Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration*

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Abstract

In this paper, I jointly investigate the political and the economic effects of immigration, and study the causes of anti-immigrant sentiments. I exploit exogenous variation in European immigration to US cities between 1910 and 1930 induced by World War I and the Immigration Acts of the 1920s, and instrument immigrants' location decision relying on pre-existing settlement patterns. I find that immigration triggered hostile political reactions, such as the election of more conservative legislators, higher support for anti-immigration legislation, and lower redistribution. Exploring the causes of natives' backlash, I document that immigration increased natives' employment, spurred industrial production, and did not generate losses even among natives working in highly exposed sectors. These findings suggest that opposition to immigration was unlikely to have economic roots. Instead, I provide evidence that natives' political discontent was increasing in the cultural differences between immigrants and natives. Results in this paper indicate that, even when diversity is economically beneficial, it may nonetheless be socially hard to manage.

Keywords: Immigration; Political Backlash; Age of Mass Migration; Cultural Diversity
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1 Introduction

The recent immigration waves to Europe and the US have generated a heated political debate, and proposals to introduce or tighten immigration restrictions are becoming increasingly common. A growing literature has shown that the inflow of immigrants has increased support for populist, far right parties in several Western democracies (Becker and Fetzer, 2016; Dustmann et al., 2019; Halla et al., 2017). However, despite the rising importance of immigration in the political arena, both the causes and the consequences of anti-immigration sentiments are not fully understood.

First, despite the evidence on voting, the link between support for anti-immigration parties and the actual policies implemented in response to immigration has not been systematically investigated. This may be partly due to the fact that, with some recent exceptions, anti-immigrant votes often accrue to marginal parties that are unable to influence the design and the implementation of government policies. Since we ultimately care about the actions and the reforms undertaken by political actors, it is crucial to understand which policies, if any, are affected by immigration, and why. Will legislation regulating the immigration regime be introduced? Will redistribution and taxation be changed to prevent immigrants from having access to public goods?

Second, evidence on the causes of anti-immigration sentiments is mixed, and two main hypotheses have been proposed. The first one is economic in nature, and argues that political discontent emerges from the negative effect of immigration on natives' employment and wages.[1] While this idea is consistent with findings in Borjas (2003), Dustmann et al. (2017), and Monras (2018) among others, it is in contrast with results in Card (2001, 2005), Foged and Peri (2016), and Ottaviano and Peri (2012) who document that immigrants have a negligible, or even positive, impact on natives' earnings.[2] The second hypothesis is that natives' backlash has cultural roots. Both today and in the past, a recurring theme in the rhetoric of anti-immigration politicians is that immigrants' cultural diversity is an obstacle to social cohesion and a menace to the values of hosting communities (Abramitzky and Boustan, 2017). Historical and anecdotal accounts present many examples of cultural opposition to immigration (Higham, 1955; Spiro, 2009). Yet, even though local amenities (e.g. crime or school quality) have been shown to be important determinants of natives' reactions to immigration (Card et al., 2012, Halla et al., 2017, and Sniderman et al., 2004), there is scant evidence on the extent to which culture directly triggers political backlash and policy change.

[1] Another, somewhat related economic explanation for natives' opposition to immigration is that immigrants are (often incorrectly) perceived as a fiscal burden (Alesina et al., 2018).

[2] Clemens et al. (2018), Lafortune et al. (2019), and Lewis (2011) find that the labor market effects of immigration can be largely mediated by firms' investment and technology adoption. See Lewis and Peri (2015) for a detailed review of this literature.

This paper studies in a unified framework the political and economic effects of immigration across US cities between 1910 and 1930, a period when the massive inflow of European immigrants was abruptly interrupted by two major shocks, World War I and the Immigration Acts (1921 and 1924). Between 1850 ad 1915, during the Age of Mass Migration, more than 30 million people moved from Europe to the United States (Abramitzky and Boustan, 2017), and the share of immigrants in the US population was even higher than it is today (Figure 1).[3] Also at that time, anti-immigration sentiments were widespread, and the introduction of immigration restrictions was advocated on both economic and cultural grounds.

This setting offers three main advantages. First, by jointly analyzing economic and political outcomes, I can test the relationship between economic insecurity and natives' political reactions. Given the disagreement in the literature on the economic effects of immigration (Dustmann et al., 2016), this is crucial to shed light on the causes of natives' backlash. Second, since cities were independent fiscal units and because the US went through a major change in its (immigration) policy regime, I can not only study the impact of immigration on voting, but I can also measure its effects on actual policies – both at the local and at the national level. Finally, in contrast with more recent immigration episodes where migrants often come from culturally homogeneous groups, at the beginning of the twentieth century there existed wide variation in immigrants' cultural background (e.g. in terms of language or religion). Exploiting such variation, I can assess how the political effects of immigration varied with cultural distance between immigrants and natives.

The key feature of this empirical setting is that the national shocks to immigration triggered by WWI and the Immigration Acts affected migration flows from different sending regions to different degrees. Since immigrants tend to cluster along ethnic lines (Card, 2001), the differential effect of these shocks across European countries generated significant variation in the number as well as in the mix of immigrants received by US cities over time. I exploit such variation to overcome the concern that immigrants' location decision might be itself influenced by political and economic conditions. In addition to controlling for city time invariant and state time varying unobserved characteristics, I construct a "leave-out" version of the shift-share instrument commonly adopted in the literature (Card, 2001).

The shift-share instrument rests on the empirical regularity that immigrants cluster geographically in receiving countries, and newcomers tend to settle where their ethnic community is larger, due to family ties and social networks (Stuart and Taylor, 2016). Starting from this observation, I predict the number of immigrants received by US cities over time by interacting 1900 settlements with subsequent migration flows from each sending region,

[3]The total number of foreign-born residents is, however, higher today. Also, contemporary immigration is underestimated because of the presence of large numbers of undocumented immigrants (see the dashed line in Figure 1 and Borjas, 2016).

net of the individuals that eventually settled in a given city's metropolitan statistical area (MSA).[4]

The validity of this instrument hinges on one critical assumption: the city-specific characteristics that attracted early movers from any given country must not affect the evolution of local economic and political conditions in subsequent decades (see also Borusyak et al., 2018, and Goldsmith-Pinkham et al., 2018). To assess the validity of this assumption, I perform several checks. First, I show that pre-period changes in the outcomes of interest are uncorrelated with subsequent immigration predicted by the instrument. Second, I separately control for a time-varying (predicted) measure of industrialization and interact year dummies with several pre-migration city characteristics. I devote special attention to the concern that the 1900 fraction of immigrants, which mechanically predicts higher immigration in subsequent decades, might have had independent, time-varying effects on either economic or political outcomes. To do so, I allow my estimates to vary flexibly depending on city 1900 immigrant population as well as on the size of the ethnic enclave from each sending country.

Next, I deal with the possibility that aggregate migration flows from each sending country may be endogenous to local economic conditions in US cities. In online appendix B1, I replace the actual number of immigrants (from each origin) entering the United States with that predicted exploiting variation solely induced by World War I and the Immigration Acts.[5] Finally, I exploit the fact that WWI and the Immigration Acts unexpectedly altered both the number and the composition of immigrants, and document that these shocks lowered the serial correlation in migration flows to US cities. I provide evidence that, in this specific context, the shift-share instrument is unlikely to conflate the short and the long run responses of the economy to immigration (Jaeger et al., 2018).

I begin my analysis by studying the political effects of immigration. First, I find that cities cut public goods provision and taxes in response to immigration. In the context of a sticky political process, immigration might have reduced public spending and tax revenues per capita by mechanically increasing city population. However, I show that not only per capita, but also total public spending and tax revenues were lower in cities receiving more immigrants. Moreover, the reduction in tax revenues was entirely driven by declining tax rates, while the fall in public goods provision was concentrated in categories where either inter-ethnic interactions are likely to be more salient (e.g. education) or poorer immigrants would get larger implicit transfers (e.g. sewerage, garbage collection). These findings suggest that immigrants were perceived as a fiscal burden, and that immigration reduced natives' demand for redistribution. Consistent with this interpretation, in 1907, Prescott Hall, one

[4] In my baseline specification, I consider only immigration from Europe (online appendix Table A1), but results are robust to extending the analysis to all other non-European countries (online appendix D5).

[5] In online appendix B2, similarly to Sequeira et al. (2019), I also construct a measure of predicted immigration determined uniquely by weather shocks in origin countries.

of the leaders of an influential anti-immigration movement, the *Immigration Restriction League*, stated that US cities were "receiving a great many immigrants who are not only worth nothing to the country, but are a positive [public] expense".

Second, immigration reduced the pro-immigrant party's (i.e., Democrats) vote share, and was associated with the election of more conservative representatives. While the ideological distance on immigration between Republicans and Democrats was less pronounced than it is today, most naturalized immigrants supported the Democratic Party. The Irish are the most emblematic example (Erie, 1990), but this was true also for other ethnic or religious groups such as the Italians and the Catholics (Kleppner, 1979; Luconi, 1996). Finally, and most directly reflecting natives' demand for anti-immigration policies, members of the House representing cities more exposed to immigration were significantly more likely to support the National Origins Act of 1924, which put an end to the era of unrestricted immigration to the US.

After establishing that immigration triggered widespread, hostile political reactions, I investigate the potential causes for natives' backlash. I start from the first, and perhaps most obvious possibility: immigrants might have increased labor market competition, lowering wages and raising unemployment among native workers. Yet, in contrast with this idea, I find that immigration had a positive and statically significant effect on natives' employment. My estimates are quantitatively large, and imply that a 5 percentage points increase in immigration (roughly one standard deviation) increased natives' employment by 1.4 percentage points, or by 1.6% relative to its 1910 level.

Since no comprehensive data on wages is available for this period, as commonly done in the literature (e.g. Abramitzky et al., 2012, 2014), I proxy for natives' income using (log) occupational scores, and document that immigration promoted natives' occupational upgrading.[6] These results were made possible by two mechanisms. First, immigration increased firms' investment and productivity, generating an outward shift in labor demand. Second, because of complementarity, natives moved away from occupations that were more exposed to immigrants' competition and specialized in jobs where they had a comparative advantage and, because of discrimination, immigrants did not have access to.

Even though immigration had, on average, positive effects on natives' employment and occupational standing, it is possible that economic losses were concentrated on some specific groups, who were able to mobilize and demand political protection. Although I cannot entirely rule out this interpretation, I provide evidence against it. First, I document that even in occupations that were highly exposed to immigrants' competition, natives were not more likely to be unemployed. Second, using data digitized from the Census of Manufactures,

[6] As discussed below, occupational scores assign to an individual the median income of his job category in 1950, and can thus be used as a proxy for lifetime earnings (Abramitzky et al., 2014).

I show that in the sector most exposed to immigration (i.e. manufacturing), there was no significant reduction in wages. These data do not distinguish between immigrant and native workers, and new immigrants tend to be closer substitutes for previously arrived migrants than for natives.[7] Hence, these findings can be interpreted as a lower bound for the negative effect (if any) – or, equivalently, as an upper bound for the absolute value of the effect – of immigration on natives' earnings.

The last part of the paper seeks to understand why, if immigration was on average beneficial and had no tangible economic costs, it nonetheless triggered political backlash. I show that natives' political reactions were increasing in the cultural distance between immigrants and natives, suggesting that backlash may have had, at least in part, non-economic foundations. I proxy for cultural diversity using both religion and linguistic distance. The use of religion, in particular, is motivated by the historical evidence that, at that time, nativism often resulted in anti-Semitism and anti-Catholicism (e.g. Higham, 1955; D'Amico and Tabellini, 2018).

While immigrants from Protestant and non-Protestant countries had very similar effects on natives' employment and on economic activity, they triggered very different political reactions. Only Catholic and Jewish, but not Protestant, immigrants induced cities to limit redistribution, favored the election of more conservative legislators, and increased support for the 1924 National Origins Act. These patterns also suggest that political backlash was unlikely to arise from increased inequality, or that cities reduced redistribution only because the median voter became richer (e.g. Meltzer and Richard, 1981). If this were to be the case, Protestant and non-Protestant immigration should have lead to similar political responses.

