MIGRANTS AND THE MAKING OF AMERICA:
THE SHORT- AND LONG-RUN EFFECTS OF
IMMIGRATION DURING THE AGE OF MASS MIGRATION

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Migrants and the Making of America: The Short- and Long-Run Effects of Immigration during the Age of Mass Migration
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ABSTRACT

We study the effects of European immigration to the United States during the Age of Mass Migration (1850-1920) on economic prosperity today. We exploit variation in the extent of immigration across counties arising from the interaction of fluctuations in aggregate immigrant flows and the gradual expansion of the railway network across the United States. We find that locations with more historical immigration today have higher incomes, less poverty, less unemployment, higher rates of urbanization, and greater educational attainment. The long-run effects appear to arise from the persistence of sizeable short-run benefits, including greater industrialization, increased agricultural productivity, and more innovation.

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A data appendix is available at http://www.nber.org/data-appendix/w23289

1. Introduction

An important issue within current American political discourse is the effect that immigrants have on the communities into which they settle. While this topic has received significant attention, the focus has generally been on the short-term effects of immigrants.[1] We know much less about their long-run effects. This is particularly important because the short-run and long-run effects could be very different, in both magnitude and in sign.

We contribute to an improved understanding of the long-run effects of immigration by taking a historical perspective. In particular, we examine migration into the United States during America's Age of Mass Migration (from 1850–1920) and estimate the causal effect of immigrants on economic and social outcomes approximately 100 years later. This period of immigration is notable for many reasons. First, this was the period in U.S. history with the highest levels of immigration. Second, the immigrants that arrived during this time were different from previous waves of immigrants. While earlier immigrants were primarily from western Europe, the new wave also included large numbers of immigrants from southern, northern, and eastern Europe who spoke different languages and had different religious practices (Hatton and Williamson, 2005, p. 51, Daniels, 2002, pp. 121–137, Abramitzky and Boustan, 2015).

Empirically studying the long-run effects of immigration is challenging. A natural strategy is to examine the relationship between historical immigration and current economic outcomes across counties in the United States. However, there are important shortcomings of such an exercise. There may be persistent omitted factors that affected immigration decisions that could independently influence the outcomes of interest. It is also possible that immigrants were attracted to locations with more growth potential. Alternatively, they may have only been able to settle in more marginal locations, where land and rents were cheaper and future economic growth was lower. These concerns would cause the OLS estimates to be biased.

An important contribution of our analysis is the development of an identification strategy that overcomes this problem. We propose an instrumental variables (IV) strategy that exploits two facts about immigration during this period. The first is that after arriving into the United States, immigrants tended to use the newly constructed railway to travel inland to their eventual place

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[1] See Kerr and Kerr (2016) for evidence of the effects of immigrants on entrepreneurial activity; Peri (2012) for evidence of the effects of immigrants on productivity; Peri and Sparber (2009) for evidence of effects on occupational specialization; Hunt and Gauthier-Loiselle (2010) for evidence of effects on innovation; Card (2012) for evidence of effects on average wages; and Card (2009) for evidence on wage inequality.

of residence (Faulkner, 1960, Foerster, 1969). Therefore, a county's connection to the railway network affected the number of immigrants that settled in the county. The second fact is that the aggregate inflow of immigrants coming to the United States during this period fluctuated greatly from decade to decade.

Holding constant the total length of time a county was connected to the railway network (in our analysis we always condition on this), if a county was connected to the railway network during periods of high aggregate immigration to the United States, then the county will tend to have had more immigrant settlement. During this time, once a county became connected to the railway network it almost always stayed connected. Therefore, asking whether a county was connected during periods with relatively higher or lower aggregate immigrant inflows is equivalent to asking whether a county became connected to the railway network just prior to a decade with particularly high aggregate immigration or just prior to a decade with particularly low aggregate immigration. All else equal, the average inflow of immigrants during the time in which the county was connected to the railway will be greater in the former case than in the latter case.

The benefit of combining the two sources of variation – the timing of the construction of the railway and the timing of immigration booms – is that the interaction between the two generates variation that is unlikely to affect our contemporary outcomes of interest through other channels. Whether a county became connected to the railway just prior to an immigration boom rather than an immigration lull is unlikely to have a direct effect on our current outcomes of interest other than through historical immigration to the county.

