly after the introduction of immigration quotas, especially from unskilled occupations and farmers. 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. While our paper complements these studies, our main focus is a different one, since we are interested in the macroeconomic implications of the immigration quota laws for local economies.[15]

2 Historical Background

Anti-immigration movements in the US go back a long time (Higham, 1955; Hutchinson, 1981).[16] The first significant US immigration law that restricted immigration at a larger scale was the passage of the Chinese Exclusion Act in 1882. While this law restricted Chinese immigration, European immigration remained virtually unrestricted until the passage of the Immigration Act in 1917, which imposed a literacy test on immigrants, requiring to read lines from the US Constitution in a language by their own choice.[17] The official statistics reveal that the total number of annual immigrants admitted to the US dropped from circa 300,000 in the years just preceding the 1917 act to 100,000 immediately afterwards. However, annual immigration increased again to 400,000 already in 1920 and exceeded 800,000 in 1921 (USCB, 1929). Effectively, when the literacy test passed in 1917, it was no longer any restrictive measure in terms of reducing European immigration, because literacy rates in Europe were rising rapidly over this time period. The 1920's quota system can therefore be viewed as a natural

[14] Lundborg and Segerstrom (2002) study the growth and welfare implications of immigration quotas in an endogenous growth model framework.

[15] 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 (2016), for example.

[16] The rise of the Know-Nothing party during the 1850s as a response to increased Catholic immigration from Germany and Ireland is such an example.

[17] Various interests groups were behind the passage of the Immigration Act in 1917. Goldin (1994) argues that different 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 until 1917 and became deceive for the vote in favor of the 1917 act was the position on free immigration among the older immigrations groups, living predominantly in the Midwestern areas, and rural-native born living in the US South, who initially had a more liberal view on immigration. See Goldin (1994) and Fairchild (1917) for more details on the Immigration Act of 1917.

continuation of the literacy test as a way of reducing immigration (Goldin, 1994).[18]

The first immigration policy effectively restricting the number of European immigrants allowed to enter the US was the Emergency Quota Act in 1921. This law restricted the annual number of immigrants from any nationality to three percent of each foreign-born group living in the US in 1910 (the National Origins Formula). The total number of immigrants fell from 805,228 in 1921 to 356,995 in 1922 (USCB, 1924). Immigrants coming from Canada, Central America, and South America were exempted from the quota system. In addition, the quota system asymmetrically affected immigration from different European regions due to the National Origins Formula. For example, the number of Italian immigrants was reduced from 222,487 in 1921 to the 1922-quota number of 42,057 immigrants, while from Scandinavia the flow of immigrants in 1921 was 25,812 and the 1922-quota number was 37,938 (USCB, 1921, 1924).

The Emergency Quota Act was replaced by the Immigration Act of 1924, which made the quota system permanent. The 1924 act involved two significant changes: First, the ceiling was reduced from three percent to two percent of the foreign-born stock, which meant that the annual quota number was reduced from 357,083 in 1924 to 164,667 in 1925 (USCB, 1929). Second, the modified National Origins Formula draws on the foreign-born stock of 1890 instead of 1910. This change almost prevented immigration from Southern and Eastern Europe. For example, the annual quota for Russia dropped from 24,405 to 2,248 immigrants only (USCB, 1929). Apart from some minor changes, the quota system, as provided by the Immigration Act of 1924, remained in place until 1965, where it was replaced with the Immigration and Nationality Act.[19]

One concern for our empirical analysis would be if the passage of the Immigration Act in 1917 is driving our results. Showing empirically that immigration is negatively correlated with wages, and that this relationship was increasing in magnitude throughout the period 1890–1920, Goldin stresses that there might be some direct economic motivations behind the restriction finally being implemented in 1917, so as to maintain the relative economic position of native (unskilled) workers, as also argued by Timmer and Williamson (1996). In addition, Goldin shows a negative correlation between the proportion of the city population that was foreign-born and anti-immigration positions, as measured by voting in the House. Our empirical strategy is designed such that we can evaluate if these findings constitute threats to identification. In

[18] In the United States Congress there was no serious consideration to implement quotas or any other blanket restrictions to restrict European immigration before 1920 (Goldin, 1994).

[19] After June 30, 1927, total annual immigration from all countries was limited to a total of 150,000.

section 5, we show that areas possibly more affected by the quota system had similar growth rates in observables prior to the 1920s, suggesting that the empirical observations before the passage of the quota acts do not contaminate our baselines estimates.

3 Economic framework

This section discusses how different economic theories would predict the quota system to influence economic agglomeration and productivity. The standard economic model, or what Peri (2016) calls the canonical model in the migration literature, assumes that immigrants are perfect substitutes to native workers, which makes it possible to consider the immigration restrictions as only a negative shock to population or labor force size (physical capital, which is assumed to be fixed in the short run, is the other factor of production). While it seems plausible to argue that the quotas led to a decline in the national population size, it is a priori unclear whether potentially more affected areas would experience a relative decline in population, which is what we attempt to exploit in our empirical analysis. For example, in the case of perfect labor mobility within the US such relative decline would not take place. This is why our empirical analysis at first demonstrates that more potentially affected areas in fact experienced a decline in population relative to less affected areas. Thus, we now simply take this assumption about imperfect mobility of labor for granted. According to a standard neoclassical constant return to scale (CRS) production function with physical capital and labor as inputs, there is an unambiguously increase in productivity as the population (labor) declines. However, the positive short-run shock would be attenuated in the long-run as the supply of physical capital adjusts. Nevertheless, if the quotas had a lasting negative effect on the population growth rate, there would even be a positive productivity effect in the long run from less capital dilution. In addition, both in the short as well as in the long run, the quotas would be associated with less economic agglomeration since total production decreases.

While the canonical model predicts non-negative effects on productivity from introducing the quotas, more recent alternative frameworks have rather different predictions.[20] For example, if there are positive scale effects in production, it is indeed possible that the quotas would be associated with declining average productivity in the short as well as in the long run. Such positive scale effects could be motivated by learning-by-doing in production or other agglom-

[20]See Peri (2016) for a summary of these frameworks.

eration externalities, which are more likely to be functioning in urban areas. Consequently, whether an increase in population size has a positive or negative effect on productivity depends on the returns to scale assumption.[21] Moreover, if immigrants are not identical to the native workers, they could increase average productivity by bringing with them intangible productive knowledge not embedded in the local economy. If we allow for complementarity in production between immigrants and native workers then, on the top of what is already mentioned, one would find (even in a CRS production function framework) that a decrease in the number of immigrants decreases native wages in the short-run (if the degree of complementarity is strong enough to offset the positive effect from the increasing capital intensity) and, under relative mild conditions, an unambiguously decrease in the long-run.[22]

4 Data and Estimation Strategy

4.1 Data

Our empirical analysis provides evidence on the relationship between immigration restrictions and economic growth using county, city, and national industry-level data. The US 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 (Haines, 2010) for the sample period 1900 to 1940.[23] For the empirical analysis, we use total population, urban population (i.e., population in areas with more than 25,000 inhabitants), and the number of establishments, workers, wages, and value added in the manufacturing sector as outcome variables. Since there are no manufacturing data available for 1910 at the county level, these values had to be imputed.[24] Moreover, the Census reported population by country of origin which is the essential ingredient to construct our quota treatment measure (see the following subsection for more details).

To check whether our treatment measure actually captures decreases in immigration, we collected microdata on immigration flows from IPUMS (Ruggles et al., 2010). For the 1900–1930

[21] If there are decreasing returns to scale due to, e.g., a fixed factor of production such as land, the quotas would even in the long-run lead to increasing productivity.

[22] We refer to the theory section of the Appendix for further details.

[23] 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.

[24] We used a linear interpolation to obtain the 1910 values for the manufacturing sector.

period, the Census reported for every foreign-born the year of arrival to the US. We combine this information with the individual's current place of residence to construct an area based measure of immigration flow which is collapsed for every decade between 1900 and 1930.[25] For native workers we also obtained income-based occupational scores from IPUMS. This information is collapsed to represent the average occupation score of a native worker at the county level. If the quota system affected the occupational distribution, one should not view this as evidence on an effect on marginal productivity, but rather that the quota system pushed native worker into lower/higher paid occupations. Panel A of Table 1 reports summary statistics for these variable by sample type (whole and urban sample).

For the city-level analysis we digitized city-level data from the Census of Manufactures for the years 1909, 1914, 1919, 1925, and 1929. The manufacturing outcome variables are the same as for the county-level analysis. To ensure that the city-level results are comparable with the urban-county findings, we restrict the sample to include only cities with more than 25,000 inhabitants in 1900.[26] This leaves us with a balanced panel of 143 cities. We calculate nationality shares at the city level using the micro data from the IPUMS 5-percent random sample in 1900.[27] The quota treatment measure is constructed analog to the county-level analysis. As can be seen from Table 1, Panel B, the average foreign-born share affected by the quota system (i.e., the treatment intensity) is circa 8 percent, while it is 5 percent in the total sample and 9 percent in the urban sample (Panel A). Finally, we use biennial industry data from the Census of Manufactures for the years 1919 to 1931. These data are retrieved from Joshua Rosenbloom's data website and described in Rosenbloom and Sundstrom (1999).[28] The manufacturing outcome variables are the same as for the city and county-level analysis. Panel C of Table 1 reports the summary statistics for the industry data.