My findings are consistent with a long-standing idea in the literature that diversity can be economically beneficial because of gains from specialization and complementarity (Alesina and La Ferrara, 2005), but may be politically hard to manage, resulting in lower preferences for redistribution (Dahlberg et al., 2012), more limited public spending (Alesina et al., 1999), and higher conflict (Bazzi and Gudgeon, 2016). My paper is also related to the recent and growing literature on the Age of Mass Migration.[8] Abramitzky et al. (2012, 2014, 2018) study the selection and the assimilation of European immigrants during the Age of Mass Migration, while Ager and Hansen (2017), Lafortune et al. (2019), and Sequeira et al. (2019) investigate their impact on contemporaneous and long-run economic development.

I complement this literature in two ways. First, by contrasting the economic and the political impact of immigration, and by exploiting variation in immigrants' background, I assess how cultural and economic factors contribute to trigger natives' backlash. Second, I

[7] This finding has been documented in a number of works in the literature (e.g. Boustan, 2009; Goldin, 1994). I also provide direct evidence for it in online appendix E3.

[8] Goldin (1994) is an early and seminal contribution on the poliltical economy determinants of the introduction of the Immigration Acts.

study the effects of immigration on key policy variables, such as tax rates and public spending – outcomes for which, as noted in Card (2009) and Borjas (2016) among others, despite the large debate on the consequences of immigration, little is known.[9]

The remainder of the paper is organized as follows. Section 2 describes the historical background. Section 3 presents the data. Section 4 lays out the empirical strategy, constructs the instrument for immigration, and presents first stage results. Section 5 studies the political effects of immigration. Section 6 investigates the effects of immigration on natives' employment and on economic activity, and provides evidence against the idea that natives' backlash was economically motivated. Section 7 documents the link between political discontent and cultural differences between immigrants natives. Section 8 summarizes the main robustness checks, which are then described in detail in the online appendix. Section 9 concludes.

2 Historical Background

2.1 The Age of Mass Migration

Between 1850 and 1915, more than 30 million people moved from Europe to the US. Until 1890, most immigrants came from the British Isles, Germany, and Scandinavia, but, from the late 1880s, immigration from Southern and Eastern Europe increased steadily, as the costs of migration fell with the advent of steam technology (Keeling, 1999). In 1870, almost 90% of the foreign born came from Northern and Western Europe, whereas less than 5% of immigrants had arrived from Southern and Eastern Europe (Figure 2). By 1920, however, the situation had changed dramatically, with the share of migrant stock from new source countries being as high as 40%. Europeans from new regions were culturally farther from natives and significantly less skilled than those from old sending regions (Hatton and Williamson, 1998, 2006). For instance, while literacy rates of immigrants that entered the US between 1900 and 1910 were very close to one for all old sending countries, they were significantly lower for new source regions (online appendix Figures A1 and A2).

The shift in the composition of immigrants and concerns over their assimilation induced Congress to establish a commission that, between 1907 and 1911, studied the economic and social conditions of immigrants (Higham, 1955). In 1911, the Immigration Commission recommended the introduction of immigration restrictions, and in 1917, after decades of heated political debate, Congress passed a literacy test requiring that all immigrants entering

[9] An exception is the paper by Chevalier et al. (2018) on the effects of internal migration in post-WWII Germany on local taxation. Also, in a companion paper (Tabellini, 2018), I study how the migration of southern born African Americans affected public goods provision and government finances in northern cities during the first wave of the Great Migration.

the United States had to be able to read and write (Goldin, 1994).

Even before the adoption of the literacy test, in 1914, the Age of Mass Migration came to an abrupt end due to the onset of World War I, which drastically reduced European immigration between 1915 and 1919 (Figure 3). In 1920, despite the literacy test, migration flows increased again to their 1910 levels, fueling nativist movements and generating even stronger political pressure to adopt more effective measures to curb immigration. Figure A3 plots trends of migration flows (right axis) and of the number of articles in local newspapers referring to immigration (left axis) over time, and shows that both fell dramatically during WWI, but then increased again once the war was over. In response to the growing demand for immigration restrictions, in 1921 and 1924 Congress finally passed the Immigration Acts to limit the number of immigrants that could enter the United States in a given year by introducing country-specific quotas based on 1890 immigrants' population.[10]

Both World War I and the Immigration Acts affected different sending countries in different ways. In particular, quotas were set so as to limit the inflow of immigrants from new sending regions, while favoring that from old sources such as the UK, Germany, and Scandinavia. Figure A4 shows the changing composition of immigrants entering the United States during the previous decade between 1900 and 1930. Until 1920, the majority of recent immigrants came from Eastern and Southern Europe, but this trend was abruptly reversed in the 1920-1930 decade, when the share of Anglo-Saxon and Scandinavian immigrants increased as a result of the Immigration Acts. Since immigrants tend to cluster along ethnic lines (Card, 2001), the post-1915 events generated substantial variation in the number as well as in the mix of immigrants received by US cities over time (Figures A5 and A6). This is the variation I exploit in my empirical analysis.

2.2 Immigrants and the US Economy

Historical accounts tend to view immigrants as one of the key determinants of American industrialization and economic development during the Age of Mass Migration. When describing the economic impact of European immigrants, historian Maldwyn Jones wrote that "The realization of America's vast economic potential has...been due in significant measure to the efforts of immigrants. They supplied much of the labor and technical skill needed to tap the underdeveloped resources of a virgin continent" (Jones, 1992, pp. 309-310). Similarly, John F. Kennedy argued that "every aspect of the American economy has profited from the contribution of immigrants" (Kennedy, 1964, p. 88).

[10] With the 1924 National Origins Act, the total number of immigrants that could be admitted in a given year was capped at 150,000. In 1921, quotas were specified reflecting the 1910 composition of immigrants. However, they were rapidly changed to 1890 to limit immigration from new sending countries even further (Goldin, 1994).

During the Age of Mass Migration, the US economy had large potentials for growth. Economic historians argue that, in this context, immigrants provided a cheap and unskilled supply of labor which could not only be absorbed, but that may have even allowed industries to expand (Foerster, 1924), in turn creating new job opportunities for native workers. Even though some studies have found a negative effect of immigrants on wages (Goldin, 1994), labor shortage was a recurring theme in this historical period. For instance, in a 1906 article, the New York Times was reporting that "Need of labor is the universal cry. Demand in all parts of the country is greater than supply. Not enough immigrants. Statements from agents show that men are scarce in all the States".

Since immigrants, especially from Eastern and Southern Europe, were unskilled and had low levels of English proficiency, they may have benefitted natives because of complementarity and gains from diversity (Foged and Peri, 2016). Along these lines, in his 1971 *The Transformation of the American Economy*, economic historian Robert Higgs argues that "the rapid pace of industrial expansion has increased the number of skilled and supervisory positions so fast that practically all the English speaking employees have had the opportunity to rise on the scale of occupations" (Higgs, 1971, p. 420).

2.3 Immigration and Natives' Backlash

Despite the positive views on the contribution of immigrants to the American economy expressed by economic historians, Europeans, especially from new sending countries, faced strong political opposition. Natives' backlash culminated in the passage of the literacy test of 1917 and, more importantly, of the Immigration Acts of 1921 and 1924, which were explicitly introduced to shut down immigration from "undesirable sources". Goldin (1994) argues that concerns about unemployment and labor market competition were the main motivation for the immigration restrictions of the 1920s. Undoubtedly, the coincidence of large immigration flows with the severe macroeconomic recessions of 1907, 1913-1914, and 1919 increased the perception among native workers that immigrants were threatening American standards of living.

However, while economic considerations certainly played a role, anti-immigration sentiments tended to have deep cultural roots (Higham, 1955; Abramitzky and Boustan, 2017). This idea is very effectively summarized in a 1921 statement by Irving Fisher, who argued that "If we could leave out of account the question of race and eugenics...I should, as an economist, be inclined to the view that unrestricted immigration...is economically advantageous...the core of the problem of immigration is...one of race and eugenics" (Leonard, 2005). On a similar vein, in 1896, the first president of the American Economic Association, Francis A. Walker, claimed that the American standard of living and the quality of American

citizenship had to be protected "from degradation through the tumultuous access of vast throngs of ignorant and brutalized peasantry from the countries of Eastern and Southern Europe" (Greenwood and Ward, 2015).[11]

Anti-immigration sentiments were most often directed towards two groups. First, Jews and Catholics, whose values were perceived as being different from the Puritan tradition prevailing in the US at that time.[12] Second, immigrants from Eastern and Southern Europe, who were culturally and linguistically distant from natives and, because of their lower socio-economic status, were regarded as belonging to inferior races. Countless statements by politicians and newspapers articles provide examples of how Eastern and Southern European immigrants were perceived at the time. For instance, in 1916, congressman Thomas Abercrombie claimed that "The color of thousands of them [i.e. the new immigrants: Mediterraneans, Slavs, Jews] differs materially from that of the Anglo-Saxons" (Higham, 1955), while the editor of the *Saturday Evening Post*, Kenneth Roberts, in a 1920 article wrote that "if a few more million members of the Alpine, Mediterranean and Semitic races are poured among us, the result must inevitably be a hybrid race of people as worthless and futile as the good-for-nothing mongrels of Central America and Southeastern Europe".

3 Data

My analysis relies on a balanced panel of 180 US cities for the three Census years 1910, 1920, and 1930. The sample includes all cities with at least 30,000 residents in each of the three censuses, and where at least some Europeans were living in 1900 (see Figure A7 and Table A2 for the complete list of cities). I restrict attention to cities with at least 30,000 residents because below this population threshold data on public spending and government finances, two of the key outcomes of my paper, were not reported. To study the economic and political effects of immigration, I combine data from several sources.

Immigration and city population. Data on city population and on the number of immigrants by country of origin at the city and at the national level were taken from the decennial US Census of Population, made available by IPUMS (Ruggles et al., 2015).[13] For

[11] Consistent with this qualitative evidence, D'Amico and Tabellini (2018) find that immigration induced local newspapers to adopt more racist terms when referring to immigrants, especially from Eastern and Southern Europe.

[12] Around the time of World War I, Jews were deemed responsible for promoting the war in order to make profits out of it. For example, in 1915 Henry Ford claimed he knew "who caused the war: German-Jewish bankers" (Watts, 2009, p. 383). During the Red Scare, and in the inter-war period more generally, Jews were often blamed for being at the origin of Bolshevism and the worldwide diffusion of Communism.

[13] See Table A1 for the list of European countries used in my work. In online appendix D5, I extend the sample to include all foreign born individuals. I classify individuals based on their country of origin following the classification made by IPUMS (Ruggles et al., 2015).

1900, I use the 5% sample, while for 1910, 1920, and 1930, I rely on the full count census datasets.

Natives' labor market outcomes. Restricting the sample to native men in working age, I compiled data on employment, literacy, and occupation from the US Census of Population.[14] Since until 1940 wage data are not available, I proxy for natives' income using (log) occupational scores, as commonly done in the literature (e.g. Abramitzky et al., 2012 and Abramitzky et al., 2014). Occupational scores assign to an individual the median income of his job category in 1950 and, as discussed in Abramitzky et al. (2014), represent a proxy for lifetime earnings.

Economic activity. I digitized city-level data from the quinquennial Census of Manufactures between 1904 and 1929 for the following variables for the manufacturing sector: value added by manufacture, value of products, establishment size, capital utilization (proxied by horsepower), total employment, and average wages.[15] Wage data is a potentially valuable piece of information, since, as noted above, the US Census of Population did not collect income data prior to 1940. While manufacturing wages were not separately reported for immigrants and natives, they can nonetheless be used to complement results on employment, skill ratios, and natives' occupational scores.

Public spending and government finances. Data on public spending and city finances were digitized from the Financial Statistics of Cities for years 1906, 1910, 1919, and 1930.[16] These are annual reports, available from 1906 onwards for cities with population above 30,000 (until 1934) or 100,000 (from 1934 onwards). From the Financial Statistics of Cities, I obtained data on land area, total and property tax revenues, property values, property tax rates, and public spending (total and by category).

Presidential elections. Data on electoral returns (votes shares and turnout) for Presidential elections come from Clubb et al. (1990). Since these data are available only at the county level, I aggregated them up to the MSA, fixing boundaries to 1940, and performed the analysis using MSA-level immigration, matching cities to the corresponding MSA.[17] Because Presidential elections are held every four years, I computed the average between the

[14] In my analysis, I focus on the age range 15-65, but results are unchanged when selecting different age combinations (see also Carlana and Tabellini, 2018). In 1920, the US Census did not report employment status, but rather only an indicator for holding any gainful occupation. For this year, I imputed values from the latter to proxy for employment. I also report results based solely on labor force participation rather than employment.