To implement our IV strategy, we proceed in three steps. We begin with a "zero-stage" regression where we examine a panel of counties every census decade from 1850 to 1920, and estimate the determinants of the share of the population that was foreign-born. The specification includes county fixed effects and time-period fixed effects. It also includes an interaction between the aggregate inflow of European immigrants into the United States (normalized by total population) during the prior ten years and an indicator variable that equals one if the county was connected to the railway network at the beginning of the ten-year period. This interaction captures the differential effect of connection to the railway network on immigrant settlement in decades with high aggregate immigrant inflows relative to decades with low aggregate immigrant inflows. This interaction is the variable (and variation) that is the basis of our instrument.

In the zero-stage panel regression, we also control for the railway connectivity indicator and the aggregate inflow of immigrants (i.e., both components of the interaction term).[2] We also include the following additional covariates: the share of immigrants in the previous decade, population density, urbanization, an indicator variable for a county being connected to the railway network, and the interaction of the railway connectivity indicator variable with a measure of aggregate industrial development.

In our zero-stage panel regression, we find that the interaction term is a strong predictor of the settlement of immigrants into a county. The coefficient on the interaction is positive and statistically significant, which means that counties experienced more immigrant settlement if they were connected to the railway network *and* the aggregate flow of immigrants into the country was high at the time.

Using the zero-stage estimates, we construct measures of the share of the population that was foreign born (for each county and decade) that is predicted *using the interaction term only*. In other words, the only variation that we interpret as exogenous is the differential effect of being connected to the railway during an aggregate immigration boom versus being connected during an aggregate immigration lull. This procedure yields a predicted immigrant share for each county and decade. Using these estimated shares, we then create, for each county, an average across all time periods to construct an average predicted immigrant share in each decade from 1860–1920. Next, we estimate the cross-county relationship between average historical immigrant share (from 1860–1920) and economic outcomes today using the predicted immigrant share as an instrument for the actual immigrant share.

There are a number of potential concerns with our identification strategy. First, even though the direct effect of railway connectivity is controlled for in our zero-stage equation, we find that our instrument is correlated with how early a county was connected to the railway. As we will show, there is a small (but significant) difference in the average date of connection for counties connected prior to boom periods relative to those connected prior to lull periods. To err on the side of caution, in our 2SLS equations, we control for a measure of when the county became connected to the railway network.

A second potential concern is that decades with high aggregate immigration flows may have

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[2]In the specification, the aggregate inflow of immigrants drops out of the specification since it is absorbed by decade fixed effects.

been different in other ways. For example, if high levels of aggregate immigration happened to have coincided with high levels of industrial development, then the differential effect of connection to the railway depending on aggregate immigration may be correlated with the differential effect of connection to the railway depending on industrial development. Given this concern, our zero-stage specification includes an interaction of the railway connection indicator and an index of aggregate industrialization in the United States to allow railway connection to have a differential effect along these lines. This controls for any differential effect of railway connection that depends on industrialization. Following the same procedure as with our instrument, we create a measure of predicted immigration using this interaction term, and we control for this generated variable in all of our IV specifications. Thus, any effects that are due to the timing of connection to the railway relative to the level of industrialization should be accounted for by this covariate.

A third potential concern with our estimates is the possibility that the aggregate flow of immigrants could have been endogenous to railway expansion. In particular, if immigrant inflows tended to increase once the railway became connected to counties with a greater future growth potential, then our instrument would suffer from reverse causality and be invalid. Thus, as a robustness check, we construct a measure of the predicted flow of European migrants to the United States that is determined solely by temperature and precipitation shocks in the origin countries. By using the flow of immigrants determined by origin-country weather shocks, we can correct for the potential endogeneity of immigrant flows to factors from within the United States – including the railway expansion. We find that predicted immigrant flows are strongly correlated with actual flows, and that using the predicted values yields estimates that are nearly identical to our baseline estimates.

We find that historical immigration (from 1860–1920) resulted in significantly higher incomes, less poverty, less unemployment, more urbanization, and higher educational attainment today. The estimates, in addition to being statistically significant, are also economically meaningful. For example, according to the estimates for per capita income, moving a county with no historical immigration (i.e., during 1860–1920) to the 50th percentile of the sample (which is 0.049) results in a 20% increase in average per capita income today. We also check whether these long-run economic benefits came alongside long-run social costs. We find no evidence that historical immigration affects social cohesion as measured by social capital, voter turnout, or crime rates.