[Table 1 about here]

[25] The decade 1900, ..., 1930 represents the flow of immigrants from 1891 to 1900, ..., 1921 to 1930. It not possible to construct this variable for the decade 1940 as the Census did not ask this question in 1940.

[26] This choice is also motivated by the fact that the quotas are supposed to have larger effects in urban areas, where most of the immigrants lived at that time.

[27] Similar results are obtained when we use the IPUMS 1-percent random sample in 1910, instead.

[28] See, http://joshua-rosenbloom.squarespace.com/datasets/ for further details.

4.2 Estimation strategy

The treatment-intensity variable captures the degree to which a given area was (potentially) affected by the quota system and is constructed as:

$$treatment\ intensity_c = \sum_{n=1}^{N} FB_{nc} \times Quota_n, \tag{1}$$

where FB_{nc} is the share of foreign-born of nationality n in county c in 1910,[29] $Quota_n$ is a dummy indicating whether the quotas implemented during the 1920s were binding for nationality n. We assume this is the case if the following condition is satisfied:

$$\frac{\overline{IM}_{n,10-14}}{\overline{Q}_{n,22-30}} \geq 1, \tag{2}$$

where $\overline{IM}_{n,10-14}$ is the average annual number of immigrants from nationality n admitted to the US during the pre-WWI years 1910–1914, and $\overline{Q}_{n,22-30}$ is the average annual quota for nationality n during the years 1922–1930.[30] All nationalities from the Americas are assigned the value zero as they were exempted from the quota system. Data on the annual quota for the years 1922–1930, the annual number of admitted immigrants by nationality for the years 1922–1930, and the actual number of immigrants admitted over the period 1910–1914 are collected from the Statistical Abstract of the US (1924, Table, 79; 1931, Tables 99 and 104). Panel A of Figure 1 visualizes $treatment\ intensity_c$ for the total sample in the US; Panel B shows this variation for the urban samples.[31] Alternative ways of coding the quota restriction yield to similar results.[32]

We use the following two specifications to describe differences in pre-quota levels and trends for several county characteristics by their treatment intensity:

$$y_c^{1920} = \alpha_1 + \gamma_1\, Treatment\ intensity_c + \epsilon_c^1 \tag{3}$$

[29] We have information form the Census on the following foreign birthplaces: Albania, Armenia, Austria, Belgium, Bulgaria, Czech Republic, Denmark, Finland, France, German, Greece, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Romania, Russia, Spain, Sweden, Switzerland, Turkey, Great Britain, Yugoslavia, Asia, Africa, Australia, Canada, Mexico, Cuba, Central and South America, Hawaii, other persons born in Europe not further specified, and persons born in other not further specified countries.

[30] The official immigration year ended in June, 30, such that the immigration year 1922 refers to immigration inflows between July 1, 1921 and June 30, 1922, for example.

[31] Urban is defined as an area with more than 25,000 inhabitants in 1900.

[32] For example, following Greenwood and Ward (2015), one could construct the quota restriction in the following continuous way $Quota\ Restrict_n = \max\left(\frac{\overline{IM}_{n,10-14} - \overline{Q}_{n,22-30}}{\overline{IM}_{n,10-14}}, 0\right)$. Reassuringly, we obtain similar results using this alternative treatment measure (available from the authors upon request).

$$\Delta y_c^{1910-1920} = \alpha_2 + \gamma_2 \ \textit{Treatment intensity}_c + \epsilon_c^2 \tag{4}$$

where y_c^{1920} is the log outcome in 1920, $\Delta y_c^{1910-1920}$ is the log difference in the outcome between 1910 and 1920 (i.e., the growth rate or trend), *Treatment intensity*$_c$ is the continuous measure of treatment intensity, depicted in Figure 1, and ϵ_c^1 and ϵ_c^2 are error terms.

The estimates of γ_1 reflect pre-quota level differences between affected and less (or non-) affected counties, while the estimates of γ_2 reveal pre-quota trend differences. The results are reported in Table 2. Our measure of treatment turns out to be positively correlated with all (log) level outcomes in 1920. Counties that were *potentially* more affected by the quota system had on average larger populations and a higher economic activity, as measured by the number of manufacturing workers and establishments, and total manufacturing wage income and value added. Moreover, the data also reveal that the *average* wage incomes for all occupations of US-born individuals were higher in these areas. These baseline differences in level characteristics show up in the total sample and in the urban sample (columns 1 and 3).

Columns 2 and 4 check if there are any pre-quota growth differences systematically related to the treatment measure. In the total sample, counties with a higher treatment intensity experienced less growth in immigration and the average wage income of US native workers from 1910 to 1920, whereas these areas, at the same time, experienced more growth in population and the number of manufacturing workers (column 2). For the urban sample, reported in the final column of the table, there is no evidence of statistically significant pre-treatment growth differences in any outcome under consideration.

In general, the evidence from Table 2 suggests that "control" and "treatment"counties in the total sample are not following the same trend in the outcomes prior to the implementation of the quota system. This fact would bias the standard DiD estimator, which assumes parallel pre-treatment trends (i.e., that there are no differences in the size of the growth rates). Thus, we include county-specific linear time trends in all our specification, changing the identifying assumption to parallel growth (i.e., that there are no differences in the changes of the growth rate), which, in fact, seems to be mostly corroborated by evidence presented in the next section. Moreover, we control for initial outcomes interacted with time to further account for the heterogeneity observed in Table 2.

[Figure 1 and Table 2 about here]

As already indicated, we use the continuous measure of treatment intensity given in equation (1) to compare the development of various economic outcome before and after the introduction of the quotas between (potentially) more affected areas to less (or non-) affected areas. Our baseline estimation equation takes the form of a so-called "flexible" model which allows us to investigate the main identifying DiD assumption about common pre-treatment growth, along with the dynamic behavior of the outcome variable in the post-treatment period (i.e., after the implementation of the quota system):[33]

$$y_{ct} = \sum_{j=1900}^{1940} \beta_j \; \textit{treatment intensity}_c \times I_t^j + \sum_{j=1900}^{1940} \mathbf{X}_i' \times \mathbf{I}_t^j \Gamma_j + \lambda_c + \mu_t + \theta_c \times t + \phi_{st} + \varepsilon_{ct}, \quad (5)$$

where y_{ct} is the outcome (in logs) in county c at time t, *treatment intensity* ($\sum FB_{nc} \times Quota_n$) is interacted with a full set of time fixed effects, $\sum I_t^j$, where the omitted year of comparison is 1920. In order to take into account potential mean reversion due to initial level or trend differences, the vector $\mathbf{X}_i'$ includes the outcome variable in 1900 and 1910, and these are then interacted with a full set of time fixed effects. All the regressions control for county fixed effects, λ_c, time fixed effects, μ_t, and county-specific linear time trends, $\theta_c \cdot t$, implying that we are only able to estimate the coefficients for the years 1910, 1930, and 1940. In the most restrictive specification, we also control for state-by-time fixed effects, ϕ_{st}. We cluster the error term, ε_{ct}, at the state level to ensure that the standard errors are robust to arbitrary correlation across counties in each state.[34]

As mentioned, the standard identifying assumption, in the DiD framework, is parallel pre-treatment trends, however, as all the specifications include county-specific linear time trends, $\theta_c \cdot t$, we need only to impose the less restrictive assumption of *parallel growth*,[35] which is supported by the data if we find that $\hat{\beta}_{1910} \approx 0$.[36] The post-quota effects are given by $\hat{\beta}_{1930}$ and $\hat{\beta}_{1940}$ which will reflect the impact of the introduction of the quota-system on the log outcome in 1930 and 1940 relative to the base year 1920.

[33] This type of specification is in the "DiD literature" also known as event-study regression.

[34] We obtain similar results if the error term is clustered at the county level, instead (available form the authors upon request).

[35] The parallel growth assumption is less restrictive as this is a necessary condition for the parallel trend assumption to be fulfilled.

[36] In other words, the identifying assumption without the county-specific trends is that the pre-treatment trends have identical slopes, while including them allows the slopes to differ across counties, as long as the changes in the slopes are similar, and since our outcomes are all denoted in logs, this is the same as saying the identifying assumption is having parallel pre-treatment growth in the outcomes.

5 Results

5.1 County evidence

5.1.1 Immigration and population

In this subsection, we present the results from estimating the (intention-to-treat) effects of implementing the quota system on immigration flows and population size. This first part of the empirical analysis can be viewed as a consistency check. If our estimation strategy based on nationality networks of previous immigrants works, we would expect reduced immigration inflows into areas with higher treatment intensities after the quota system, while the effect on population size can be mitigated via internal-migration and/or changes in fertility behavior and mortality (e.g., Peri, 2016). Thus, although we only consider the reduced-form impact of the quota system one can think of these estimates along the lines of a first stage.

Table 3 contains the estimates for immigration, which due to lack of data is limited to the sample period 1900–1930. Columns 1–3 report the estimates for the total sample, columns 4–6 for the urban sample. The first specification controls for county and time fixed effects, as well as county-specific linear time trends. The second specification adds pre-quota cross-county variation in log immigration in 1900 and 1910 interacted with a full set of time fixed effects as controls to the estimating equation. Our preferred specification is the third, which also controls for state-by-time fixed effects. This specification only compares affected counties to less (or non-) affected counties with similar pre-quota outcome values within the same state. Since $\hat{\beta}_{1910} \approx 0$, there is support for the assumption of parallel growth between treatment and control counties prior to the implementation of the quota system. From 1920 to 1930, however, more affected counties experienced a relative decline in immigration. The point estimate indicates that going from a non-affected county to the average treated county reduces the total number of immigrants by 11 percent. This effect is with 15 percent somewhat larger in urban counties.