[15] I use 1909, 1919, and 1929 data to proxy for 1910, 1920, and 1930 respectively. I make use of 1904 data to test if pre-period changes in outcomes are correlated with subsequent changes in predicted immigration.

[16] Since data for 1920 is missing, I digitized the 1919 and 1921 volumes. Results are robust to using 1921 in place of 1919, but 1919 is preferable since 1921 data was not reported for several cities. Data for 1906 is used to test the validity of the empirical strategy.

[17] Matching cities to MSAs lowers the number of units from 180 to 127. However, data on Presidential elections are not available for Washington DC, further reducing the number of MSAs to 126.

closest two elections after each Census year. That is, for 1910 and 1930, I averaged electoral results from 1912 and 1916 and from 1932 and 1936 respectively, while for 1920, I considered 1920 and 1924. Results are unchanged when taking the average from the two closest election years, i.e. 1908 and 1912 for 1910, and 1928 and 1932 for 1930 (see online appendix D8).

Legislators' ideology. I collected data on congressmen ideology between 1910 and 1930 from Voteview, for Congresses 61, 66, and 71 respectively.[18] Following Autor et al. (2017) as well as a vast political science literature, I proxy for politicians' ideology using the first dimension of the Poole-Rosenthal DW Nominate scores, which rank congressmen on an ideological scale from liberal to conservative using voting behavior on previous roll-calls (Poole and Rosenthal, 1985; McCarty et al., 2006). To exploit local geographic variation, I restrict my attention to the House of Representatives, and use digital boundary definitions of US congressional districts from Lewis et al. (2013) to match cities to their corresponding district in any given year.

When constructing this dataset, two problems must be dealt with. First, boundaries of congressional districts vary over time. Second, a single congressional district may represent multiple cities, while the same city may belong to more than one district. To address these issues, I follow Autor et al. (2017) and conduct the analysis at the city by congressional district level. The city-to-congressional district mapping is almost identical for the 66^{th} and the 71^{st} Congress, but redistricting between the 61^{st} and the 66^{th} Congress, especially in Massachusetts and Pennsylvania, prevents the construction of a balanced panel which includes all the cities in my sample. Below, I present results both for the unbalanced panel and for the balanced panel of cities whose congressional districts were unchanged.[19]

Representatives' voting behavior. Data on voting patterns on the National Origins Act of 1924 come from Swift et al. (2000). This dataset includes the name, the district represented, the main demographic characteristics, and the voting behavior on any rollcall of each representative in all US Congress between 1789 and 1989. As for congressmen ideology, I focus on the House of Representatives and conduct the analysis at the city by congressional district, matching each representative to the corresponding city (or cities) in my sample in the 68^{th} Congress (when the National Origins Act was passed).[20]

Table 1 reports the summary statistics for the main variables used in my analysis. City population ranges from more than 6.9 million (New York City in 1930) to as little as 30,200 (Pasadena in 1910). There is also wide variation in the fraction of immigrants across cities and over time, which was higher in the northeastern states of New Jersey, New York, Con-

[18] To assess the validity of the empirical strategy, I also compiled data for the 56^{th} Congress.

[19] The unbalanced and the balanced panels are composed respectively of 157 and 146 city to congressional district units.

[20] Whenever multiple congressmen represent the same city, I average their votes on the Immigration Act to create a unique value, which is then assigned to that city.

necticut, and Massachusetts, and lower in the US South. As already discussed in Section 2, immigration fell significantly between 1910 and 1930, because of both World War I and the Immigration Acts: in 1910, the fraction of immigrants over city population was, on average, 0.18, but this number fell to 0.12 in 1930. Even starker was the decline in the fraction of foreign born that entered the United States in the previous decade, which moved from an average of 0.08 in 1910 to 0.02 in 1930.

Immigration and most of the fiscal data are available for all the 540 city-year observations in my sample. However, employment outcomes were missing for Sacramento (CA) and New Bedford (MA) in 1920, whereas data from the Census of Manufactures were not reported for a handful of cities, leaving me with 538 and 525 observations respectively.[21] Finally, aggregating cities to MSAs (for Presidential elections) and to congressional districts (for legislators' ideology) reduces the number of observations to 378 and 470 respectively.

4 Empirical Strategy

In this section, I introduce the baseline estimating equation (Section 4.1), construct the instrument for immigration (Section 4.2), and report first stage results (Section 4.3).

4.1 Baseline Estimating Equation

To study the political and economic effects of immigration, I focus on the three Census years between 1910 and 1930, and I estimate

$$y_{cst} = \gamma_c + \delta_{st} + \beta Imm_{cst} + u_{cst} \tag{1}$$

where y_{cst} is the outcome for city c in state s in Census year t, and Imm_{cst} is the fraction of immigrants over city population. γ_c and δ_{st} are city and state by year fixed effects, implying that β is estimated from changes in the fraction of immigrants within the same city over time, compared to other cities in the same state in a given year. Since city population could itself be an outcome of immigration, the number of immigrants is scaled by predicted (rather than actual) city population, constructed by multiplying 1900 population by average urban growth in the US, excluding that of the Census division where the city is located.[22]

[21] Data from the Census of Manufactures were not available for Superior (WI), Washington DC in 1909 and 1919, and for Flint (MI), Galveston (TX), Huntington (WV), Lexington (KY), McKeesport (PA), Pueblo (CO), Quincy (IL), and Roanoke (VA) in 1929.

[22] Predicted city population is calculated as $\hat{P}_{cst} = Pop_{c,1900} \cdot g_t^{-R}$, where $Pop_{c,1900}$ is 1900 city population and $g_t^{-R} \equiv \frac{Pop_t^{-R} - Pop_{t-1}^{-R}}{Pop_{t-1}^{-R}}$ is urban population growth between Census year t and $t-1$, constructed leaving out the region of city c.

In online appendix D5, I also report results obtained when scaling immigration by either contemporaneous or 1900 city population. Standard errors are clustered at the MSA level, and MSA boundaries are fixed to 1940 in order to keep geography constant.

In my baseline specification, Imm_{cst} refers to the stock of European immigrants that entered the United States during the previous decade. I impose this sample restriction because, at that time, immigrants could apply for citizenship after 5 years (Shertzer, 2016). While historical accounts suggest that after 1910 immigrants' political engagement fell steadily (Kleppner, 1982), focusing on recently arrived immigrants allows me to more confidently interpret my findings on political outcomes as natives' reactions, rather than as the direct effect of immigrants' preferences. Results are unchanged when considering all immigrants, regardless of their arrival year and irrespective of their country of origin (online appendix D5).

4.2 Instrument for Immigration

A priori, we may expect immigrants to be attracted to cities with better job opportunities, or with more appealing tax-public spending bundles. Alternatively, immigrants might settle in otherwise declining cities, where house prices are lower. In either case, OLS estimates of equation (1) will likely be biased. To deal with this endogeneity problem, I construct a modified version of the shift-share instrument (Card, 2001). The instrument predicts the number of immigrants received by US cities over time by interacting 1900 settlements of different ethnic groups with subsequent migration flows from each sending region, excluding individuals that eventually settled in a given city's MSA. Formally, Imm_{cst} in (1) is instrumented with

$$Z_{cst} = \frac{1}{\hat{P}_{cst}} \sum_{j} \alpha_{jc} O_{jt}^{-M} \tag{2}$$

where $\hat{P}_{cst}$ is predicted city population; α_{jc} is the share of individuals of ethnic group j living in city c in 1900; and O_{jt}^{-M} is the number of immigrants from country j that entered the US between t and $t-1$, net of those that eventually settled in city c's MSA.[23]

The instrument constructed in equation (2) exploits two sources of variation: first, cross-sectional variation in the share of individuals from each ethnic group living in different US cities in 1900 (α_{jc}); second, time-series variation induced by changes in the total number of immigrants from any sending region entering the United States in a given decade (O_{jt}^{-M}). Figure C1 in online appendix C1 presents a simple example for three cities (Chicago, Milwaukee, and San Francisco) and two ethnic groups (Italians and Germans) to illustrate the variation underlying the instrument. Online appendix C2 describes the cross-sectional

[23] A similar "leave-out" strategy is also used in Burchardi et al. (2019).

variation in 1900 immigrant settlements.

4.2.1 Identifying Assumptions and Instrument Validity

The key identifying assumption behind the instrument is that cities receiving more immigrants (from each sending area) before 1900 must not be on different trajectories for the evolution of economic and political conditions in subsequent decades (see also Borusyak et al., 2018, and Goldsmith-Pinkham et al., 2018). This assumption can be violated for two main reasons.

First, if the characteristics of cities that attracted early immigrants (from each sending country) had persistent, confounding effects on migration patterns as well as on changes in the outcomes of interest. I deal with this concern in two different ways. First, I show that the pre-period change in outcomes of interest is uncorrelated with subsequent changes in immigration predicted by the instrument. Second, I augment my baseline specification by: *i*) separately controlling for a time varying predicted measure of industrialization;[24] and *ii*) including interactions between year dummies and several 1900 city characteristics that might have attracted more migrants (from each sending country) before 1900 and may have had a time varying effect on economic and political conditions across cities. I devote special attention to one such characteristic: 1900 immigrant population.[25]

By construction, the instrument predicts higher immigration to cities with a larger number of immigrants at baseline. If larger immigrant stocks also had an independent and time varying effect on city politics or labor markets, the identifying assumption would be violated. To address this concern, I start by flexibly controlling for interactions between year dummies and 1900 city and immigrant population. This implies that the effects of immigration are identified exploiting variation only in the ethnic composition of immigrant enclaves across cities, holding constant the size of their foreign born populations. Next, I include interactions between year dummies and the share of immigrants from each European country, α_{jc} in equation (2), to assuage concerns that specific immigrant groups (e.g. the Irish or the Italians) were more likely to settle in cities where they could influence city politics or alter economic conditions.

The second reason why the identifying assumption can be violated is that outmigration from each European country might not be independent of cross-city pull factors systematically related to 1900 settlers' country of origin (Borusyak et al., 2018). To deal with this potential threat, I construct an alternative version of the instrument where the number of immigrants from each European country entering the US is predicted exploiting solely vari-

[24] Following Sequeira et al. (2019), I construct this variable by interacting 1900 industry shares with national growth rates.

[25] See online appendix D1 for a detailed discussion of all other variables.

ation generated by WWI and the Immigration Acts. I describe this strategy in detail in online appendix B1, and only briefly review the main steps in the next paragraph.[26]

As discussed in Section 2, WWI and the Immigration Acts induced large and exogenous variation both in the number and in the ethnic composition of immigrants arriving in the US over time. To directly exploit such variation, I start by taking stacked first differences of equation (1). Next, I replace the actual number of immigrants entering the US from each country (O_{jt}^{-M}) with a measure of predicted immigration. For the 1910-1920 decade, the number of immigrants is predicted using WWI. I assume that if a country was not part of the Allies, its immigration was completely shut down, whereas if a country belonged to the Allies, there was no change in immigration. For the 1920-1930 decade, instead, O_{jt}^{-M} in (2) is replaced with the sum of the yearly quota for country j specified by the Immigration Acts of 1921 and 1924.

An additional advantage of WWI and the Immigration Acts is that these shocks lowered substantially the serial correlation in migration flows to US cities over time. This is desirable since, as noted by Jaeger et al. (2018), one potential threat to shift-share instruments for the contemporaneous period is the high persistence of migration flows. In online appendix D4, I show that the national composition of immigration changed markedly between 1910 and 1930 (Figures D3 and D4), and that the estimation strategy proposed by Jaeger et al. (2018) to deal with the issue of serial correlation can be successfully implemented in this setting (Table D8).

4.3 First Stage Results

Table 2 presents first stage results for the relationship between actual and predicted immigration, after partialling out city and state by year fixed effects. In column 1, the dependent variable is the fraction of immigrants over actual city population, and the regressor of interest is the baseline instrument constructed in equation (2). Columns 2 and 3 replicate column 1 by dividing the actual and the predicted number of immigrants by, respectively, 1900 and predicted population. In all cases, the F-stat is very high, and there is a strong and significant relationship between the fraction of immigrants and the instrument. Figure 4 reports the graphical analogue of column 3, plotting the relationship between the fraction of immigrants and the instrument, after partialling out city and state by year fixed effects.