Our analysis also attempts to gain some insight into the potential mechanisms that underlie our estimates. We first examine whether our estimates reflect the creation of economic benefits by immigrants or the displacement of economic benefits from locations that received fewer immigrants to locations that received more immigrants.[3] To address this question, we test for the presence of spillovers effects. If our findings are due to the relocation of economic activity, we expect to find that immigration to a location has negative effects in nearby regions. Therefore, we estimate the effect that immigration into a county affects economic outcomes in neighboring counties, in other counties within the same state, and in other counties within the same state that are not neighbors. We find no evidence of immigration into a county resulting in a decline in long-run economic prosperity in nearby counties.

As a second step in better understanding mechanisms, we ask when the economic benefits of immigrants began to emerge. It's possible that in the short-run, immigrants acted as a burden on the economy and the benefits they brought were only felt in the medium- or long-run. The immigration backlash and the rise of social and political nativist movements at the time suggest that there may have been initial costs to immigration. However, our estimates show that immigration resulted in benefits that were felt soon after their arrival. Immigration resulted in more and larger manufacturing establishments, greater agricultural productivity, and higher rates of innovation.

These findings are consistent with a long-standing narrative in the historical literature suggesting that immigrants benefitted the economy by providing an ample supply of unskilled labor, which was crucial for early industrialization. Immigration also resulted in a small but potentially important supply of skilled individuals, who provided knowledge, know-how, skills, and innovations, which were economically beneficial and particularly important for industrial development.[4]

Having estimated the short-run effects of immigrants, we then turn to an examination of the full dynamic effects, examining their effects in the short-, medium-, and long-runs. Examining urbanization rates in each decade from 1920–2000, we find that the vast majority of the benefits

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[3]As in Kline and Moretti's (2014) analysis of the Tennessee Valley Authority, greater early industrialization may be directly offset by a decrease in industrialization elsewhere in the economy.

[4]On average, immigrants appear to have been less educated than native-born populations. We find that, consistent with this, immigration is associated with lower levels of education in the short-run (prior to 1920). However, in the medium- and long-run (1950 and later), we find that historical immigration switches to having a positive effect on education levels, which increases monotonically over time.

of immigration from 1850–1920 were felt by 1920, and that these benefits persisted, increasing slightly, until 2000. We find a similar pattern for income and education for the post WWII period from when there are data.

This study provides several new findings that help better understand the effects of immigration in U.S. history. The first is that in the long-run, immigration has provided large economic benefits. The second is that there is no evidence that these long-run benefits come at the expense of short-run economic costs. In fact, immigration immediately led to economic benefits that took the form of higher incomes, higher productivity, more innovation, and more industrialization. These findings complement recent scholarship examining the selection of immigrants to the United States (e.g., Abramitzky, Boustan and Eriksson, 2012, 2013, Spitzer and Zimran, 2013) and their experiences after arrival (e.g., Abramitzky, Boustan and Eriksson, 2014), as well as the existing literature on the importance of the cultural legacies of immigration (e.g., Fischer, 1989, Ottaviano and Peri, 2006, Ager and Bruckner, 2013, Grosjean, 2014, Bandiera, Mohnen, Rasul and Viarengo, 2016). Our findings of the long-term benefits of immigrants within the United States complement existing studies that also find long-term benefits of historical immigration in Brazil (Rocha, Ferraz and Soares, 2015) and Argentina (Droller, 2013).

Our findings add new long-run evidence to a large empirical literature that examines the short-run consequences of immigration in the United States (e.g., Borjas, 1994, 1995, 1999, Card, 1990, 2009, 2012, Hunt and Gauthier-Loiselle, 2010, Peri, 2012, Rodriguez-Pose and von Berlepsch, 2014).[5] The results also complement Atack, Bateman, Haines and Margo's (2010) findings that show that in the United States Midwest from 1850–1860, railways accounted for more than half of the increase in urbanization rates. Our findings provide evidence for a potential channel underlying the Atack et al. (2010) result. The railways brought immigrants to the connected locations which, in turn, increased income and urbanization in those areas.