Table 4 presents our findings for population. The specifications are the same as in Table 3, however, since data on county-level population is available for 1940, the sample period extends to 1900–1940. This is also the case for the remaining outcome variables discussed in section 5.1. For the total sample, there is only a statistically significant negative relationship between treatment and population size in 1930 and 1940, while there are no significant differences in parallel growth before the quota restrictions were implemented in the 1920s. This estimate implies that increasing treatment from zero to the mean value decreases population by about 3.4 percent in 1940. For the urban sample, our preferred specification of column 6 reveals that counties with a higher treatment value experienced increased population growth before the quota system was implemented during the 1920s. While urban population growth increased from 1900-1910 to 1910-1920 in the more affected counties, these counties experienced a reversal of this growth pattern after introducing the quota system.[37] Both post-treatment estimates are negative and statistically significant at the one-percent level. For example, according to the estimate in 1940, a one-mean increase in treatment intensity decreased the urban population by 13 percent.

The appendix presents a simple accounting exercise which evaluates, using the fundamental demographic equation as the underlying basis, if the effect of the quota system on immigration is able to account for the effect on population. This accounting exercise requires estimates for immigration rates, which are reported in Appendix Table 1. These results are consistent with the estimates reported in Table 3. Comparing the estimates of Appendix Table 1 to Table 4, we find that the effect of the quotas on the immigration rates explains about 33 percent of the decline in population in the total sample and 71 percent in the urban sample. Taking uncertainty into account, this suggests that most of the quota effect on population can be explained by reduced immigration. While not ruling out that the quota system influenced in- and out-migration, births and death rates, on net we do not need large changes in these to explain our findings in Table 4 by immigration. Moreover, it suggests that we are not capturing some spurious trends related to other events such as the Great Depression. Having now established that the quota system indeed reduced population via immigration, the remaining part of the empirical analysis is devoted to investigating how this negative shock to population influenced local economic growth.

[Tables 3 and 4 about here]

5.1.2 Manufacturing

This section examines the effect of the quota system on the economic performance of the manufacturing sector. We study the following outcomes: the number of manufacturing workers,

[37] Since we find a negative coefficient for 1910 ($\hat{\beta}_{1910}^{urban} < 0$) and the omitted comparison year is 1920, this should be interpreted as urban population growth was increasing from 1900-1910 to 1910-1920 in more affected counties.

the number of manufacturing establishments, total wage income, and total value added. Since all aggregate outcomes are measured in logs, we can easily calculate the effect on measures of productivity, such as wage per worker and value added per worker, by subtracting the estimates from each other.

The estimates for manufacturing workers are reported in Table 5, following the same structure as the previous tables. In the total sample, the growth rate of the manufacturing workforce was increasing relatively more in counties with higher treatment values prior to the introduction of the quota system, since $\hat{\beta}^{total}_{1910} < 0$ is statistically significant in column 3, our preferred specification. This trend difference in the growth rate, however, breaks in the post-treatment years, where the estimated coefficients are negative and highly statistically significant. The estimates imply that a one-mean increase in treatment intensity reduced the manufacturing workforce in 1930 by five percent and by nine percent in 1940. For the urban sample, a similar pattern exists for the post-treatment years, but the effects are statistically insignificant. Table 6 presents results on the number of manufacturing establishments. Our preferred specifications (columns 3 and 6) show again breaks in the trend of the growth rate only after 1920 for the more treated counties, but these are not statistically significant at any conventional levels. Tables 7 and 8 show how total wage income and value added responded to the quota system. As we move across the columns of both tables (and samples), we see that pre-quota differences in the trend of the growth rate is disappearing, so that in our preferred specification (columns 3 and 6), the assumption of parallel growth is supported (except in column 6 of Table 7, where there is a small negative effect on wage in 1910). By 1930, however, there is a negative and statistical significant relationship between treatment and wage income (and value added). In the total sample, we find that a one-mean increase in treatment intensity decreases wage income by 4 percent and value added by 3 percent in 1930. This effect increases in 1940 to 5 and 6 percent, respectively. For the urban sample, these effects are about 7–12 percentage points larger.

Finally, we can evaluate how the quota system influenced average productivity in the manufacturing sector at the county level, as measured by wage expenditure per worker and value added per worker.[38] The overall conclusion is that while the effect of the quota system on productivity for urban areas is negative, there is, in general, a positive effect in the total sample. One possible explanation points to the importance of economics of scale in more densely pop-

[38]Since the outcomes are in logs it is sufficient to subtract the estimates in Table 5 from those of Tables 7 and 8.

ulated areas, whereas decreasing returns and capital dilution might be dominating elsewhere. However, we note that the positive productivity effects documented in the total sample are not very robust; see section 5.1.4 for more details.

[Tables 5-8 about here]

5.1.3 Native wage income

Table 9 presents estimates for average native wage income (based on the IPUMS occupational scores). The specifications follow the same structure as in the previous tables. The point estimates reveal that the quota system had a negative effect on average native wages. Despite there is some evidence of differential pre-treatment growth changes in the total sample (column 3), the estimates increase substantially in numerical magnitude and become statistically significant at the one percent level in 1930 for both samples. This negative effects persist to 1940. Interestingly, we find no significant effect on the average wage of foreign-born already living in the US (available from the authors upon request). Overall the evidence presented in Table 9 is consistent with the conclusion that the quota system caused native workers to take up lower paid occupations in order to fill out the gap from the decrease in immigration. This finding is also in line with Foged and Peri (2016), who reach a similar conclusion exploiting longitudinal data on workers in Denmark for the period 1991-2008.

[Tables 9 about here]

5.1.4 Robustness

This section establishes the robustness of our county-level evidence to the Great Depression.[39] Although the common shock of the Great Depression is taken out by the inclusion of time fixed effects, it seems likely that some local economies were more affected by the Great Depression than others. If this heterogeneity is correlated with our treatment measure, this constitutes a threat to identification. Fishback et al. (2005) argue that retail sales are an important indicator of the local impact of the Great Depression. Thus, we use the growth rate of retail sales per capita from 1929 to 1933 as a proxy for the local impact of the Great Depression. If

[39] The timing of the quota system overlaps with the timing of the Great Depression due to the use of decadal data.

our treatment measure is *not* able to predict this variation, this would be suggestive evidence that our baseline estimates is not confounded by the Great Depression. We check whether this is the case by estimating the following regression:

$$\ln \mathit{retail}\ pc_{33,c} - \ln \mathit{retail}\ pc_{29,c} = \alpha + \pi \sum_{n=1}^{N} FB_{nc} \times Quota_n + \phi_s + \epsilon_c, \quad (6)$$

where the outcome variable is the approximate growth rate of retail sales per capita from 1929 to 1933, and the remaining variables are described in section 4.2. In the total sample, we find no evidence of such a relationship, the point estimate, $\hat{\pi}^{total}$, is 0.11 with a standard error of 0.14.[40] The conclusion remains intact even if we add the initial outcomes as controls to this specification. For the urban sample, we find some evidence of a positive relationship, $\hat{\pi}^{urban} = 0.40$ with a standard error of 0.18, which is statistically significant at the five percent level. However, in most specifications, this relationship becomes statistical insignificant when including the initial controls. We regard this as further evidence that our strategy of controlling for initial outcomes accounts to some extent for the issue that more populous areas (with more aggregate economic activity and relatively more foreign born) were more severely affected by the crisis.[41]

Our second robustness check relates to local New Deal expenditures, measured by public works and relief grants. These data were collected and provided by Fishback et al. (2005). Since the New Deal programs were only rolled out in the post-treatment period (i.e., during the 1930s), the baseline 1930 estimates cannot be contaminated by this variation. However, it remains interesting to check if the conclusions for 1940 are affected. Table 10 reports the results for all the outcomes, controlling for expenditures on public works and relief grants interacted with a full set of time fixed effect.[42] The results are qualitatively similar to the baseline, although the pre-quota effect in the total sample becomes statistically significant for population and native wages. Since these pre-treatment effects are negative ($\hat{\beta}^{total}_{1910} < 0$), it means that counties with higher treatment values experienced a reversal in the trend of the

[40] As suggested in equation (6), this estimate is conditional on state fixed effects, however, the unconditional estimate is very similar.

[41] Along the lines of Tables 10 and 11 below, we have also checked that the urban baseline estimates are robust to controlling for the growth rate in retail sales per capita (1929-1933), interacted with a full set of time fixed effects. These results are available from the authors upon request.

[42] We only show the estimates from our preferred specification, which controls for county and time fixed effects, county-specific linear time trends, state-by-time fixed effects, and the initial outcome interacted with time, to save space.

growth rate in these observables. In this perspective, the post-treatment effects are, if anything, under-estimating the effect of treatment.

The third robustness check looks at whether initial variations in local financial and fiscal development, as proxied with the prevalence of banks and tax income, before the implementation of the quota system played a role in explaining our baseline findings. Table 11 presents the results from estimating the preferred specification for all outcomes, including the number of banks and log tax income in 1920 interacted with time fixed effects as additional controls. Overall, these additional controls do not substantially change the main findings, although the estimates suggest that the effect on productivity for the total sample was indeed negative.