From column 3 onwards, both the actual and the predicted number of immigrants are scaled by predicted city population. Column 4 shows that the estimates are barely affected when aggregating the unit of analysis from cities to MSAs. Next, in columns 5 and 6, I

[26] Similarly to Sequeira et al. (2019), in online appendix B2, I also construct a measure of predicted immigration that only exploits temperature and precipitation shocks in origin countries.

augment the baseline specification by including interactions between year dummies and the 1900 (log of) city and immigrants' population, and the 1904 (log of) value added by manufacture. Reassuringly, neither the economic nor the statistical significance of the coefficient is affected. Online appendix B shows that a similarly strong first stage relationship holds for the WWI and quotas instruments (Table B1) as well as for the weather shocks instrument (Table B2). Online appendix D5 further explores the robustness of the instrument(s) to the exclusion of potential outliers.

5 The Political Effects of Immigration

This section studies the political effects of immigration. First, cities cut tax rates and public spending in response to immigration (Section 5.1). Second, the inflow of immigrants reduced support for the pro-immigration party (i.e., Democrats) and increased the Republican-Democrat vote margin (Section 5.2). Third, immigration was associated with the election of more conservative representatives who were, in turn, more likely to vote in favor of the 1924 National Origins Act (Section 5.3). I conclude this section by arguing that these patterns are consistent with immigration triggering natives' backlash, and unlikely to be explained by alternative interpretations (Section 5.4).

5.1 Tax Revenues and Public Spending

At least until the Great Depression, US cities were responsible for the provision of public goods such as education, police, and spending on welfare or on infrastructure (e.g. roads, sewerage, etc.), while the federal (or the state) government played only a marginal role (Monkkonen, 1990). Also, since federal and state transfers were very limited, cities had to independently raise funds to finance their expenditures. More than 75% of cities' resources came from local taxes, with property taxes accounting for around 90% of total tax revenues (Fisher, 1996). Even though cities could issue debt, property tax rates represented the key (fiscal) policy variable at disposal of local public officials. If immigration lowered the desired level of redistribution and natives' utility from public goods consumption, one would expect to find larger reductions in tax revenues, and in particular in tax rates, in cities that received more immigrants.

Motivated by this discussion, in Table 3, I study the effects of immigration on public spending per capita (column 1) and tax rates (column 2). Throughout the paper, Panels A and B always present, respectively, OLS and 2SLS estimates. I also report the mean of the dependent variables at baseline as well as the KP F-stat for weak instruments at the bottom of all tables. In the interest of space, all robustness checks are summarized in Section 8, and

then extensively described in online appendix D.

Immigration had a negative and statistically significant effect on both public spending per capita and tax rates.[27] Coefficients in columns 1 and 2 of Panel B imply that a 5 percentage points (or, one standard deviation) increase in immigration reduced public spending per capita and property tax rates by, respectively, 5% and 7.5% relative to their 1910 average.[28] Next, exploiting the granularity of the data digitized from the Financial Statistics of Cities, Table A3 documents that the inflow of immigrants reduced total and property tax revenues per capita (columns 1 and 2). Consistent with a net reduction in tax revenues, lower tax rates were not compensated by a significant increase in either property values (columns 3 and 4) or in business taxes per capita (column 5).

If the political process whereby cities decided on redistribution were sticky, it is possible that the reduction in public spending and tax revenues per capita documented above was merely due to the mechanical effect of immigration on city population. The fact that cities reduced tax rates (Table 3, column 2) already suggests that this is unlikely to be the case. To more directly rule out this possibility, Table A4 shows that immigration reduced not only per capita (columns 1 and 2) but also total (columns 3 and 4) public spending and property tax revenues. Finally, Table A5 breaks down total expenditures across categories, and shows that spending cuts were larger for education (column 1) and for sanitation, sewerage and garbage collection (column 5), where inter-racial interactions are likely to be more salient and poorer immigrants would get larger implicit transfers.

Taken together, these findings suggest that immigration lowered (natives') demand for redistribution and induced cities to cut tax rates. One interpretation, consistent with several historical accounts (e.g. Higham, 1955; Leonard, 2016), is that the reduction in public goods provision reflected natives' backlash against immigrants, triggered by two different mechanisms. First, most immigrants, at least until 1920, came from relatively poor countries, and may have thus been perceived as a fiscal burden by natives. Second, ethnic diversity brought about by immigration might have lowered preferences for redistribution among natives (Easterly and Levine, 1997; Alesina et al., 1999). To sharpen this interpretation and rule out alternative explanations, in what follows, I turn to three additional political outcomes: electoral returns in Presidential elections (Section 5.2); the ideology of members of the House (Section 5.3.1); and, legislators' voting pattern on the 1924 National Origins Act (Section 5.3.2).[29]

[27] Data on property tax rates was not reported for the city of Pittsfield (MA) in 1930: for this reason, the number of observations in column 2 is 539, rather than 540 as in column 1.

[28] Interestingly, OLS and 2SLS estimates are very close to each other, suggesting that immigrants were unlikely to endogenously select cities that provided more redistribution.

[29] Alternative interpretations, such as the effects of immigration on the income distribution or the possibility that results in this section reflect political demands of previous immigrants, are discussed in Section 5.4.

5.2 Presidential Elections

I now investigate how immigration affected electoral outcomes in receiving places. Since prior to 1951 systematic data on municipal elections do not exist (see de Benedictis-Kessner and Warshaw, 2016), I focus on Presidential elections between 1910 and 1930. Because electoral results are only available at the county level, I aggregate them at the MSA level, using 1940 MSA definitions.[30] In column 3 of Table 3, I focus on the Democrats' vote share, again reporting OLS and 2SLS estimates in Panels A and B respectively.

The inflow of immigrants had a negative and statistically significant effect on support for Democrats, which was also economically relevant. The 2SLS coefficient in column 3 implies that a 5 percentage points increase in the fraction of immigrants reduced the Democrats' vote share by approximately 2 percentage points, or 5% relative to its 1910 mean. As shown in Table A6, the negative effect of immigration on the Democrats' vote share was accompanied by increasing support for third parties (column 2) and, to a lesser extent, for Republicans (column 1). Even if the coefficient in column 1 is not statistically significant, immigration had a strong, negative effect on the Democrats-Republicans margin (column 3). The estimates in column 3 (Panel B) imply that a 5 percentage points increase in the fraction of immigrants reduced the Democrats-Republicans margin by 2.9 percentage points, or by almost 15% relative to its 1910 mean – a sizeable effect.

While both Republicans and Democrats tried to win immigrants' support, between 1890 and 1940, most naturalized immigrants tended to vote for the Democratic party (Shertzer, 2016).[31] The Irish are probably the most emblematic example, but this was true also of other ethnic or religious groups such as the Italians and the Catholics (Luconi, 1996; Kleppner, 1979). I examined the voting behavior of members of the House who represented the 180 cities in my sample between 1910 and 1930, finding that Democrats were significantly less likely to vote in favor of both the literacy test of 1917 and the Immigration Acts of 1921 and 1924. Even after controlling for state fixed effects, immigration, and a number of 1900 city characteristics, Democratic legislators were 20 percentage points more likely to vote against the immigration restrictions.

One interpretation of these results, consistent with similar estimates for the contemporaneous period (e.g. Dustmann et al., 2019; Halla et al., 2017), is that immigration triggered natives' backlash, reducing support for the pro-immigrant party, i.e. Democrats. These ideas are corroborated by historical accounts, which document that, during the Progressive Era, political reformers were often openly racists and directly involved in the eugenic soci-

[30] As discussed in Section 3, since Presidential elections are held every four years, I computed the average between the closest two elections after each Census year. Results are unchanged when taking the average from the two closest election years (see online appendix D8).

[31] Shertzer (2016) notes that the Democratic party was particularly appealing to foreign born because of its support for ethnic parochial schools and its opposition to the prohibition of alcohol.

ety (Leonard, 2005, 2016). Moreover, the policy platform of Progressives was centered on radical urban reforms aimed at dismantling the political machines, whose main supporters were precisely the foreign born (e.g. Erie, 1990; Menes, 1999).

5.3 Congressmen Ideology and Voting Behavior

5.3.1 Legislators' Ideology

In column 4 of Table 3, I estimate the effects of immigration on the ideology of members of the House that represented the 180 cities in my sample in each Congress corresponding to the three Census years considered in my analysis, i.e. Congress 61 (1909-1911), Congress 66 (1919-1921), and Congress 71 (1929-1931). As discussed in Section 3, following Autor et al. (2017), I proxy for Congress members' ideology using the first dimension of the DW Nominate scores (Poole and Rosenthal, 1985; McCarty et al., 2006), and conduct the analysis at the city by congressional district level.[32] While most of the city-congressional district combinations did not change between 1910 and 1930, redistricting between the 61^{st} and the 66^{th} Congress prevents the construction of a balanced panel including all cities in my sample. For this reason, I present results for both the unbalanced panel (Table 3) and the balanced panel that includes only cities not affected by redistricting between 1910 and 1920 (Table 4, column 2).

Focusing on the 2SLS coefficient reported in column 4 of Table 3, immigration had a positive and statistically significant effect on legislators' Nominate scores.[33] This effect is also economically relevant: one standard deviation increase in the fraction of immigrants increases Nominate scores by approximately 0.25 standard deviations.[34] These magnitudes are close to those in Autor et al. (2017) who estimate that a one standard deviation increase in trade exposure raises Nominate scores by 0.36 standard deviations.

Since the analysis is conducted at decennial frequency, most of the effect of immigration on legislators' Nominate scores comes from the election of new, more conservative representatives, rather than from changes in the ideology of incumbent politicians.[35] Note that the increase in Nominate scores can come from the election of either more moderate (i.e. less liberal) Democrats or more conservative (i.e. less moderate) Republicans. Moreover,

[32] DW Nominate scores rank Congress members on an ideological scale from liberal to conservative using voting behavior on previous roll-calls, with higher (lower) values indicating a more conservative (liberal) ideology.

[33] The difference between OLS and 2SLS estimates indicates that immigrants endogenously settled in cities with a less hostile political environment. Column 2 of Table 4 confirms that results are similar when restricting the analysis to the balanced panel of cities to congressional districts.

[34] This number is obtained by multiplying the coefficient in column 4 (Panel B) by one standard deviation increase in immigration (0.05), and dividing it through the 1910 standard deviation in the DW Nominate scores (0.372).

[35] Indeed, only in six cases, the same congressman in office in 1910 was also in office in 1930.

since immigration had a strong impact on the Republican-Democrat vote margin (Table A6, column 3), the rise in Nominate scores may simply reflect a shift from moderate Democrats to moderate Republicans.

Columns 3 to 6 of Table 4 address these issues by studying if immigration affected the probability of electing, respectively, a liberal Democrat (column 3), a moderate Democrat (column 4), a moderate Republican (column 5), or a conservative Republican (column 6). Liberal (resp. moderate) Democrats are defined as legislators with a Nominate score below (resp. above) the median score for Democrats in the 61^{st} Congress. Likewise, a Republican legislator is classified as moderate (resp. conservative) if his Nominate score is below (resp. above) the median score for Republicans in the 61^{st} Congress. Similar results are obtained when classifying legislators relative to the four quartiles of the overall 1910 distribution of Nominate scores.

The replacement of more liberal Democrats with more moderate Democrats is not responsible for the rise in Nominate scores. In fact, even though the point estimate is not significant at conventional levels, there is a negative and quantitatively large effect of immigration on the probability of electing a moderate Democrat (column 4). Also, moderate Democrats are not replaced by moderate Republicans (column 5), but rather by conservative Republicans (column 6). Figure A8 visually displays this pattern, by plotting 2SLS coefficients reported in columns 3 to 6 of Table 4. Interpreting the magnitude of these results, a 5 percentage points increase in immigration raises the probability of electing a conservative Republican by 12.5 percentage points. This effect is, once again, close to that estimated in Autor et al. (2017), who find that a one standard deviation increase in trade exposure increases the probability of electing a conservative Republican by 17.5 percentage points.

5.3.2 Legislators' Voting Behavior and the National Origins Act

In this section, I turn to the most direct proxy for anti-immigration sentiments, namely, the voting behavior of legislators on the 1924 National Origins Act. This bill, approved in 1924 as part of the Johnson-Reed Act, was the last of a series of attempts undertaken by US Congress to restrict immigration in the early twentieth century, and remained in place until 1965. While Congress approved the literacy test in 1917 and the Emergency Quota Act in 1921, it was not until the passage of the National Origins Act that the inflow of immigrants, especially from Eastern and Southern Europe, was effectively and permanently shut down. On the one hand, even though the literacy test was accompanied by a heated political debate (Goldin, 1994), by the time of its approval it was no longer binding. On the other, the Emergency Quota Act introduced only temporary measures, which were then

made permanent (and more stringent) with the National Origins Act of 1924.[36] For these reasons, I focus on the 1924 Immigration Act, and not on its predecessors.