Our paper examines the effect of immigrants in general and not the different effects of immigrants from different countries, which has been the focus of some lines of research (e.g., Fischer, 1989, Fulford, Petkov and Schiantarelli, 2015, Burchardi and Hassan, 2015). In theory, our identification strategy could be used to instrument separately for immigrants from different countries. Following the same logic as for all immigrants, in theory, one could estimate a

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[5]While much of the literature focuses on short-run effects, an exception is Rodriguez-Pose and von Berlepsch (2014) who also examine the relationship between historical immigration and long-term economic development today.

zero-stage equation that uses variation from the interaction of the total flow of immigrants from a specific sending-country and a county's connection to the railway network. However, in practice, the large number of countries (and thus endogenous variables and instruments) results in first stage estimates that are weak and counterintuitive.[6]

Our paper is structured as follows. We begin with a description of the historical setting of our analysis. This is followed, in Sections 3 and 4 by an overview of our data and identification strategy. In Section 5, we report our baseline estimates, and in Section 6 we conduct a variety of robustness checks. In Section 7, to better understand the mechanisms, we estimate the short- and medium-run effects of immigrants. We end with concluding thoughts in Section 8.

2. Historical Background

A. *Immigration and the Railway*

Throughout our period of interest, migration was facilitated by the railways. The best land was often granted to railway companies by the Federal government in an attempt to promote the development of uninhabited territories. The railway companies, including the Union Pacific, Santa Fe, Burlington, Northern Pacific, through a variety of mechanisms, intentionally promoted the settlement of these tracks of land contiguous to their railway lines (Luebke, 1977, p. 410). They did this by selling the land cheaply and by encouraging immigrants from Europe to settle there. Common methods used to accomplish this were the establishment of advertising offices in Europe and subsidizing migrants' trans-Atlantic travel. Historian James Hedges (1926, p. 312) describes these efforts, writing that: "The stream of population which followed the wake of the railroads of the West was in part the natural consequences of the mere fact of the construction of the roads, but more largely the result of the strenuous efforts put forth by the railroad companies themselves."

Upon arrival to the United States, railroads were the primary means of transport to the interior. James Hedges (1926, p. 312) goes on to describe the settlement of the Western United States as "a story of Mennonites and sects from South Russia, journeying out to the prairies of Kansas, not

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[6] In practice, one would have over 30 endogenous immigrant share variables, one for each sending country for which we have data, and the same number of instruments. Doing this, one finds that the first stages are all very weak. In addition, in the first-stage equations, immigrant flows often load on the "wrong" instruments e.g., other countries' instruments are better predictors than the own-country instrument. These issues are most likely due to the collinearity that is present in the endogenous variables and the instruments.

with wagon and ox-teams but in the drab passenger coaches of early western railroads. It is the story of Swedes and Norwegians in Minnesota, of Germans in Dakota, Bohemians in Nebraska and of Hollanders in Iowa, who sought new homes where the railroads led them." Thus, the railways were an important means of transport for immigrants moving from the coastal ports of the east to the interior of the United States.

B. *Why Migrants Matter in both the Short- and Long-Run*

There are several reasons why immigration during America's Age of Mass Migration may have mattered in both the short- and long-runs. The contributions of immigrants are nicely summarized by John F. Kennedy in his book, *A Nation of Immigrants*, where he writes: "Between 1880 and 1920 America became the industrial and agricultural giant of the world...This could not have been done without the hard labor, the technical skills and entrepreneurial ability of the 23.5 million people who came to America in this period" (Kennedy, 1964, p. 34). We discuss each of these potential contributions of immigration below.

Provision of unskilled labor: Immigrants may have spurred industrialization by providing a large supply of unskilled labor. During the Age of Mass Migration, immigrants provided labor to newly established factories. As historian James Bergquist (2007, pp. 264–265) puts it: "New Immigration from England, Ireland, and Germany brought many of the working classes to the growing industrial centers and to the coal-mining regions. Many of the English and Germans had previous experience in the industrial cities of their homelands."

Many have hypothesized that the rapid increase in industrialization in the United States was fueled by immigrant labor. For example, Foerster (1924, p. 331) writes that "the sixfold increase in the capital invested in manufactures between the outbreak of the Civil War and the year 1890, a period in which the population in the country doubled, was largely made possible by the inpouring immigrants."

Evidence that immigration resulted in cheaper labor costs – i.e., low wages – has been put forth by Goldin (1994). Examining variation across American cities from 1890 to 1903, she finds that greater immigration was associated with lower wage growth: a one-percentage-point increase in the foreign-born population is associated with a decrease in wages of about 1.0–1.5 percent. Interestingly, these effects are found both for less-skilled laborers and more-skilled artisans.

Provision of important skills for industry: Although the vast majority of immigrants worked in unskilled occupations, an important fraction engaged in more specialized activities. Malone (1935) reports that among the noteworthy and exceptional individuals summarized in the fifteen volume *Dictionary of American Biography*, 12.5% of those born after 1790 were foreign born, which is higher than the national proportion of foreigners (10.1% in our sample). More recently, Abramitzky et al. (2014) examine the occupational distribution of immigrants and natives in 1900, and find that immigrants were as equally likely as natives to be in unskilled occupations, much less likely to be in farming, and more likely to hold semi-skilled or skilled blue collar occupations such as carpenters or machinists.