[Tables 10 and 11 about here]

5.2 City evidence

This section reports estimates for the manufacturing sector from 1909 to 1929 using city-level data. One important advantage of the city-level data is that those are available for the years 1909, 1914, 1919, 1925, and 1929, and we can more tightly evaluate the main identifying assumption of parallel pre-treatment growth and mitigate the concern of omitted time-varying shocks during the post-treatment period. For example, this means that the Great Depression cannot be a possible confounding factor for the city-level analysis since we now have two post-treatment observations (1925 and 1929) before the Great Depression compared to the county-level analysis. Since most of the immigrants at that time lived in cities, a second advantage of these data is that we can evaluate the impact of the quota system on the manufacturing sector in cities where one would expect it mattered the most.

The city estimation equation is given by:

$$y_{it} = \sum_{j=1909}^{1929} \gamma_j \left(\sum_{n=1}^{N} FB_{ni} \times Quota_n \right) \times I_t^j + \sum_{j=1909}^{1929} \theta_j y_{i1909} \times I_t^j + \lambda_i + \mu_t + \theta_i \times t + \phi_{st} + \varepsilon_{it} \quad (7)$$

where y_{it} is the outcome (in logs) in city i at time t. The term $\sum \theta_j y_{i1909} \times I_t^j$ denotes the initial outcome measured in 1909 interacted with a full set of time fixed effects. The remaining variables are defined in section 4.2. In this flexible specification, the omitted year of comparison is 1919. The error term is clustered at the city level.

Our analysis considers a balanced sample of all cities with more than 25,000 inhabitants in 1900. The manufacturing outcomes are the same as in the county analysis. The resulting estimates for the baseline specification, which includes all the fixed effects and initial outcome, are reported in Table 12. We see that the estimated pre-quota coefficient, $\hat{\gamma}_{1909}$, is negative for the number of worker, wage income, and value added, and positive for number of establishments, but in all cases statistically insignificant, supporting the identification assumption of parallel growth. After the implementation of the quota system, we see a trend break in 1925 for the first three mentioned outcomes (columns 1, 3 and 4). However, the estimated coefficient, $\hat{\gamma}_{1925}$, is only statistically significant for value added, but statistically insignificant for the number workers and wage income. In 1929, the estimates for wage income and value added are negative and statistically significant at the 5-percent level. Their magnitude suggests that a one-mean increase in the treatment intensity led to reductions in total wage income of 11 percent and total value of 17 percent, which is pretty similar to what we found in the urban sample at the county level. Subtracting the estimates for the number of workers (column 1) from the estimates for wage income or value added (columns 3 or 4) reveals that the quota system led to productivity losses in the manufacturing sector in cities (as was the case in the urban-county sample).

[Tables 12 about here]

5.3 Industry evidence

In this section, we ask how industries in the manufacturing sector that were relatively more dependent on foreign-born workers from nationalities affected by the quota system developed afterwards. We use biennial data for industries from the Census of Manufacturers for the years 1919 to 1931. In particular, we estimate the following industry-by-year specification:

$$y_{vt} = \gamma \left(\sum_{n=1}^{N} FB_{nv} \times Quota_n \right) \times I_t^{post} + \theta \; Emp \; share_{vt} + \lambda_v + \mu_t + \varepsilon_{vt}, \tag{8}$$

where is y_{vt} the outcome (in logs) in industry v at time t, FB_{nv} is the foreign-born share of nationality n in industry v, which, as in the city analysis, is measured in 1900, $Emp\ share_{vt}$ is the employment share out of all industries,[43] $Quota_n$ is defined in section 4.2, λ_v and μ_t are industry and time fixed effects, and ε_{vt} is the error term. Due to data availability, we only have one pre-quota year (1919), implying that we cannot include industry-specific linear time trends and tests for possible pre-quota trend differences. This is also the reason why we now interact the treatment measure with an indicator, I_t^{post}, which equals one after 1919, instead of interacting the treatment measure with a full set of time fixed effects.

The results from estimating equation (8) are presented in Table 13. The evidence at the national-level suggests that industries which were more dependent on foreign-born workers from nationalities affected by the quota system experienced smaller increases in the number of workers, wages income, and value added, while there are no differences in the number establishments. Comparing these estimates, we see that the quota system had negative productivity effects for industries that depended more on foreign-born workers from nationalities affected by the quota system. For example, according to columns 1 and 3, a one-mean increase in treatment intensity is associated with a decrease in value added per worker of 11 percent.

[Table 13 about here]

6 Conclusion

The introduction of the quota system during the 1920s marked a fundamental change in US immigration policy. The doors for European immigrants that were widely open during the 19th and the beginning of the 20th centuries closed in 1921 (Goldin, 1994). Economic historians have investigated the political economy of immigration restrictions in the US (Goldin, 1994; Timmer and Williamson, 1996) or studied how the quota system affected migrant selection and return migration (Greenwood and Ward, 2015; Massey 2016), but a rigorous quantitative assessment of the macroeconomic consequences of these immigration restrictions on local economies has been lacking. This paper aims to fill this gap. The so-called National Origins Formula introduced with the passage of the Emergency Quota Act in 1921 implied that only some nationalities, mainly Europeans, were affected by the quotas while for other nationalities immigration to the US still remained open without any restrictions. We used this variation along with the

[43] Other papers using a similar type of specification, but for other purposes, also control for the employment share (e.g., Rajan and Zingales, 1998; Cetorelli and Strahan 2006; Mitchener and Wheelock, 2013)

spatial distribution of different, pre-existing, nationality networks across the US to construct treatment-control differences which allowed us to evaluate the economic performance of local economies and industries before and after the implementation of the quota system.

We found that the quota system had a negative effect on population size. Counties which were potentially more influenced by the quotas experienced a relative population decline afterwards. We documented that this decline is mainly due to the asymmetric effect of reduced immigration inflows across counties. Our analysis of the manufacturing sector revealed that total wages and valued added shrank after the introduction of the quota system in the more affected counties and cities and in industries that depended more on foreign-born workers from nationalities affected by the immigration restrictions. Labor productivity declined only in urban counties, large cities and immigration dependent industries, where we expect the quota system to matter the most, because immigrants tended to settle in urban areas and thereby increased the density of economic activity (e.g., Ciccone and Hall, 1996). The immigration restrictions during the 1920s had also an adverse effect on US natives, since we show that the quota system pushed native workers into low-wage occupations, both in rural and urban areas.

One of our key findings suggest that restricting immigration might decrease labor productivity more in urban than rural areas. However, as stressed by Abramitzky and Boustan (2016), immigrants back then were closer substitutes to natives, for example, in terms of human capital skills, indicating that restricting immigration today might have a less negative effect on productivity in case immigrants' skills are significantly lower compared to natives. If this is the case, this would make our finding that the quota system pushed native workers into low-wage occupations even stronger in a contemporary context.

References

[1] Abramitzky, R. and L. P. Boustan (2016). Immigration in American History. Journal of Economic Literature, (forthcoming).

[2] Abramitzky, R., Boustan, L. P. and K. Eriksson (2012). Europes Tired, Poor, Huddled Masses: Self-Selection and Economic Outcomes in the Age of Mass Migration. American Economic Review, 102, 1832-1856.

[3] Abramitzky, R., Boustan, L. P. and K. Eriksson (2013). Have the poor always been less likely to migrate? Evidence from inheritance practices during the Age of Mass Migration. Journal of Development Economics, 102, 2–14.

[4] Abramitzky, R., Boustan, L. P. and K. Eriksson (2014). A Nation of Immigrants: Assimilation and Economic Outcomes in the Age of Mass Migration. Journal of Political Economy, 122, 467–717.

[5] Abramitzky, R., Boustan, L. P. and K. Eriksson (2016). Cultural Assimilation During the Age of Mass Migration. Mimeo.

[6] Ager, P. and M. Brueckner (2013). Cultural Diversity and Economic Growth: Evidence from the US During the Age of Mass Migration. European Economic Review, 64, 76–97.

[7] Ager, P. and M. Brueckner (2016). Immigrants' Genes: Genetic Diversity and Economic Development in the US. Mimeo.

[8] Bandiera, O., Rasul, I. and M. Viarengo (2013). The Making of Modern America: Migratory Flows in the Age of Mass Migration, 102, 23–47.

[9] Bandiera, O., Rasul, I., Mohnen M. and M. Viarengo (2016). Nation-Building Through Compulsory Schooling During the Age of Mass Migration, Mimeo.

[10] Bartel, A. (1989). Where do the new US immigrants live? Journal of Labor Economics 7, 371–391.

[11] Bisin A. and T. Verdier (2000). Beyond the Melting Pot: Cultural Transmission, Marriage, and the Evolution of Ethnic and Religious Traits. Quarterly Journal of Economics, 3, 955–988.

[12] Bisin A. and T. Verdier (2001). The Economics of Cultural Transmission and the Dynamics of Preferences, Journal of Economic Theory, 97, 298–319.

[13] Borjas, G. (1994). The Economics of Immigration, Journal of Economic Literature, 32, 1667–1717.

[14] Borjas, G. (1999). Heavens Doors, Princeton: Princeton University Press.

[15] Borjas, G. (2003). The Labor Demand Curve is Downward Sloping: Re-examining the Impact of Immigration on the Labor Market. Quarterly Journal of Economics, 118, 1335–1374.

[16] Borjas, G. (2006). Native Internal Migration and the Labor Market Impact of Immigration. Journal of Human Resources 41, 221–258.