As for Section 5.3.1, the analysis is conducted at the city by congressional district level, and attention is restricted to members of the House who represented the 180 cities in my sample during the Congress that approved the National Origins Act, i.e. Congress 68. Since I examine voting behavior at a specific point in time, redistricting is no longer an issue. However, precisely because of the cross-sectional nature of the analysis, results should be interpreted as suggestive. To indirectly gauge the size and the direction of the potential bias due to the impossibility of including city (and state by year) fixed effects, online appendix D8 (Table D16) replicates columns 1 to 6 of Table 4 using cross-sectional regressions. Reassuringly, results remain close to those reported in the main text.

Columns 7 and 8 of Table 4 document a positive and statistically significant relationship between a legislator's propensity to vote in favor of the National Origins Act and the 1910 to 1920 change in the fraction of immigrants received by the city (or cities) he represented. Column 7 only includes state fixed effects, while column 8 also controls for a number of 1900 characteristics, such as the fraction of Europeans and of African Americans, as well as congressmen party of affiliation. Even if the magnitude of the coefficient in column 8 is somewhat lower, the association between immigration and representatives' voting behavior remains positive and significant.

The coefficient in column 8 implies that, when comparing cities at the 25^{th} and 75^{th} percentiles of immigration, legislators representing the more exposed city were more likely to vote in favor of the National Origins Act by approximately 10 percentage points. This is a large, but not unreasonable effect, given that immigration was at that time (as it is today) at the forefront of the political debate.[37]

5.4 Interpretation of Results

So far, I presented three sets of results. First, cities cut tax rates and public spending in response to immigration. Second, the inflow of immigrants reduced support for the Democratic Party. Third, cities receiving more immigrants elected more conservative members of the House of Representatives who were in turn more likely to vote in favor of the 1924 National

[36] The 1921 Emergency Quota Act temporarily limited the number of immigrants from any given country that could enter the United States to 3% of the 1910 population of each ethnic group. With the 1924 National Origins Act, which made the 1921 Immigration Act permanent, the ceiling was lowered to 2% and the "base" year was moved to 1890. These two changes were undertaken to shut down the inflow of immigrants from "undesired" sources, such as Eastern and Southern Europe. As the *Saturday Evening Post* put it, "if there is one thing we need more than another it is a little discrimination in our immigration policy" (Spiro, 2009).

[37] Consider, for instance, that one standard deviation increase in the mortgage default rate during the 2007 Great Recession increased legislators' propensity to support the American Housing Rescue and Foreclosure Prevention Act by 12.6 percentage points (Mian et al., 2010).

Origins Act. My interpretation of these patterns is that immigration generated political opposition among natives. In what follows, I discuss a number of alternative explanations, and argue that none of them can be entirely reconciled with my results.

First, at that time, after five years immigrants could apply for citizenship, becoming eligible to vote (Shertzer, 2016). If immigrants had different preferences relative to natives, changes in public spending and in tax rates may have resulted from the direct effect of immigrants' preferences rather than from natives' reactions. This idea, however, is inconsistent with electoral results presented in Sections 5.2 and 5.3, and with the historical literature documenting that, after 1910, the political involvement of foreign born fell steadily (Kleppner, 1982).

Second, it is possible that immigrants increased natives' income; if the (native) median voter became richer, demand for redistribution might have declined for reasons unrelated to political discontent (e.g. Meltzer and Richard, 1981). Note that both the election of more conservative Republicans and stronger support for anti-immigration legislation might be consistent with this alternative interpretation. Not only conservative Republicans were less likely to favor redistribution (relative to Democrats), but also, restricting the inflow of (poorer) immigrants may have been one way to limit redistribution. However, in contrast with this idea, Section 7 documents that, while Protestant and non-Protestant immigrants had similar effects on natives' employment and on economic activity, they triggered very different political reactions. Only Catholics and Jews – who were considered culturally further from natives relative to Protestants immigrants (Higham, 1955) – induced cities to limit redistribution, favored the election of more conservative legislators, and increased support for the 1924 National Origins Act.

A third possibility is that the political effects of immigration estimated above did indeed reflect backlash, but that this came from previously arrived immigrants rather than from natives. For instance, more established immigrants from Northern and Western Europe might have displayed a "cultural distaste" for more recent ones originating from Eastern and Southern Europe. Similarly, past immigrants might have been harmed economically due to labor market competition brought about by new migrants (Goldin, 1994). While this is a plausible interpretation, previous immigrants could have been pivotal in the political arena only if their preferences were aligned with those of natives. Said differently, previously arrived immigrants could have significantly influenced local politics only by forming a coalition with (at least some) natives.

6 The Economic Effects of Immigration

This section first documents that immigrants increased natives' employment and fostered industrial production (Section 6.1). Next, it shows that, while immigration had no positive effect for workers closely substitutable for immigrants, it did not lower either employment or wages even for natives working in the most exposed sector to immigration, i.e. manufacturing (Section 6.2). It concludes with a discussion on the relationship between the economic and the political effects of immigration, arguing that natives' backlash was not predominantly driven by standard economic forces (Section 6.3).

6.1 Natives' Employment and Economic Activity

Perhaps the most obvious cause for natives' backlash is that immigrants increased labor market competition, depressing wages and raising unemployment among natives. To investigate this idea, Table 5 studies the economic effects of immigration. Restricting the sample to native men of working age, I focus on the employment to population ratio (column 1) and on log occupational scores (column 2).[38]

Starting from employment, both OLS (Panel A) and 2SLS (Panel B) coefficients are positive, statistically significant, and very close to each other. The point estimate in column 1 implies that a 5 percentage points increase in immigration raised natives' employment probability by 1.4 percentage points, or by 1.6% relative to the 1910 mean. Figure A9 displays the residual scatterplot for the reduced form estimates of the 2SLS specification and visually confirms the pattern emerging from column 1. Next, column 2 documents that immigration had a strong, positive effect on natives' occupational scores.[39] Since occupational scores measure cross-occupational changes in earnings, this suggests that employment gains for natives were accompanied by occupational and skill upgrading.

These findings are in contrast with some results from the contemporary immigration literature such as Borjas (2003), Borjas and Katz (2007), and Dustmann et al. (2017) among others, who find a negative and sizeable effect of immigration on natives' labor market outcomes. They are also somewhat different from those of a number of contemporaneous cross-city studies that estimate a zero effect of immigration on natives' wages (e.g. Card, 2001, 2005). One reason for the difference between my findings and those from the more

[38] In my baseline specification, I consider men in the age range 15 to 65, but results are robust to the use of different age thresholds (see also Carlana and Tabellini, 2018). As commonly done in the literature, the employment to population ratio is constructed as the number of native men (between 15 and 65) employed, over the total number of native men in the same age range.

[39] Differently from column 1, 2SLS estimates in column 2 are an order of magnitude larger than OLS. One possible interpretation is that, even though immigrants did not systematically select cities with stronger labor demand, they might have nonetheless moved to places with fewer opportunities for skill upgrading due to congestion costs (see also Sequeira et al., 2019).

recent period is that, at the beginning of the twentieth century, the US economy had high potential for growth. Especially in manufacturing, labor scarcity was a recurrent problem (see the discussion in Section 2.2). In this context, by increasing the supply of cheap labor, immigration might have relaxed firms' (labor) constraints, allowing them to expand. Because of complementarity between immigrants and natives (Peri and Sparber, 2009), as firms grew, they also created more (relatively high skilled) jobs for natives.

To test these ideas, columns 3 and 4 of Table 5 investigate the impact of immigration on (the log of) value added per establishment and (the log of) establishment size. 2SLS estimates are positive, statistically significant, and economically large. They imply that a 5 percentage points increase in immigration raised industrial production and establishment size by approximately 10%. Table A7 documents that similar results hold when using alternative measures of industrialization (columns 1 to 3), and that immigration had a large effect also on capital utilization and on firms' productivity (columns 4 and 5).[40]

When comparing the magnitudes in column 4 with those in column 1 of Table 5, it immediately appears that the growth in natives' employment is an order of magnitude smaller than that in establishment size. There are two complementary explanations for this. First, firms grew not only by hiring natives, but also, and importantly, by absorbing the immigration shock (Table A8, column 6). Second, establishment size increased because of firm consolidation, as immigration lowered the number of establishments (Table A7, column 6).

I refer the interested reader to online appendix E, where I present several additional results, including: *i*) a detailed analysis of immigrants-natives complementarities; *ii*) the margins (previously unemployed natives; increase in labor force participation of young natives; internal migration) through which natives' employment gains might have occurred; *iii*) the effects of immigration on employment of previous immigrants; *iv*) additional effects of immigration on firm productivity; and *v*) heterogeneous effects depending on city characteristics.[41]

6.2 Heterogeneous Effects and Manufacturing Wages

Even if immigrants had, on average, positive economic effects, did they make any group of natives strictly worse off? Answering this question is important when interpreting the political results presented in Section 5. For instance, if immigrants increased labor market competition for low-skilled natives employed in manufacturing (the most exposed sector to

[40] Consistent with the literature, I proxy for capital utilitazion using the log of horsepower (results are robust to using the log of horsepower per capita or per establishment). To estimate the effects of immigration on productivity, I assumed a Cobb-Douglas production function with two factors of production, capital and (homogeneous) labor.

[41] Section 8 reports a summary of the robustness checks, which are described in detail in online appendix D.

immigration), anti-immigration sentiments might reflect discontent arising from economic losers, who pushed for political and economic protection.

Table A8 shows that, consistent with economic theory, immigration did not increase employment for natives who were close substitutes for immigrants, like illiterate native whites (column 3) or African Americans (column 4). Similarly, immigration had no effect for natives employed as manufacturing laborers (column 5) – one of the most exposed occupations to immigrants' competition.[42] However, even in highly exposed occupations and for groups that were close substitutes for immigrants, immigration did not significantly affect employment. As noted above, one explanation for this pattern is that manufacturing was able to expand, in turn absorbing the immigration-induced supply shock. Indeed, total employment in manufacturing increased almost one for one with immigration (Table A8, column 6).

Even if immigration had a positive effect on natives' employment, and no negative effect even for natives working in highly exposed sectors, it is nonetheless possible that it lowered wages at least for some workers. Unfortunately, the US Census of Population did not collect income or wage data prior to 1940, and so, this issue cannot be directly addressed using census data. While occupational scores can be used to proxy for natives' income, they do not capture within occupation changes in earnings. To overcome this limitation, in column 7 of Table A8, I estimate the effects of immigration on (log) average manufacturing wages, digitized from the Census of Manufactures. The coefficient is negative, but not statistically significant, with very large standard errors. Moreover, the implied magnitude is quite small: a 5 percentage points increase in the fraction of immigrants lowered wages in manufacturing by less than 1%.[43]

Summing up, the evidence presented in this section is inconsistent with the idea that immigration lowered wages or reduced employment even for workers in the most exposed sector, or whose skills were very similar to those of immigrants. While I do not observe the entire distribution of wages, since data from the Census of Manufactures do not distinguish between immigrants and natives, since new immigrants were closer substitutes for previously arrived immigrants than for natives, and because manufacturing was the most exposed sector to immigrants' competition, one can confidently interpret these results as a lower bound for the impact of immigration on natives' earnings.

[42] In 1910, recent immigrants were twice as likely as natives to be employed in unskilled occupations. Similarly, while around 21% of natives were working in manufacturing, almost 45% of immigrants were employed in this sector.

[43] This finding is somewhat in contrast with Goldin (1994), possibly because she focused on a different sample of cities and used a different empirical strategy.

6.3 Did Natives' Backlash Have Economic Roots?

The analysis presented above weighs against the idea that political discontent was driven by purely economic factors. Not only immigration had a positive effect on natives' employment and occupational standing, but also, it did not reduce either employment or wages even for workers that were highly exposed to immigrants' competition. Yet, there exist a few other explanations for natives' political reactions that rest on economic grounds.