Some immigrant groups were disproportionately represented in skilled occupations. For example, in 1870, 37% of German-born workers were employed in skilled occupations (Daniels, 2002, p. 150). Bergquist (2007, p. 194) describes the early migrants from 1870–1920 as often bringing "skills and knowledge that paved the way to becoming self-sufficient tradesmen". These skilled immigrants included carpenters, cabinetmakers, blacksmiths, brewers, distillers, barbers, tailors, machinists, jewelers, clockmakers, butchers, bakers, sculptors, artists, and musicians. Immigrants commonly used expertise and/or experience to gain a foothold in particular trades.

Different immigrant groups tended to bring with them different sets of experiences and skills that allowed them to specialize in particular occupations. For example, Bergquist (2007, p. 195) describes the Genoese Italians: "Reflecting their origins in a region with a venerable tradition in the commercial trades, the Genoese opened saloons and restaurants; they also went into confectionary and fresh fruit businesses." Describing Jewish immigrants, he writes that "their premigration experiences as well as cultural traditions also equipped eastern European Jews and Armenians with abilities suitable to the retail and professional undertakings" (Bergquist, 2007, p. 195).[7]

Provision of agricultural know-how: Immigrants represented a small but important proportion of farm operators (15.3% in 1900 and 10.5% in 1920), with the vast majority of these being owner-operators (80% in 1920) (Cance, 1925, pp. 102–103). Immigrants also contributed to productivity improvements within agriculture, bringing with them knowledge about agricultural techniques. Cance (1925, p. 113), writing just after the end of the Age of Mass Migration, argues that "some

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[7]Formal empirical evidence of skilled immigrants having important effect on industrial development has been put forth in other contexts. For example, Hornung (2014) finds large positive effects of 17th century Huguenot immigration into Prussia on the productivity of textile manufacturing.

of the very best of our farmers are immigrants of the first and second generation," a fact that he attributed to their "better farm practices" (p. 104).

The most notable group of immigrant farmers were the Germans, the largest immigrant group within the farming sector, accounting for 25% of all foreign-born farm-operators in 1920 (Cance, 1925, p. 113). Kollmorgen (1942, pp. 53–54), describes the Pennsylvania Germans: "Not only did the Pennsylvania German adopt new kinds of crops and better stock, he also perfected and popularized certain seeds, crops and foods. He was the first to breed the Conestoga horse; he became known for the variety of vegetables he raised; he played an important part in perfecting several kinds of wheat and apples. Moreover, he pioneered the rotation and diversification of crops and in providing good shelter for stock." A particularly telling example of this is the introduction of the alfalfa seed, which was widely adopted as an excellent foraging crop in the Northwest. In 1857, the seed was taken to Minnesota from a village in Baden by a German immigrant named Wendelin Grimm (Saloutos, 1976, p. 66). In his analysis of German immigrant farmers of Texas in the late 19th century, Jordan (1966, pp. 5–7) documents numerous contemporary reports of the superiority of German farmers, citing their advanced "intelligence, industriousness, and thrift," and describing them as "laborious, persevering, and eager to accumulate."

A concrete example of the effect that immigrants had on agricultural innovation can be found in a study by Gripshover and Bell (2012) that documents innovations in the U.S. onion farming industry from 1883 to 1939. The authors examine the 97 onion-farming inventions during this period. They use the micro-census, as well as biographical and genealogical sources, to obtain as much information as possible on the inventors. They find that of the 81 different inventors, a significant proportion – 19% – were foreign-born, and 49% were either first- or second-generation immigrants. The first ever patent for a mechanical "onion-cultivator" was granted in 1883 to James Peter Turner, an immigrant born in England who moved to the United States in 1850.

Provision of knowledge and innovation: It has been noted that immigrants contributed directly to the productivity of the United States economy through important technological innovations. One example of such an innovation is the suspension bridge. John A. Roebling, a German-born and trained civil engineer, is credited with ushering in the era of the suspension bridge at a time in U.S. history in which transportation infrastructure was desperately needed. He built numerous suspension bridges, his most noteworthy being the Niagara Fall Suspension Bridge and the Brooklyn Bridge (Faust, 1916, p. 10). Other notable engineers include: Charles Conrad

Schneider (born in Saxony), who constructed the famous cantilever bridge across the Niagara River in 1883; Austrian Gustav Lindenthal, who built the Hell Gate Bridge; and John F. O'Rourke, an Irish engineer, who built seven of the tunnels under the East and Hudson Rivers, and six of the tunnels of the New York subway systems (Wittke, 1939, pp. 389–390).