[17] Borjas, G. (2014). Immigration Economics, Cambridge: Harvard University Press.

[18] Burchardi, K. B., Chaney, T. and T. A. Hassan, Migrants, Ancestors, and Investments, (2016). NBER Working Paper 21847.

[19] Card, D. (1990). The Impact of the Mariel Boatlift on the Miami Labor Market, Industrial and Labor Relations Review, 43, 245–257.

[20] Card, D. (2001). Immigrant Inflows, Native Outflows, and the Local labor Market Impacts of Higher Immigration. Journal of Labor Economics, 19, 22–64.

[21] Card, D. (2005). Is the New Immigration Really So Bad? Economic Journal, 115, 300–323.

[22] Card, D. (2007). How Immigrants Affects U.S. Cities. Centre for Research and Analysis of Migration Discussion Paper 11/07.

[23] Card, D. (2009). Immigration and Inequality. American Economic Review: Paper and Proceedings, 99, 1–21.

[24] Card, D. (2012). The Elusive Search for Negative Wage Impacts of Immigration. Journal of the European Economic Association, 10, 1–21.

[25] Carter, S. B. and R. Sutch (1999). Historical Perspectives on the Economic Consequences of Immigration into the United States, in DeWind, J., Hirschman, C. and P. Kasinitz, eds.,

Handbook of International Migration: The American Experience. New York: Russell Sage Foundation, 319–341.

[26] Cetorelli N. and P. E. Strahan (2006). Finance as a barrier to entry: Bank competition and industry structure in local u.s. markets. The Journal of Finance, 61, 437–461.

[27] Ciccone, A. and R. E. Hall (1996). Productivity and the Density of Economic Activity. American Economic Review, 86, 54–70.

[28] Chen J. J., (2015). The Impact of Skill-Based Immigration Restrictions:The Chinese Exclusion Act of 1882. Journal of Human Capital, 9, 298-328.

[29] Collins, W. J. (1997). When the Tide Turned: Immigration and the Delay of the Great Black Migration, Journal of Economic History, 57, 607-632.

[30] Cortes, P. (2008). The Effect of Low Skilled Immigration on U.S. Prices: Evidence from CPI Data, Journal of Political Economy, 116, 381–422.

[31] Dunlevy, J. A. and H. A. Gemery (1977). The Role of Migrant Stock and Lagged Migration in the Settlement Patterns of Nineteenth Century Immigrants. Review of Economics and Statistics 59, 137–44.

[32] Dunlevy, J. A. and W. K. Hutchinson (1999). The Impact of Immigration on American Import Trade in the Late Nineteenth and Early Twentieth Centuries. Journal of Economic History 59, 1043–62.

[33] Fairchild, H. P. (1917). The Literacy Test and Its Making. Quarterly Journal of Economics, 31, 447–460.

[34] Fernández, R. and A. Fogli (2009). Culture: An Empirical Investigation of Beliefs, Work, and Fertility, American Economic Journal: Macroeconomics, 1, 146-177.

[35] Ferrie, J. P. (1994). The Wealth Accumulation of Antebellum European Immigrants to the US, 1840–60. Journal of Economic History 54, 1–33.

[36] Ferrie, J. P. (1999). "Yankees Now": The Economic Mobility of Antebellum Immigrants to the US. New York: Oxford University Press.

[37] Ferrie, J. P. (2005). The End of American Exceptionalism? Mobility in the United States Since 1850. Journal of Economic Perspectives, 19, 199-215.

[38] Fishback, P. V., Horrace, W. C. and S. Kantor (2005). Did New Deal Grant Programs Stimulate Local Economies? A Study of Federal Grants and Retail Sales During the Great Depression, Journal of Economic History, 65, 36–71.

[39] Foged, M. and G. Peri (2016). Immigrants' Effect on Native Workers: New Analysis on Longitudinal Data. American Economic Journal: Applied Economics 8,1–34.

[40] Fulford, S. L., Petkov, I. and F. Schiantarelli (2016). Does It Matter Where You Came From? Ancestry Composition and Economic Performance of US Counties, 1850–2010, Mimeo.

[41] Goldin, C. (1994). The political economy of immigration restriction in the United States, 1890 to 1921. In: Goldin, C., Libecap, G.D. (Eds.), The regulated economy: A historical approach to political economy. NBER Chapters. National Bureau of Economic Research, 223–258.

[42] Greenwood, M. J. (2008). Family and Sex-Specific US Immigration from Europe, 1870–1910: A Panel Data Study of Rates and Composition. Explorations in Economic History 45, 356–382.

[43] Greenwood, M. J. and Z. Ward (2015). Immigration quotas, World War I, and emigrant flows from the United States in the early 20th century. Explorations in Economic History, Explorations in Economic History, 55, pp. 76–96.

[44] Grosjean, P. (2014). A History of Violence: The Culture of Honor and Homicide in the US South, Journal of the European Economic Association, 12, 1285–1316.

[45] Haines, M., 2010. Historical, Demographic, Economic, and Social Data: The United States, 1790-2002. ICPSR02896-v3. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2010-05-21. http://doi.org/10.3886/ICPSR02896.v3.

[46] Hatton, T. J. and J. G. Williamson (1998). The Age of Mass Migration: Causes and Economic Impact. Oxford University Press, New York, NY.

[47] Higham, John. (1955). Strangers in the Land: Patterns of American Nativism, 1860-1925. Westport, CT: Greenwood Press.

[48] Hunt, J. and M. Gauthier-Loiselle (2010). How Much Does Immigration Boost Innovation?, American Economic Journal: Macroeconomics, 2, 31–56.

[49] Hutchinson, E. P. (1981). Legislative History of American Immigration Policy, 1798-1965. Philadelphia: University of Pennsylvania Press.

[50] Kerr P. S. and Kerr W. (2011). Economic Impacts of Immigration: A Survey. Finnish Economic Papers, 24, 1–32.

[51] Kim, S. (2007). Immigration, Industrial Revolution and Urban Growth in the United States, 1820-1920: Factor Endowments, Technology and Geography. NBER Working Paper 12900.

[52] Lafortune, J., Covarrubias M. and J. Tessada (2015). Who comes and Why? Determinants of Immigrants Skill Level in early XXth century US, Journal of Demographic Economics, 81, 115–155.

[53] Lundborg, P., P. S. Segerstrom (2002). The growth and welfare effects of international mass migration. Journal of International Economics, 56, 177–204.

[54] Nunn, N., Qian, N. and S. Sequeira (2016). Migrants and the Making of America: The Short- and Long-Run Effects of Immigration during the Age of Mass Migration, Mimeo.

[55] Massey, C. G. (2016). Immigration quotas and immigrant selection., Explorations in Economic History, 60, 21–40.

[56] Mitchener K. J. and D. C. Wheelock (2013). Does the structure of banking markets affect economic growth? evidence from u.s. state banking markets. Explorations in Economic History, 50, 161-178.

[57] Moser, P., A. Voena, and F. Waldinger (2014), German Jewish Emigres and US Invention, American Economic Review, 104, 3222–3255.

[58] Munshi, K. (2003). Networks in the Modern Economy: Mexican Migrants in the U.S. Labor Market. Quarterly Journal of Economics, 18, 549–599.

[59] Peri, G. (2012). The Effects of Immigration on Productivity: Evidence from US States. Review of Economics and Statistics, 94, 348–358.

[60] Peri, G. (2016). Immigrants, Productivity, and Labor Markets, Journal of Economic Perspective, 30, 3–30.

[61] Peri, G., and C. Sparber (2009). Task Specialization, Immigration and Wages, American Economic Journal: Applied Economics 1, 135–169.

[62] Rajan, R. G. and L. Zingales (1998). Financial dependence and growth. American Economic Review, 88, 559-586.

[63] Rodriguez-Pose, A. and V. von Berlepsch, When Migrants Rule: The Legacy of Mass Migration on Economic Development in the United States, Annals of the Association of American Geographers, 104, 628–651.

[64] Rosenbloom J. L. and W. A. Sundstrom (1999). The Sources of Regional Variation in the Great Depression, Journal of Economic History, 59, 714–747.

[65] Ruggles, S., Alexander, T., Flood, S., Genadek, G., Schroeder, M. B., Trampe, B., Vick, R., King, M., 2010. Integrated public use microdata series, current population survey: Version 3.0.[machine-readable database].

[66] Saiz, A. (2003), Room in the Kitchen for the Melting Pot: Immigration and Rental Prices, Review of Economics and Statistics, 85, 502–521.

[67] Saiz, A. (2007), Immigration and housing rents in American cities, Journal of Urban Economics, 61, 345–371.

[68] Timmer, A. S. and Williamson, J. G. (1996). Racism, Xenophobia or Markets? The Political Economy of Immigration Policy Prior to the Thirties, NBER Working Paper No. 5867.

[69] Timmer, A. S., and J. G. Williamson (1998). Immigration Policy Prior to the 1930s: Labor Markets, Policy Interactions, and Globalization Backlash. Population and Development Review, 24, 739–71.

[70] UN (2015). International migrant stock 2015, United Nations, Population Division.

[71] USCB (various years). United States Census Bureau: Statistical Abstracts of the United States, U.S. Government Printing Office: Washington, D.C.

[72] USCB (various years). United States Census Bureau: Census of Manufactures, U.S. Government Printing Office: Washington, D.C.