First, although immigration did not create clear economic losers among natives, it might have increased inequality. If individuals assess their life satisfaction relative to other (socio-economic) groups in the society, natives' backlash can be explained by a "keep up with the Jones" type of argument. Lack of survey data for this historical period prevents me from directly addressing this issue. However, in the next section, I show that this interpretation is unlikely to hold: immigrants from different religious groups had very similar economic effects – likely moving the income distribution in the same way – and yet triggered very different political reactions.

A second, related possibility is that immigrants were able to upgrade faster than natives, in turn triggering natives' grievances, who may have felt threatened by "socially inferior" groups. Abramitzky et al. (2012, 2014) show that, contrary to the common wisdom, immigrants did not experience occupational mobility at a faster rate than natives, and that it often took multiple generations for immigrants' earnings to converge towards those of natives. Consistent with their findings, online appendix E3 documents that, if anything, immigration had a negative (although not statistically significant) effect on employment of previously arrived immigrants – an effect that disappears for immigrants who had spent more than 20 years in the US, and who were arguably more similar to natives (and thus less likely to be perfect substitutes for new migrants).

Third, it is possible that the positive economic effects of immigration did not reflect the creation of new jobs, but were instead simply due to the relocation of economic activity from one city to the others. In online appendix D2, I show that the economic effects of immigration are unchanged when aggregating the unit of analysis to the MSA, suggesting that any negative spillover had to operate outside the local labor market (e.g. at the state level). Exploring this possibility, online appendix D2 also tests whether immigration to other cities in the same state had any effect on a given city. Consistent with Sequeira et al. (2019), I find little evidence of negative spillovers from immigration to other cities, weighting against the possibility that political discontent was due to this channel.

Fourth, by increasing housing demand, immigration might have raised house prices and rents. While this would have benefitted homeowners, it would have nonetheless increased the

cost of living for natives who did not own a house.[44] These dynamics, rather than economic competition in the labor markets, may explain natives' hostile reactions. I provide two pieces of evidence against this possibility. First, as noted in Section 5.1, immigration did not have any significant effect on property values (Table A3). Second, online appendix E5 documents that immigration was not correlated with rents paid by natives. One possible explanation for this pattern is that immigrants represented a production amenity, but were perceived by natives as a consumption disamenity.[45]

Finally, immigration might have triggered natives' backlash by generating congestion costs in already crowded cities. Counter to this explanation, however, in online appendix E6, I show that political reactions were not systematically more pronounced in larger or in more densely populated cities.

7 Backlash, Cultural Distance, and Ethnic Diversity

If immigration was economically beneficial and did not reduce employment even for natives in highly exposed occupations, why did backlash emerge? In this section, exploiting variation in the "mix" of immigrants received by US cities over time, I show that cultural differences between immigrants and natives were responsible, at least in part, for natives' anti-immigration reactions.

7.1 Cultural Distance: Religious Affiliation

The historical evidence reviewed in Section 2.3 suggests that opposition to immigration during the Age of Mass Migration had deep cultural roots. Anti-immigration sentiments were often directed towards Jews and Catholics, whose values were perceived as a threat to the Puritan tradition prevailing in the US at that time (Higham, 1955; Spiro, 2009). One of the best examples for the strength of these sentiments is the revival of the Ku Klux Klan in the 1920s, which openly embraced an anti-Catholic and anti-Semitic ideology. Similarly, immigrants from non Anglo-Saxon and non English-speaking countries were the main target of the anti-immigration rhetoric at that time (Abramitzky et al., 2018; Leonard, 2016).

Motivated by these observations, I proxy for cultural distance between natives and immigrants using, respectively, religion and linguistic distance from English. Starting from religion, I estimate

$$y_{cst} = \gamma_c + \delta_{st} + \beta_1 Imm_{cst}^{Non-Prot} + \beta_2 Imm_{cst}^{Prot} + u_{cst} \quad (3)$$

[44] In 1910, only 40% of natives living in the cities in my sample were homeowners.

[45] This idea is consistent with findings of several papers for both Europe and the US today (e.g. Card et al., 2012; Saiz and Wachter, 2011).

where $Imm_{cst}^{Non-Prot}$ (resp. Imm_{cst}^{Prot}) is the fraction of Jews or Catholics (resp. Protestant) immigrants. In practice, equation (3) is estimated using two separate instruments, one for each religious group, constructed by summing predicted immigration from each sending region (see (2) in Section 4.2) across non-Protestant and Protestant countries respectively. I standardize the fraction of immigrants from non-Protestant and Protestant countries, so that coefficients β_1 and β_2 in (3) can be interpreted as the effect of one standard deviation increase in immigration from either religious group.

As a preliminary step, online appendix E7 shows that the economic effects of immigration were very similar across religious groups (see Figures E6 and E7). This is important because it rules out the possibility that any difference in natives' political reactions can be explained by differences in the economic effects of Protestant and non-Protestant immigrants. Next, in Table 6, I estimate (3), focusing on a set of political outcomes. Immigration had a negative and significant effect on taxes and spending only when immigrants came from non-Protestant countries (columns 1 to 4), whereas the coefficient on Protestant immigrants is quantitatively very small (or even positive, as in columns 1 and 2) and never statistically significant. Turning to electoral outcomes, even though both non-Protestant and Protestant immigrants seem to reduce the Democrat-Republican vote margin, results are statistically significant only for the former (column 5).

To more directly investigate the rise of anti-Catholic sentiments, in column 6, I study if the 1910-1930 (instrumented) change in Catholic and Protestant immigration had an effect on the percent of votes received by Alfred Smith in 1928 Presidential elections. Smith was the first Roman Catholic to run for presidency for the Democratic party, and historical accounts consider his religious affiliation one of the main reasons for his defeat (Slayton, 2001). The strong, negative association between Catholic (but not Protestant) immigrants and the percent of votes received by Smith is consistent with this view as well as with the idea that immigration triggered natives' backlash in receiving areas.

Finally, column 7 indicates that the increase in legislators' ideology was entirely due to non-Protestant immigration. Likewise, legislators' propensity to support the 1924 National Origins Act is strongly correlated with the 1910-1920 change in Catholic and Jewish immigration (column 8). Conversely, there is a negative, albeit not significant, correlation between the 1910-1920 change in the fraction of Protestant immigrants and the probability of voting in favor of the immigration restrictions. Consistent with these results, using data from local newspapers and Congressional speeches, D'Amico and Tabellini (2018) document that only Catholic and Jewish, but not Protestant, immigration increased the frequency of racially charged terms both in local media and in the Senate.

7.2 Linguistic Distance and Ethnic Diversity

As an alternative proxy for cultural differences between immigrants and natives, I rely on the measure of linguistic distance constructed by Chiswick and Miller (2005), which is described in detail in online appendix E1. First, I compute the weighted average of immigrants' linguistic distance from English, $LD_{cst} = \sum_j \left(sh^j_{cst} \cdot L^j \right)$, where sh^j_{cst} is the share of ethnic group j among the foreign born population of city c in Census year t, and L^j is the linguistic distance between country j and English. Then, I re-estimate (1) using as main regressor of interest LD_{cst}, always controlling for the (instrumented) fraction of immigrants and instrumenting the actual shares (sh^j_{cst}) with the same logic of the instrument in (2).[46] To ease the interpretation of results, presented in Table 7, I standardize LD_{cst} by subtracting its mean and dividing it by its standard deviation.

Consistent with findings in Table 6, higher linguistic distance is associated with larger reductions in taxes and public spending (columns 1 to 4). Moreover, and similarly to Table A5, the fall in spending is concentrated in education and, even though the point estimate is not statistically significant, in categories where inter-ethnic interactions are likely to be more salient (columns 5, 7, and 8). These results reject the idea that natives' reactions were primarily driven by economic considerations, since it was precisely immigration from linguistically far countries that boosted natives' occupational standing (Table E3 in online appendix E1).

Findings in Tables 6 and 7 are robust to simultaneously including a (standardized) index of average literacy among immigrants, thus reducing concerns that they might be capturing not only cultural, but also economic attributes of the foreign born (Table A9).[47] Not surprisingly, since there are now three different endogenous regressors and three instruments, the precision of the estimates deteriorates relative to Table 7. Nonetheless, only linguistic distance has a significant effect on taxes and public spending. Moreover, except for columns 7 and 8, the coefficient on linguistic distance is an order of magnitude larger (in absolute value) than that on literacy.

Differently from what one may expect, the correlation between the fraction of non-Protestant immigrants and the index of linguistic distance is as low as 0.05, suggesting that findings for linguistic diversity are unlikely to merely replicate those for religious affiliation (see also online appendix E8). Finally, online appendix E9 provides evidence that the

[46] The estimated effect of immigration is not reported to save space. However, I always report the AP F-stat associated with its first stage.

[47] The literacy index was constructed as $LIT_{cst} = \sum_j \left(sh^j_{cst} \cdot Lit^j_t \right)$, where Lit^j_t is the average literacy rate of males in working age from ethnic group j who entered the US in the previous decade. To ease the interpretation of results, I multiplied LIT_{cst} by -1, so that higher values of this index can be interpreted as lower average literacy among immigrants, and can be directly compared to LD_{cst}. The correlation between LD_{cst} and LIT_{cst} is relatively low, with a value of 0.26.

(negative) effect of immigration on redistribution was larger when ethnic diversity among foreign born was higher. These findings are consistent with the large literature showing that ethnic diversity is associated with lower public goods provision and with more limited redistribution (e.g. Alesina et al., 1999; Beach and Jones, 2017; Luttmer, 2001).

It is possible that the negative relationship between redistribution and cultural or ethnic diversity merely reflects heterogeneity in preferences, and did not have any effect on social cohesion. However, as documented in Table 6, cultural diversity not only reduced public goods provision, but also generated a number of additional effects, such as the election of more conservative legislators and higher support for anti-immigration legislation. These patterns indicate that cultural diversity brought about by immigration caused hostile political reactions among natives, likely raising social conflict in receiving cities.

8 Summary of Main Robustness Checks

Online appendix D presents several robustness checks for results presented in the main paper. First, to address concerns that 1900 immigrants' settlements might be correlated with other city-specific characteristics that had a time varying effect on economic and political conditions: *i*) I show that there is no correlation between the pre-period change in any of the outcomes of interest and the change in immigration predicted by the instrument (Table D1); *ii*) I augment the baseline specification by separately controlling for predicted industrialization, and interacting year dummies with several 1900 city characteristics (Tables D2 to D4). Second, I document that the economic effects of immigration are unchanged when aggregating the analysis to the MSA level, and that there are no spillovers from cities in the same state (Table D5).

Third, to deal with the possibility that the 1900 share of immigrants were not independent of cross-city pull factors systematically related to 1900 settlers' country of origin (see Goldsmith-Pinkham et al., 2018), I replicate results interacting year dummies with the share of immigrants from each sending country (Figures D1 and D2). Fourth, to show that city-specific, time-varying shocks did not endogenously affect immigration from specific European countries, I replicate the analysis using the WWI and quotas instruments and the weather shocks instrument constructed in online appendix B (Tables D6 and D7). Fifth, I present a number of results (Figures D3 and D4; Table D8) to show that, in this setting, the shift-share instrument is unlikely to conflate the long and the short run effects of immigration (Jaeger et al., 2018).

Sixth, I show that results are robust to: *i*) dropping potential outliers (Tables D9 to D11; Figure D5); *ii*) scaling the number of immigrants with different population measures (Table D12); *iii*) considering not only European or recently arrived immigrants (Table D13).

Finally, I estimate non-parametric regressions to test for the possibility that immigration had non-linear effects (Figures D6 to D9), check that results are robust to using a specification in logs (Table D14), and replicate the analysis allowing for different assumptions on bias due to selection on observables and unobservables (Table D15), following the procedure outlined in Oster (2017).

9 Conclusions

Today, immigration is at the forefront of the political debate, and immigrants are increasingly opposed on both economic and cultural grounds. In this paper, I exploit variation in the number of immigrants received by US cities between 1910 and 1930 to jointly study the political and economic consequences of an episode of mass immigration. Using a leave-out version of the shift-share instrument (Card, 2001), I find that immigration triggered hostile political reactions, such as limiting redistribution, electing more conservative politicians, and raising support for the introduction of immigration restrictions.

Exploring the causes of such political reactions, I provide evidence that natives' backlash was unlikely to have economic roots. Not only immigration was on average beneficial to US cities, by increasing natives' employment and occupational standing and by fostering industrial production. But also, it did not induce losses even among natives working in sectors or occupations highly exposed to immigrants' competition. Exploiting variation in immigrants' background, I document that natives' backlash was increasing in the cultural distance between immigrants and natives, suggesting that opposition to immigration, at least in this context, was more likely to arise because of cultural, rather than economic, considerations.

Findings in this paper may be specific to the conditions prevailing in US cities in the early twentieth century. However, they may still be relevant for the design of policies aimed at dealing with the economic and the political effects of immigration today. My results suggest that, when cultural differences between immigrants and natives are large, opposition to immigration can arise even if immigrants are on average economically beneficial and do not create economic losers among natives. Thus, favoring the cultural assimilation of immigrants and reducing the (actual or perceived) distance between immigrants and natives may be at least as important as addressing the potential economic effects of immigration.

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Table 1. Summary Statistics

VARIABLES	*Mean*	*Median*	*St. Dev.*	*Min*	*Max*	*Obs.*
			Panel A. City Demographics			
Fr. all immigrants	0.152	0.149	0.097	0.007	0.518	540
Fr. recent immigrants	0.042	0.026	0.044	0.001	0.343	540
Recent immigrants over 1900 population	0.074	0.048	0.078	0.002	0.678	540
City population (1,000s)	190.1	76.05	510.4	30.20	6,930	540
			Panel B. Economic Outcomes			
Employed	0.858	0.889	0.071	0.648	0.952	538
Log occupational scores	3.263	3.265	0.047	3.080	3.427	538
Value added per establishment	87.66	65.92	74.47	7.945	556.3	525
Establishment size	52.86	43.09	37.98	5.465	229.9	525
			Panel C. Political Outcomes			
Tax rate per 1,000$ of assessed valuation	29.42	25.78	16.48	6.450	114.3	539
Expenditures per capita	14.57	12.89	7.336	3.443	49.99	540
Democrats' vote share	0.482	0.465	0.189	0.103	0.967	378
DW Nominate Score	0.178	0.334	0.338	-0.578	0.991	470

Note: the sample includes a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930. *Employed* is the employment to population ratio for native men in the age range (15-65). *Fr. all immigrants* (resp. *Fr. recent immigrants*) is the total number of European immigrants (resp. the number of European immigrants arrived in the last 10 years) divided by city population.

Table 2. First Stage

	Dep. Variable: *Fraction of Immigrants*					
	(1)	(2)	(3)	(4)	(5)	(6)
Z	0.840*** (0.056)	0.968*** (0.064)	0.999*** (0.059)	0.948*** (0.104)	0.893*** (0.091)	0.900*** (0.081)
1900 population		X				
Predicted population			X			
MSA analysis				X		
Year by 1900 Log					City and imm pop	Value added manuf.
F-stat	225.1	226.7	288.3	82.65	96.48	124.8
Cities	180	180	180	127	180	176
Observations	540	540	540	381	540	528

Note: the sample includes a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930. In Col 1 the actual number of immigrants is scaled by actual population, and the instrument is the leave-out version of the shift-share IV in equation (2) (Section 4.2). Cols 2 and 3 replicate Col 1 by scaling the actual and predicted number of immigrants by, respectively, 1900 and predicted population. From Col 3 onwards, Table 2 presents results from specifications where both the predicted and the actual number of immigrants are scaled by predicted population. Col 4 replicates the analysis aggregating the unit of analysis at the MSA level. Cols 5 and 6 include the interaction between year dummies and, respectively, the (log of) 1900 city and immigrants population, and the (log of) 1904 value added by manufacture per establishment. F-stat refers to the K-P F-stat for weak instrument. All regressions partial out city and state by year fixed effects. Robust standard errors, clustered at the MSA level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Table 3. The Political Effects of Immigration

Dependent Variable:	(1) Public Spending per Capita	(2) Property tax rate	(3) Democrats' Vote Share	(4) DW Nominate Score
Panel A: OLS				
Fr. Immigrants	-5.958*	-28.49***	-0.528***	0.745
	(3.589)	(9.754)	(0.106)	(0.468)
Panel B: 2SLS				
Fr. Immigrants	-8.699*	-29.44*	-0.404***	1.658**
	(4.453)	(16.95)	(0.141)	(0.808)
F-stat	288.3	292.7	83.14	23.11
Mean of dep var.	12.16	19.75	0.490	0.165
Observations	540	539	378	460

Note: columns 1 and 2 present results for a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930. The dependent variable is public spending per capita in column 1 and property tax rate for $1,000 of assessed valuation in column 2. In column 3, the dependent variable is the Democrats vote share in Presidential elections, and the sample includes the balanced panel of the 126 metropolitan statistical areas (MSAs) containing at least one of the 180 cities in my sample. In column 4, the dependent variable is the first dimension of DW Nominate scores for members of the House of Representatives, for the panel of city-to-congressional district units for Congress 61, 66, and 71, for the 180 cities considered in my sample. Panels A and B present OLS and 2SLS results for the baseline specification (equation (1)). *Fr. Immigrants* is the fraction of immigrants arrived in the previous decade over predicted city population, and is instrumented using the baseline version of the instrument constructed in Section 4.2 (see (2) in the main text). F-stat refers to the K-P F-stat for weak instrument. Regressions in columns 1 and 2 include city and state by year fixed effects, while regressions in columns 3 and 4 include MSA (column 3) or congressional district to city (column 4) and state by year fixed effects. Robust standard errors, clustered at the MSA (for columns 1 to 3) or congressional district to city (column 4) level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Table 4. Congressmen Ideology and the National Origins Act of 1924

Dep. Variable:	*DW Nominate Scores*		*Pr. that Winner has Given Political Orientation*				*1[Restrict Immigration]*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS								
Fr. Immigrants	0.745	0.603	-0.045	-0.804	-0.290	1.238	2.121*	2.024
	(0.514)	(0.521)	(0.317)	(0.711)	(0.991)	(1.135)	(1.189)	(1.362)
Panel B: 2SLS								
Fr. Immigrants	1.658**	1.575*	-0.601	-1.655	-0.198	2.592*	3.784**	3.365*
	(0.808)	(0.841)	(0.817)	(1.039)	(1.717)	(1.354)	(1.569)	(1.770)
F-stat	23.11	19.56	23.11	23.11	23.11	23.11	88.05	39.34
Mean dep var	0.165	0.150	0.167	0.161	0.359	0.314	0.676	0.676
Observations	470	437	470	470	470	470	155	155
Balanced Panel		X						
Political Orientation			Liberal Democrat	Moderate Democrat	Moderate Republican	Conservative Republican		

Note: Cols 1 to 6 report results for the panel of city-to-congressional district units for Congress 61, 66, and 71, for the 180 cities considered in my sample (see Table A2). Because of redistricting between the 61st and the 66th Congress, it was not possible to construct a balanced panel including all city-congressional district cells in my sample. For this reason, Col 2 restricts the attention to the balanced panel of cities (to congressional districts) that were not affected by redistricting. The unbalanced (resp. balanced) panel is composed of 157 (resp. 146) units of observations. Cols 7 and 8 present results from a cross-sectional regression for the 155 combinations of cities to congressional districts in Congress 68, for the 180 cities considered in my sample. Panels A and B report, respectively, OLS and 2SLS results. The dependent variable is the first dimension of the DW Nominate score in Cols 1 and 2, an indicator for electing a politician with a given political orientation (see bottom of the Table) in Cols 3 to 6, and an indicator for voting in favor of the 1924 National Origins Act in the House of Representatives. *Fr. Immigrants* is the fraction of immigrants arrived in the previous decade over predicted city population, and is instrumented using the baseline version of the instrument constructed in Section 4.2 (see (2) in the main text). F-stat refers to the K-P F-stat for weak instrument. Cols 1 to 6 include city by congressional district and state by year fixed effects. Cols 7 and 8 control for state fixed effects. Col 8 also includes the 1900 log of black, immigrants, and total population, as well as the share of Democratic legislators representing the city (to congressional district) in the 68th Congress. Robust standard errors, clustered at the congressional district level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Table 5. The Economic Effects of Immigration

	Natives' Outcomes		Economic Activity	
Dependent Variable:	(1) Employment to Population Ratio	(2) Log Occupational Scores	(3) Log Value Added per Establishment	(4) Log Establishment Size
Panel A: OLS				
Fr. Immigrants	0.287***	0.000	2.057***	2.195***
	(0.040)	(0.049)	(0.647)	(0.565)
Panel B: 2SLS				
Fr. Immigrants	0.299***	0.097***	2.889***	2.532***
	(0.064)	(0.036)	(0.954)	(0.815)
F-stat	251.3	251.3	270.5	270.5
Mean of dep var.	0.911	3.245	3.820	3.539
Observations	538	538	525	525

Note: this table presents results for a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930, restricting the attention to native men in the age range 15 to 65 who are not enrolled in schools (columns 1 and 2). Columns 3 and 4 further restrict the sample to city-year observations for which data were reported in the Census of Manufacture between 1909 and 1929. The dependent variable is natives' employment to population ratio in column 1, and natives' log occupational scores in column 2. Occupational scores are computed by IPUMS, and assign to an individual the median income of his job category in 1950. The dependent variable is the log of value added per establishment in column 3, and the log of establishment size in column 4. Panels A and B present OLS and 2SLS results for the baseline specification (equation (1)). *Fr. Immigrants* is the fraction of immigrants arrived in the previous decade over predicted city population, and is instrumented using the baseline version of the instrument constructed in Section 4.2 (see (2) in the main text). F-stat refers to the K-P F-stat for weak instrument. All regressions include city and state by year fixed effects. Robust standard errors, clustered at the MSA level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Table 6. Immigration and Religion

Dep. Var.	(1) Total tax revenues PC	(2) Property tax revenues PC	(3) Property tax rate	(4) Public spending PC	(5) Dem-Rep. margin	(6) Smith's pct. votes	(7) DW Nominate Scores	(8) 1[Restrict Immigration]
Panel A: OLS								
Fr. Non-Prot.	-0.519	-0.449	-1.235***	-0.320*	-0.039***	-0.042***	0.035	0.099***
	(0.329)	(0.278)	(0.477)	(0.180)	(0.007)	(0.007)	(0.025)	(0.037)
Fr. Prot.	0.406	0.277	-0.077	0.154	0.023	0.025*	-0.009	-0.057
	(0.339)	(0.326)	(0.722)	(0.313)	(0.016)	(0.015)	(0.017)	(0.045)
Panel B: 2SLS								
Fr. Non-Prot.	-0.515*	-0.483*	-1.219*	-0.366**	-0.017**	-0.050***	0.063**	0.145***
	(0.306)	(0.284)	(0.649)	(0.183)	(0.009)	(0.008)	(0.030)	(0.053)
Fr. Prot.	0.193	0.067	-0.109	-0.007	-0.010	0.036	0.006	-0.066
	(0.399)	(0.351)	(1.120)	(0.250)	(0.013)	(0.024)	(0.030)	(0.078)
KP F-stat	26.37	26.37	26.23	26.37	37.89	35.87	32.16	23.74
F-stat (Non-Prot)	115.9	115.9	118.9	115.9	50.64	38.60	85.91	69.49
F-stat (Prot)	27.53	27.53	27.39	27.53	38.95	36.58	32.27	21.68
Mean of dep var	12.76	12.10	19.75	12.16	0.180	0.398	0.165	0.676
Observations	540	540	539	540	378	126	460	155