Appendix

Theory

This section shows how a one-time increase in the number of immigrants would influence native wages in a simple theoretical model, where we assume that physical capital is fixed and the degree of complementarity between native workers and immigrant workers is allowed to vary. Consider a closed economy with the following CRS production function:

$$Y_t = K_0^{\alpha} \tilde{L}_t^{1-\alpha}, 0 < \alpha < 1 \text{ and } K_0 \text{ given,} \tag{9}$$

where Y_t is total production and K_0 is the aggregate capital stock, which is assumed fixed in the short run, $\tilde{L}_t$ is CES aggregate of the two types of labor in the economy given by:

$$\tilde{L} = \left(\eta L_{1t}^{\frac{\sigma-1}{\sigma}} + (1-\eta) L_2^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}},\ 0 < \eta < 1,\, \sigma > 0 \text{ and } \sigma \neq 1. \tag{10}$$

where L_{1t} denotes the immigrant labor force and L_2 is the native work force. By assuming fully competitive labor- and goods markets, the real wage for native workers is given by:

$$w_{2t} = (1-\alpha)(1-\eta) K_t^{\alpha} \tilde{L}_t^{1-\alpha-\frac{\sigma-1}{\sigma}} L_2^{\frac{\sigma-1}{\sigma}-1}. \tag{11}$$

Now we can evaluate how w_{2t} changes as the number of immigrants, L_1, increases:

$$\frac{\partial w_{2t}}{\partial L_1} = (1-\alpha)(1-\eta)\eta \left(1-\alpha-\frac{\sigma-1}{\sigma}\right) K_t^{\alpha} \tilde{L}^{-\alpha-\frac{\sigma-1}{\sigma}} L_2^{\frac{\sigma-1}{\sigma}-1}$$
$$\left(\eta L_1^{\frac{\sigma-1}{\sigma}} + (1-\eta) L_2^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}-1} \eta L_1^{\frac{\sigma-1}{\sigma}-1}, \tag{12}$$

we see that if $1-\alpha > \frac{\sigma-1}{\sigma}$ then $\partial w_{2t}/\partial L_{1t} > 0$. For the case of perfect substitution, $(\sigma \to \infty)$, there is a negative effect on native wages from increasing immigration $(\partial w_{2t}/\partial L_1 < 0)$, while in case of perfect complementarity $(\sigma \to 0)$, there is a positive effect on native wages $(\partial w_{2t}/\partial L_1 > 0)$. This simply tells us that if the degree of complementarity between the two types is strong enough, this would compensate the native worker from the capital dilution effect.

Accounting for the effect of immigration

This section outlines an accounting exercise for the effect of the quotas on the number of immigrants on population growth, using the so-called fundamental demographic equation. In particular, we ask if the magnitude of estimates for the flow of immigrants, reported in Table 3, can account for the estimated effect on population in Table 4.

The fundamental demographic equation is given by:

$$P_{ct+1} = P_{ct} + B_{ct} - D_{ct} + I_{ct} - X_{ct}, \tag{13}$$

where P_{ct+1} is the population size in year $t+1$ in county c, B_{ct} is the total number of birth, D_{ct} is the total number of death, I_{ct} is the total number of immigrants (and in-migrants), and X_{ct} is the total number of emigrants (and out-migrants). Equation (13) can be rearranged to give:

$$\hat{p}_{ct+1} = b_{ct} - d_{ct} + i_{ct} - x_{ct}, \tag{14}$$

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 (14) are functions of the quota system Q_t:

$$\hat{p}_{ct+1}(Q_{ct}, b_{ct}, d_{ct}, i_{ct}, x_{ct}) = b_{ct}(Q_{ct}) - d_{ct}(Q_{ct}) + i_{ct}(Q_{ct}) - x_{ct}(Q_{ct}). \tag{15}$$

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

$$\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}} = \frac{\partial b_{ct}}{\partial Q_{ct}} - \frac{\partial d_{ct}}{\partial Q_t} + \frac{\partial i_{ct}}{\partial Q_t} - \frac{\partial x_{ct}}{\partial Q_t}, \tag{16}$$

where the arguments have been suppressed for simplicity. Since the estimates in Table 4 can be interpreted as the effect of the quota system on population growth, we currently have estimates for $\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}}$, which, taken at face value, provides the total effect of all the changes in the demographic components due to the implementation of the quota system.

Before asking how much of the estimates of $\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}}$ can be explained by the effect on immigration flows, one should note that i_{ct} is the immigration rate, and Table 3 reports the estimates for the total immigration flow, controlling for, e.g., county-specific linear time trends. For this reason, Appendix Table 1 reports the estimates for the (log) immigration rate when *not* controlling for the county-specific trends, which then allows us to include one extra pre-treatment year (i.e., 1900), but otherwise all the baseline controls are included. The reported estimates for the years 1900 and 1910 demonstrate that there are no significant pre-treatment *trend* differences, though, in the total sample β_{1910}^{total} is borderline significant (at the 15 percent level) but negative, suggesting that, if anything, counties with a higher treatment intensity had increasing immigration rates from 1910 to 1920. After the implementation of the quota system in the 1920s, the estimates are negative and statistically significant at the conventional levels.

The idea is now to compare these estimates to the estimates reported in Table 4, which can be interpreted as estimates of $\frac{\partial \hat{p}_{ct+1}}{\partial Q_{ct}}$.[44] By simply comparing the point estimates and using the mean of the immigration rate (0.07 in the total sample and 0.23 in the urban sample), we see that the effect on the immigration rates explains about 33 percent of total population growth effect in the total sample and 71 percent in the urban sample. Taking uncertainty into account, this suggests that most of the effect on population growth can be explained by the effect of the quota system on immigration. Thus, we do not need large effects on in- and out-migration or births and deaths to explain the findings on population growth. Moreover, it suggests that we are not capturing some spurious trends related to, e.g., the Great Depression.

[44] Note, the immigration rate is measured in logs (Appendix Table 1), implying that $\frac{\partial i_{ct}}{\partial Q_t} = \hat{\beta} \times i_{ct}$, which we then evaluate at the sample mean before the quota system.

Table 1: Summary Statistics

	Panel A: County sample									
	Total					Urban				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	N	mean	sd	min	max	N	mean	sd	min	max
Outcomes (in logs)**:**										
Immigration flow	4,260	6.464	2.092	2.571	13.97	536	9.093	1.838	4.389	13.97
Population	6,890	10.38	0.870	7.431	15.22					
Urban population						670	11.68	1.048	10.13	15.12
Mfg. establishments	6,890	6.972	1.599	1.099	13.04	670	9.763	1.182	6.188	13.04
Mfg. workers	6,890	7.171	1.527	4.248	13.31	670	9.835	1.213	5.707	13.31
Mfg. total wage income	6,890	13.55	1.812	7.720	20.41	670	16.59	1.339	13.24	20.41
Mfg. total value added	6,890	14.45	1.774	9.161	21.47	670	17.50	1.353	14.25	21.47
Native avg. wage income	6,890	2.394	0.195	1.736	3.042	670	2.675	0.0963	2.389	2.893
Explanatory:										
FB×Quota	1,378	0.0541	0.0697	0	0.373	134	0.121	0.0874	0.00505	0.369
	Panel B: City sample					**Panel C:** Industry sample				
Outcomes (in logs)**:**	N	mean	sd	min	max	N	mean	sd	min	max
Mfg. establishments	780	5.645	1.067	3.784	10.98	143	7.818	1.505	2.639	10.86
Mfg. workers	780	9.364	1.124	5.476	13.93	143	12.35	0.727	9.859	13.81
Mfg. total wage income	780	16.16	1.243	13.11	21.32	143	19.43	0.755	16.95	21.01
Mfg. total value added	780	17.07	1.285	13.98	24.17	143	20.39	0.726	18.06	23.15
Explanatory:										
FB×Quota	143	0.0779	0.0621	0.00294	0.364	21	0.101	0.0375	0	0.153

Notes. This table reports summary statistics for the main variables used in the empirical analysis. Quota×FB is the treatment intensity measure, which only varies across counties, cities, or industries.

Table 2: Pre-quota Characteristics by Treatment Intensity

	Total sample			Urban sample		
		levels 1920	changes 1910-1920		levels 1920	changes 1910-1920
(in logs)	# obs	(1)	(2)	# obs	(3)	(4)
Immigration flow	995	17.20***	-2.214***	134	14.26***	-0.368
		(1.946)	(0.658)		(1.539)	(0.512)
Population	1,378	4.824***	0.553***	134	4.604***	0.0970
		(1.103)	(0.0974)		(1.086)	(0.192)
Mfg. workers	1,378	8.948***	0.702**	134	5.723***	-0.389
		(1.776)	(0.289)		(1.235)	(0.279)
Mfg. establishments	1,378	8.522***	0.224	134	6.147***	0.177
		(1.943)	(0.210)		(1.301)	(0.158)
Mfg. total wage income	1,378	10.33***	-0.0391	134	6.998***	0.0481
		(2.013)	(0.150)		(1.277)	(0.0959)
Mfg. total value added	1,378	10.27***	0.0659	134	6.740***	0.0948
		(1.971)	(0.153)		(1.368)	(0.122)
Native avg. wage income	1,378	1.549***	0.200***	134	0.555***	-0.0610
		(0.261)	(0.0724)		(0.101)	(0.0515)