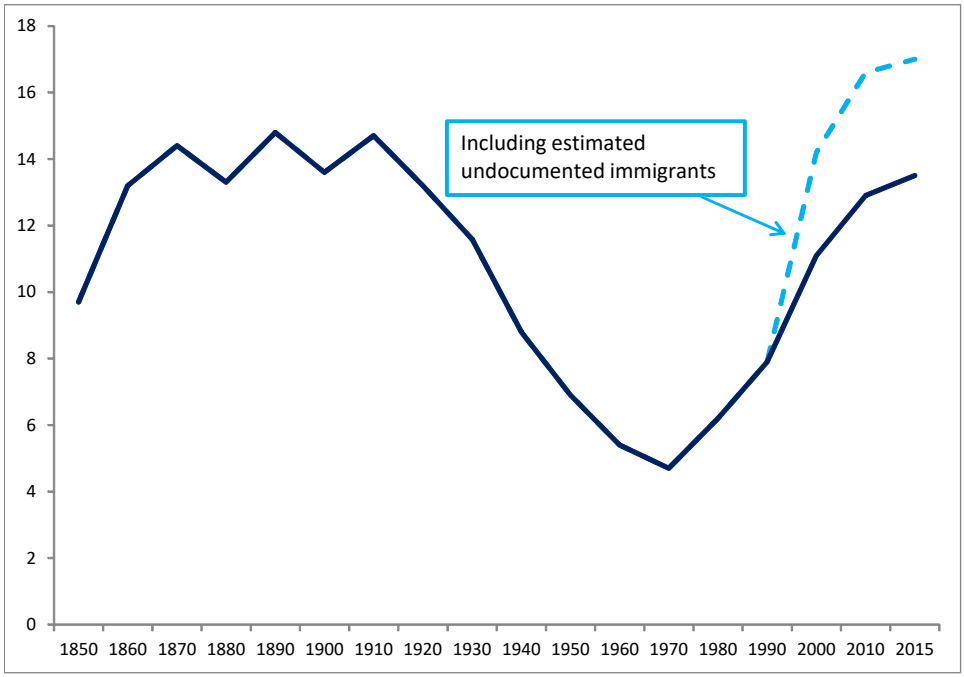
Note: this table presents results for a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930. The analysis is conducted at the MSA rather than at the city level, fixing boundaries using 1940 definitions in Cols 5 and 6, and at the city to congressional district level in Cols 7 and 8. Panels A and B report, respectively, OLS and 2SLS results. The dependent variable is displayed at the top of each column. *1[Restrict Immigration]* (Col 8) is an indicator for voting in favor of the 1924 National Origins Act in the House of Representatives. In Cols 1 to 5 and in Col 7, *Fr. Non-Prot.* (resp. *Prot.*) refers to the fraction of immigrants arrived in the previous decade from non-Protestant (resp. Protestant) countries, over predicted city population, for each of the three decades, 1910, 1920, and 1930. In Cols 6 and 8, *Fr. Non-Prot.* (resp. *Prot.*) is the 1910 to 1930 (1910 to 1920) change in the fraction of recent immigrants from non-Protestant (resp. Protestant) countries over predicted city population. Each endogenous regressor is instrumented with the predicted fraction immigrants (see (2) in Section 4.2), obtained by summing (predicted) immigration across non-Protestant and Protestant countries. To ease the interpretation of results, both actual and predicted immigration is standardized by subtracting the mean and dividing through the standard deviation. F-stat (Non-Prot) and F-stat (Prot) refer to the partial F-stats for joint significance of the instruments in the two separate first-stage regressions. KP F-stat is the Kleibergen-Paap F stat for joint significance of instruments. Cols 1 to 4 (resp. 5) include city (resp. MSA) and state by year fixed effects, while Col 7 includes congressional district by city and state by year fixed effects. Cols 6 and 8 present results from a cross-sectional regression and control for state dummies. Robust standard errors, clustered at the MSA level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Table 7. Linguistic Distance and Redistribution

Dep. Var.	(1) Total tax revenues PC	(2) Property tax revenues PC	(3) Property tax rate	(4) Public spending PC	(5) Education	(6) Police	(7) Charities and Hospitals	(8) Sanitation
Panel A: OLS								
Ling. Distance	-0.361*	-0.346	-1.485*	-0.213	-0.050	-0.032	-0.010	-0.045
	(0.205)	(0.212)	(0.840)	(0.160)	(0.060)	(0.021)	(0.039)	(0.029)
Panel B: 2SLS								
Ling. Distance	-0.875*	-0.809*	-2.308	-0.519*	-0.199*	-0.013	-0.119	-0.053
	(0.468)	(0.458)	(1.598)	(0.301)	(0.117)	(0.042)	(0.084)	(0.052)
KP F-stat	21.02	21.02	21.47	21.02	21.14	21.02	16.31	21.02
F-stat (Imm.)	123.1	123.1	124.7	123.1	106.9	123.1	101.6	123.1
F-stat (Ling.)	50.38	50.38	53.48	50.38	48.05	50.38	34.06	50.38
Mean of dep var	12.76	12.10	19.75	12.16	4.250	1.338	0.635	1.129
Observations	540	540	539	540	534	540	516	540

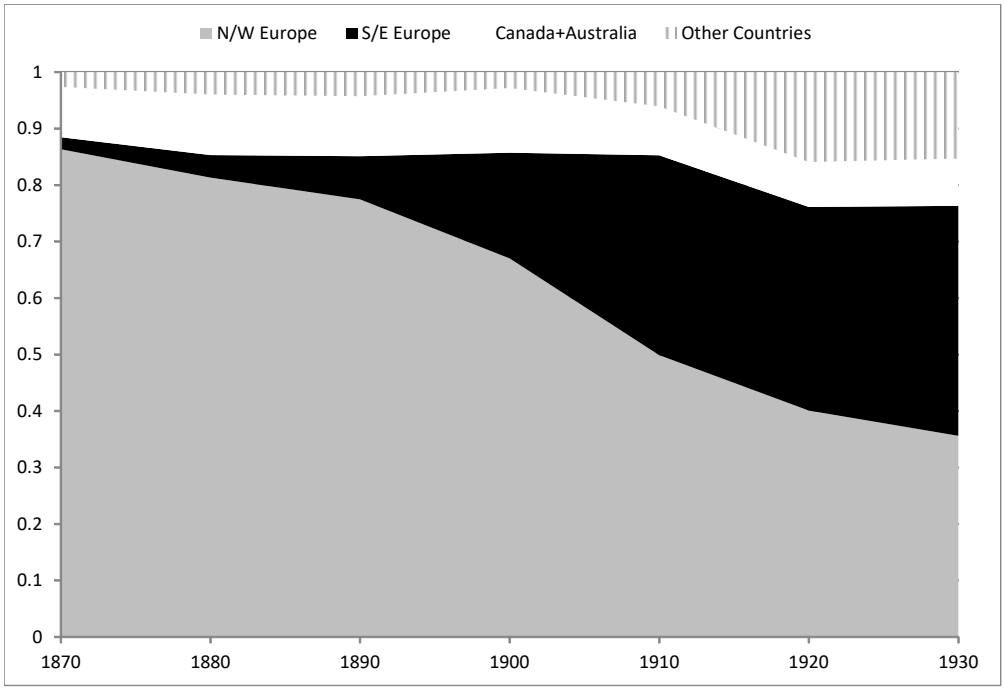
Note: this Table presents results for a balanced panel of the 180 US cities with at least 30,000 residents in each Census year 1910, 1920, and 1930. Panels A and B report, respectively, OLS and 2SLS results. The dependent variable is displayed at the top of each column. In Cols 5 to 8, the dependent variable is spending per capita on the category listed at the top of the column. The main regressor of interest is the (standardized) weighted average linguistic distance constructed in Section 7.2, instrumented using predicted shares of immigrants from each sending region obtained from (2) in Section 4.2. F-stat is the Kleibergen-Paap F stat for joint significance of instruments. F-stat (Imm.) and F-stat (Ling.) refer to the partial F-stats for joint significance of the instruments in the two separate first-stages. All regressions include the main effect of immigration (instrumented with the baseline shift-share instrument from (2)), and control for city and state by year fixed effects. Robust standard errors, clustered at the MSA level, in parenthesis. *** p<0.01; ** p<0.05; * p<0.1.

Figure 1. Immigrants as a Percent of US Population



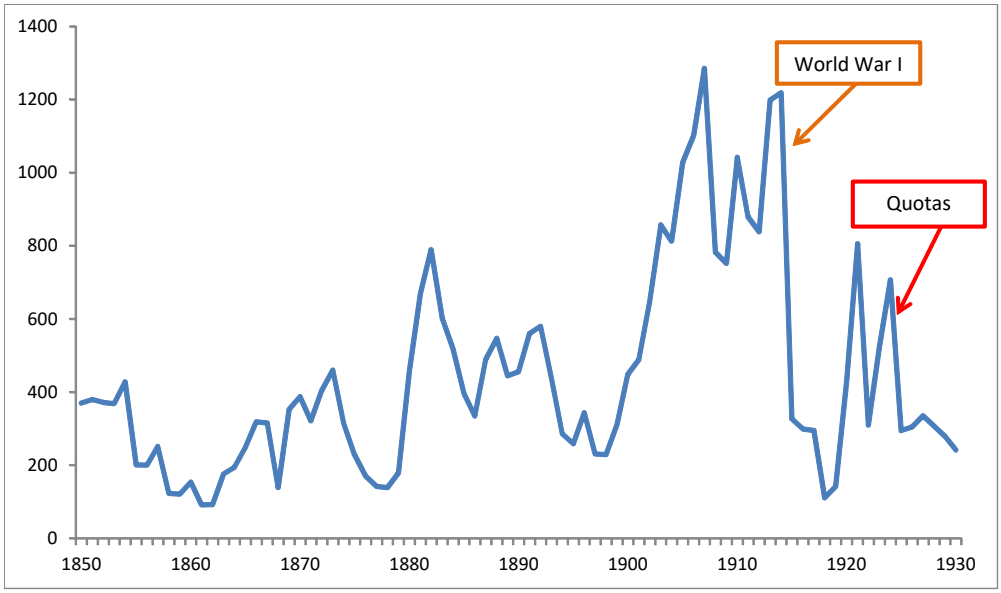
Note: The solid line shows the number of legal immigrants as a percent of US population. The dashed line includes also the estimated number of illegal immigrants, available from 2000 onwards. Source: the number of legal immigrants comes from the Migration Policy Institute, while the number of illegal immigrants was taken from the Pew Research Center tabulations

Figure 2. Share of Foreign Born in the United States, by Region



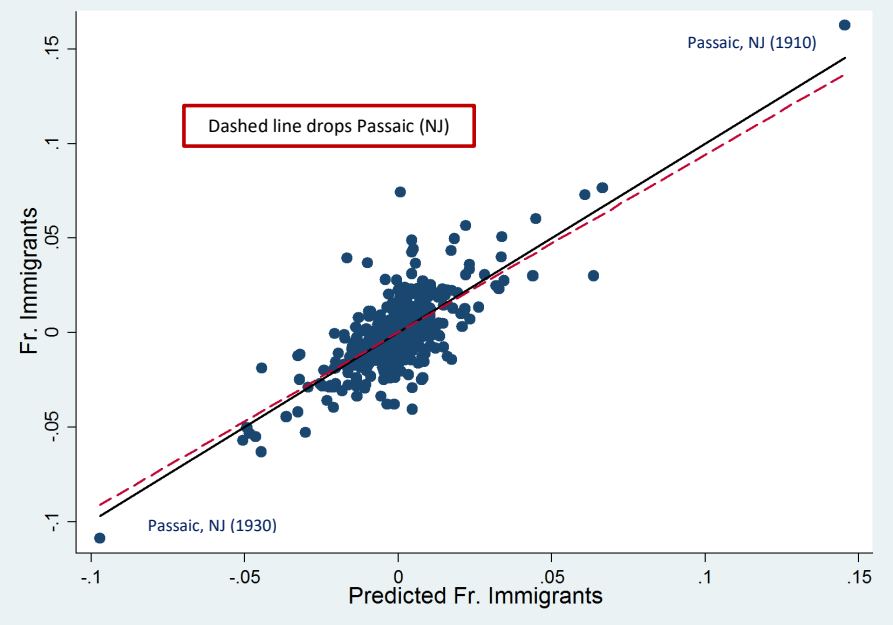
Note: Share of immigrant stock living in the United States, by sending region and by decade. Source: Author's calculations from IPUMS sample of US Census (Ruggles et al., 2015).

Figure 3. Total Number of Immigrants (in Thousands)



Note: Annual inflow of immigrants to the United States (1850-1930). Source: Migration Policy Institute.

Figure 4. First Stage: Actual vs Predicted Immigration



Note: the y-axis (resp. x-axis) reports the actual (resp. predicted) number of immigrants over predicted city population in each of the three Census years, 1910, 1920, and 1930. Each point in the scatter diagram represents the residual change in a city's actual and predicted fraction of immigrants after partialling out city and year by state fixed effects. The predicted number of immigrants is constructed as discussed in Section 4.2 in the text (see (2)). Predicted city population is obtained by multiplying 1900 city population with average urban growth, excluding that of the Census division where a city is located. The solid line shows the regression coefficient for the full sample (coefficient=0.999, standard error=0.059). The dotted (red) line shows the regression coefficient obtained when dropping the city of Passaic, NJ (coefficient=0.940, standard error=0.068).