Notes: The table reports OLS estimates of estimating equations (3) and (4). The total sample includes all counties for which there exists data all the years (1900-1940), whereas the urban sample only includes a subset of counties with more than 25,000 population in 1900. The outcome variables, indicated by name in the first row, are either in logs in 1920 (columns 1 and 3) or log differences between 1910 and 1920 (columns 2 and 4). In all the specifications, the explanatory variable is the treatment intensity variable, which is an interaction between a Quota dummy and the foreign-born share (see, e.g., the table note for Table 2). Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Immigration Flow

	Dependent variable: log number of immigrants					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times 1910$	1.286***	0.452	-0.519	0.737**	0.488	-0.335
	(0.356)	(0.437)	(0.430)	(0.346)	(0.454)	(0.345)
$\sum_n Quota{\cdot}FB \times 1930$	-1.353**	-1.337*	-2.214***	-1.579**	-1.478*	-3.005***
	(0.608)	(0.753)	(0.661)	(0.773)	(0.862)	(0.652)
Initial outcome ($\times \sum I^j$)	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	3,836	3,836	3,836	536	536	520
Number of counties	959	959	959	134	134	130

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, and 1930. All regressions include county and time fixed effects and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log inflow of immigrants, which is extracted using information on the year of immigration to the US and current place of residence (IPUMS). Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the log inflow of immigration flows in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Population

	Dependent variable (in logs):					
	Total population			Urban population		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota \cdot FB \times 1910$	0.187**	-0.0971	-0.152	0.173	-0.0323	-0.224*
	(0.0800)	(0.0607)	(0.0947)	(0.119)	(0.0887)	(0.109)
$\sum_n Quota \cdot FB \times 1930$	-0.437***	-0.210	-0.366***	-0.627***	-0.582***	-0.642***
	(0.155)	(0.169)	(0.136)	(0.114)	(0.115)	(0.153)
$\sum_n Quota \cdot FB \times 1940$	-1.190***	-0.357	-0.677***	-1.158***	-0.935***	-1.088***
	(0.248)	(0.267)	(0.226)	(0.222)	(0.138)	(0.196)
Initial outcome ($\times \sum I^j$)	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log population size (in columns 1-3) and log population in urban areas (in columns 4-6). Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: MFG Workers

	Dependent variable: log manufacturing workers					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times 1910$	-0.105	-0.484***	-0.616***	0.332	0.103	0.0165
	(0.267)	(0.164)	(0.222)	(0.354)	(0.182)	(0.251)
$\sum_n Quota{\cdot}FB \times 1930$	-1.842***	-1.453***	-0.942***	-0.485	-0.637	-0.632
	(0.280)	(0.317)	(0.345)	(0.310)	(0.385)	(0.505)
$\sum_n Quota{\cdot}FB \times 1940$	-3.233***	-2.612***	-1.715***	-0.635	-0.625	-0.734
	(0.520)	(0.554)	(0.613)	(0.524)	(0.527)	(0.675)
Initial outcome $(\times \sum I^j)$	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, 1930, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log total number manufacturing workers. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 6: MFG Establishments

	Dependent variable: log manufacturing establishments					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times 1910$	0.00860	0.0705**	0.0440	0.0430	-0.0110	-0.0589***
	(0.0812)	(0.0334)	(0.0595)	(0.0715)	(0.0450)	(0.0201)
$\sum_n Quota{\cdot}FB \times 1930$	-1.015**	-0.745**	-0.492	-1.026***	-0.949***	-0.657
	(0.395)	(0.343)	(0.368)	(0.305)	(0.329)	(0.408)
$\sum_n Quota{\cdot}FB \times 1940$	-1.882***	-1.180**	-0.634	-1.480**	-1.634**	-1.083
	(0.591)	(0.460)	(0.431)	(0.553)	(0.615)	(0.637)
Initial outcome $(\times \sum I^j)$	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, 1930, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log total number of manufacturing establishments. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: Total MFG Wage Income

	Dependent variable: log total manufacturing wages					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times 1910$	-0.118	0.0819***	0.0431	0.142	0.0274	-0.0273*
	(0.218)	(0.0206)	(0.0463)	(0.159)	(0.0471)	(0.0146)
$\sum_n Quota{\cdot}FB \times 1930$	0.102	-0.275	-0.685*	-0.952**	-1.004**	-0.954*
	(0.516)	(0.338)	(0.384)	(0.456)	(0.377)	(0.463)
$\sum_n Quota{\cdot}FB \times 1940$	-0.269	-0.804	-0.954**	-1.410*	-1.758***	-1.643**
	(0.826)	(0.517)	(0.453)	(0.699)	(0.610)	(0.713)
Initial outcome ($\times \sum I^j$)	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, 1930, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log total wage expenditures in manufacturing Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 8: Total MFG Value Added

	Dependent variable: log total manufacturing value added					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times 1910$	0.0461	0.0753***	0.0367	0.126	0.00747	-0.0526
	(0.189)	(0.0212)	(0.0477)	(0.158)	(0.0517)	(0.0387)
$\sum_n Quota{\cdot}FB \times 1930$	0.229	0.0272	-0.547*	-1.012**	-1.070***	-1.270***
	(0.488)	(0.295)	(0.304)	(0.386)	(0.313)	(0.406)
$\sum_n Quota{\cdot}FB \times 1940$	-0.431	-0.591	-1.167***	-1.872***	-1.826***	-1.836**
	(0.769)	(0.403)	(0.391)	(0.634)	(0.535)	(0.686)
Initial outcome ($\times \sum I^j$)	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, 1930, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log total value added in manufacturing Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, which are interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** p<0.01, ** p<0.05, * p<0.1.

Table 9: Average Native Worker Income

	Dependent variable: log average wage income (occupational score)					
	Total sample			Urban sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum_n Quota{\cdot}FB \times \mathit{1910}$	-0.148***	-0.115*	-0.0932**	-0.00710	-0.0437	-0.0383
	(0.0401)	(0.0633)	(0.0391)	(0.0332)	(0.0347)	(0.0361)
$\sum_n Quota{\cdot}FB \times \mathit{1930}$	-0.341***	-0.279**	-0.328***	-0.114**	-0.178***	-0.246***
	(0.106)	(0.112)	(0.0773)	(0.0529)	(0.0523)	(0.0429)
$\sum_n Quota{\cdot}FB \times \mathit{1940}$	-0.651***	-0.381*	-0.439***	-0.165*	-0.264***	-0.340***
	(0.216)	(0.206)	(0.154)	(0.0866)	(0.0909)	(0.0937)
Initial outcome ($\times \sum I^j$)	No	Yes	Yes	No	Yes	Yes
State-by-year FEs	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
County specific lin. trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,890	6,890	6,890	670	670	625
Number of counties	1,378	1,378	1,378	134	134	125

Notes: The table reports "flexible" OLS estimates which are relative to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, 1930, and 1940. All regressions include county and time fixed effects, and county-specific linear time trends. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. In column 6, 14 counties drop out as we here control for state-by-year fixed effects, which requires at the least two counties per state. The outcome variable is the log average wage income for all occupations of the US-born population (i.e., natives), which is derived from the occupational income score from IPUMS. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). The initial outcomes are the outcomes measured in 1900 and 1910, interacted with a full set of year fixed effects. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 10: Robustness to the New Deal

	Dependent variable (in logs):						
	immi-grants	pop-ulation	mfg. workers	mfg. establish-ments	mfg. wage income	mfg value added	avg. wage income native
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Total sample						
$\sum_n$Quota·FB×1910	-0.491	-0.162*	-0.618***	0.0414	0.0407	0.0347	-0.0930**
	(0.428)	(0.0953)	(0.233)	(0.0586)	(0.0465)	(0.0479)	(0.0422)
$\sum_n$Quota·FB×1930	-2.213***	-0.353**	-1.001***	-0.535	-0.726*	-0.549*	-0.330***
	(0.648)	(0.146)	(0.359)	(0.377)	(0.392)	(0.322)	(0.0816)
$\sum_n$Quota·FB×1940		-0.677***	-1.771***	-0.724*	-0.999**	-1.138***	-0.443***
		(0.223)	(0.635)	(0.435)	(0.468)	(0.420)	(0.163)
Number of obs.	3,630	6,710	6,710	6,710	6,710	6,710	6,710
Number of counties	936	1,342	1,342	1,342	1,342	1,342	1,342
	Panel B: Urban sample						
$\sum_n$Quota·FB×1910	-0.358	-0.204	0.0113	-0.0645**	-0.0300	-0.0475	-0.0245
	(0.394)	(0.128)	(0.277)	(0.0272)	(0.0193)	(0.0340)	(0.0328)
$\sum_n$Quota·FB×1930	-3.119***	-0.563***	-0.538	-0.574	-0.874*	-1.152***	-0.240***
	(0.624)	(0.182)	(0.501)	(0.420)	(0.483)	(0.376)	(0.0531)
$\sum_n$Quota·FB×1940		-1.029***	-0.715	-1.132*	-1.580**	-1.706***	-0.345***
		(0.275)	(0.738)	(0.666)	(0.730)	(0.630)	(0.112)
Number of obs.	500	625	625	625	625	625	625
Number of counties	125	125	125	125	125	125	125
Controls ($\times \sum I^j$):							
New Deal	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Initial outcome	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, and 1930 (column 1) and sample period is extended to include 1940 in the remaining columns. Baseline controls are: county and time fixed effects, state-by-year fixed effects, county-specific linear time trends, initial outcomes, measured in 1900 and 1910, interacted with a full set of year fixed effects. The New Deal controls are expenditures on "public works" and "relief grants" (Fishback et al., 2005). The total sample includes all counties for which there exists data all the years (reported in panel A), whereas the urban sample only includes counties with more than 25,000 population in 1900 (reported in panel B). The outcome variable is indicated in the top row. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 11: Robustness to Fiscal and Financial Development

	Dependent variable (in logs):						
	immi-grants	pop-ulation	mfg. workers	mfg. establish-ments	mfg. wage income	mfg. value added	avg. wage income native
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Total sample						
$\sum_n$Quota·FB×1910	-1.082***	-0.110	-0.196	0.0652	0.0533	0.0453	-0.0333
	(0.338)	(0.0814)	(0.221)	(0.0510)	(0.0398)	(0.0416)	(0.0378)
$\sum_n$Quota·FB×1930	-2.275***	-0.384***	-0.800**	-0.602*	-0.897**	-0.769**	-0.261***
	(0.674)	(0.146)	(0.335)	(0.365)	(0.387)	(0.321)	(0.0749)
$\sum_n$Quota·FB×1940		-0.672***	-1.130*	-1.007***	-1.368***	-1.507***	-0.282**
		(0.228)	(0.617)	(0.391)	(0.429)	(0.403)	(0.140)
Number of obs.	3,646	6,795	6,795	6,795	6,795	6,795	6,795
Number of counties	940	1,359	1,359	1,359	1,359	1,359	1,359
	Panel B: Urban sample						
$\sum_n$Quota·FB×1910	-0.531	-0.222**	0.118	-0.0542*	-0.0259	-0.0374	-0.0115
	(0.447)	(0.106)	(0.286)	(0.0297)	(0.0187)	(0.0332)	(0.0340)
$\sum_n$Quota·FB×1930	-2.468***	-0.652***	-0.183	-0.419	-0.829**	-1.336***	-0.220***
	(0.752)	(0.150)	(0.558)	(0.306)	(0.395)	(0.270)	(0.0523)
$\sum_n$Quota·FB×1940		-1.124***	-0.210	-0.825	-1.458**	-1.805***	-0.303**
		(0.224)	(0.724)	(0.546)	(0.597)	(0.451)	(0.110)
Number of obs.	492	615	615	615	615	615	615
Number of counties	123	123	123	123	123	123	123
Controls ($\times \sum I^j$):							
Banks	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tax income	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Initial outcome	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1920. The observations are at the county level for the years: 1900, 1910, 1920, and 1930 (column 1) and sample period is extended to include 1940 in the remaining columns. Baseline controls are: county and time fixed effects, state-by-year fixed effects, county-specific linear time trends, initial outcomes, measured in 1900 and 1910, interacted with a full set of year fixed effects. "Banks" is the total number of banks in 1920 and "Tax income" is county level takes. The total sample includes all counties for which there exists data all the years (reported in panel A), whereas the urban sample only includes counties with more than 25,000 population in 1900 (reported in panel B). The outcome variable is indicated in the top row. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share of this nationality (out of the total population). Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: City MFG Sector

	Dependent variable (in logs):			
	workers	establish-ments	wages	value added
	(1)	(2)	(3)	(4)
$\sum_n Quota{\cdot}FB \times 1914$	-0.278	0.419	-0.917	-1.537**
	(0.510)	(0.296)	(0.715)	(0.711)
$\sum_n Quota{\cdot}FB{\times}1925$	-0.898	-0.270	-1.881	-2.867**
	(1.035)	(0.453)	(1.332)	(1.448)
$\sum_n Quota{\cdot}FB{\times}1929$	-0.687	-0.0669	-1.466**	-2.280**
	(0.561)	(0.285)	(0.723)	(1.003)
Number of obs.	572	572	572	572
Number of cities	143	143	143	143

Notes: The table reports "flexible" OLS estimates, relatively to the omitted year 1919. The observations are at the city level for the years: 1909, 1914, 1919, 1925, and 1929. The sample includes only cities which in 1900 were larger than 25,000. All regression include: city and time fixed effects, state-by-year fixed effects, city-specific linear time trends, and the initial outcome, measured in 1909, interacted with a full set of year fixed effects. The outcome variable is indicated in the top row. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the city share of this nationality measured in 1900 (out of the total city population). Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

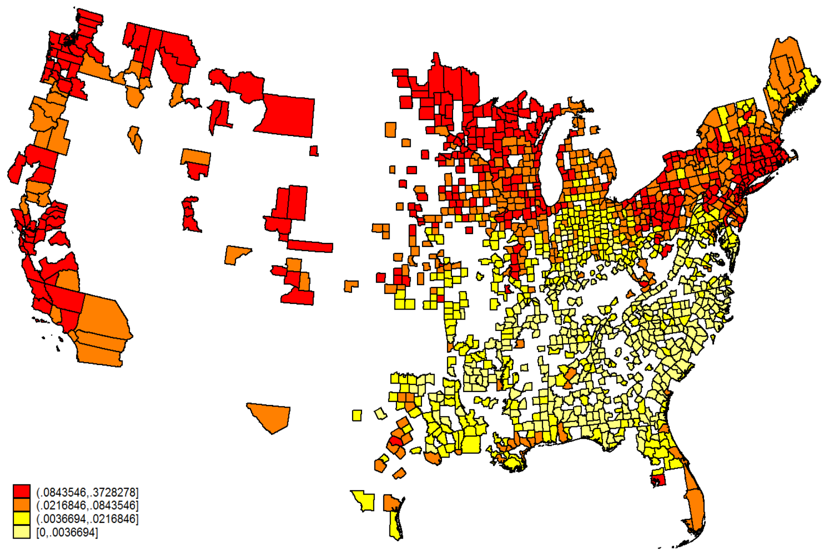
*** p<0.01, ** p<0.05, * p<0.1.

Table 13: MFG Industries at the National level

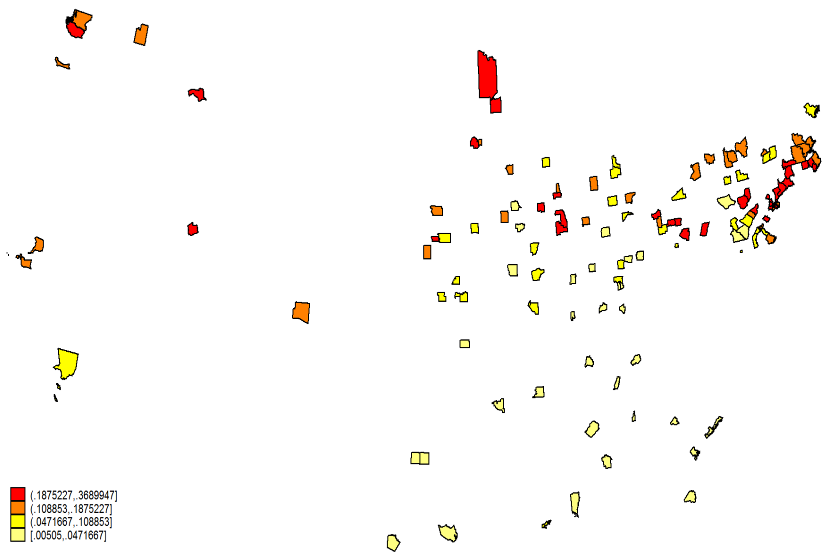
	Dependent variable (in logs):			
	workers	establish-ments	wages	value added
	(1)	(2)	(3)	(4)
$\sum_n Quota{\cdot}FB \times I^{post}$	-0.968**	2.808	-2.760**	-2.125*
	(0.346)	(2.280)	(1.296)	(1.029)
Number of obs.	143	143	143	143
Number of industries	21	21	21	21

Notes: The table reports OLS estimates. The observations are at the national industry level for the years: 1919, 1921, .., 1931. All regressions include: industry fixed effects, time fixed effects, and industry employment share. The outcome variable is indicated in the top row. Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the industry share of this nationality measured in 1900. I^{post} is an indicator equal to one in the post-treatment years (t>1919). Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.
*** p<0.01, ** p<0.05, * p<0.1.

Figure 1: Treatment intensity



Panel A



Panel B

Notes: These maps depict the treatment intensity, $\sum \text{Quota} \cdot \text{FB}$ for the counties included in the total sample (panel A), and the counties included in the urban sample (panel B). A warmer color reflects more "intense" treatment.

Appendix Tables

Table 1a: Quotas and Immigration Rates

	Dependent variable is the log immigration rate	
	total	urban
	(1)	(2)
$\sum_n Quota{\cdot}FB \times 1900$	-0.737	-0.108
	(0.820)	(0.695)
$\sum_n Quota{\cdot}FB \times 1910$	-1.210	-0.442
	(0.808)	(0.517)
$\sum_n Quota{\cdot}FB \times 1930$	-1.729***	-1.991**
	(0.588)	(0.763)
Observations	3,836	520
Counties	959	130

Notes: The table reports "flexible" OLS estimates which are relative to the omitted year 1920. The observations are reported at the county level for the period 1900-1930 (every decade). All regressions include county fixed effects and state-by-year fixed effects, and initial outcomes (measured in 1900 and 1910) interacted with a full set of year fixed effects. The total sample includes all counties for which there exists data all the years, whereas the urban sample only includes counties with more than 25,000 population in 1900. The outcome variable is the log immigration rate which is extracted using information on the year of immigration to the US from IPUMS and population data from Haines (2010). Quota is a dummy variable indicating whether the quotas were binding for nationality n and FB is the county share (out of the total population) of this nationality. Constants are not reported. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the state level.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.