Another example is Alexander Graham Bell, born in Scotland in 1847, and moved to Boston in 1871. In 1876, Bell developed an acoustic telegraph that could transmit voices and sounds telegraphically, and within a year, the Bell Telephone company was established. Other notable inventors include: David Thomas (Welsh), who invented the hot blast furnace; John Ericsson (Swedish), who invented the ironclad ship and the screw propeller; Conrad Hubert (Russian), who invented the flashlight; and Ottmar Mergenthaler (German), who invented the linotype machine (Kennedy, 1964, pp. 33–34).

Immigrants also made important contributions to the educational system of the United States (Faust, 1916, p. 10). For example, the concept of kindergarten was brought to the United States by German immigrant Friederich Fröbel. Recent research by Paz (2015) finds that the presence of kindergartens during the kindergarten movement (1890–1910) resulted in an average of 0.6 additional years of total schooling by adulthood and six percent higher income. Further, Ager, Cinnirella and Jensen (2016) show that not only did kindergartens increase education and incomes of children, but they also caused parents to have fewer children. The State University system, which began in Michigan, was modeled after the Prussian state school and university system. The Michigan model then became the standard for other state schools in the West (Faust, 1916, p. 11). The current structure of graduate departments at American Universities is also modeled after the German system. It was first introduced by Johns Hopkins University at its inception in 1876.

Immigrants also contributed to business innovation. For example, Hatton and Williamson (2005, p. 94) report that among individuals born from 1816–1850, immigrants are disproportionately represented among the top businessmen in the United States.

3. Data

Our zero-stage estimation uses a panel of counties and census decades from 1860 to 1920.[8] The key variables of the analysis are measures of whether a county was connected to the railway network in each decade and the total inflow of immigrants into the United States.

Data on a county's historical connectivity to the railway network were constructed using a number of historical maps. With these, we digitized and constructed the location of the railway network for each decade from 1830 to 1920.[9] To construct the digitized railway network, we first obtained an accurate and geo-referenced shape file of the current railway network.[10] We then laid the modern shapefile over a digitized version of a paper map of the most recent historical time period of interest: 1920. We then proceeded to remove all railway lines that exist today but did not exist in 1920. We repeated this for each earlier time period in sequence – i.e., 1910, 1900, etc – at each point removing railway lines that did not exist in the previous decade. This procedure ensures the greatest precision in digitizing the exact location of the railway lines. Because of mapping imprecisions from the original historical maps, simply tracing the lines from each paper map would have generated inaccurate maps of historical railway networks. There were a very small number of cases where railway lines existed at some point in the past, but are not in the modern shapefile. In these cases, the historical railway lines were drawn using the geo-referenced paper maps. Thus, our final dataset contains the locations of all railways that ever exists in the United States.[11]

As a measure of whether a county was connected to the railway network, we use an indicator variable that equals one if a county's boundary is intersected by at least one railway line. The proportion of connected counties steadily increased overtime from just under 20% in 1850 to over 90% in 1920 (see appendix Figure A12 for the proportion in all decades).

The second important source of information in our analysis is data on aggregate immigration

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[8]Although 1860 is the first year of our panel, we measure the presence of the railway one-decade prior. Therefore, 1850 is the earliest period of railway data that we use in our analysis. It is the decade in which the census started to consistently record whether an individual was foreign-born. The census were obtained through the Natural Historical Geographic Information System (NHGIS), which is available at `www.nhgis.org` (see Minnesota Population Center, 2011), and the Inter-university Consortium for Political and Social Research (ICPSR), which is available at `www.icpsr.umich.edu` (see Haines and Inter-university Consortium for Political and Social Research, 2010).

[9]Figures A1–A11 of the online appendix show, for time periods from 1850–1920, the digitized and geo-referenced railway network overlaid on the original paper maps from which the data were obtained.

[10]The shapefile that was used is the 2009 version of the National Transportation Atlas Railroads (NTAR), which is at a 1:100,000 scale. The data are from the United States Department of Transportation.

[11]Full details of the procedure are further reported in the paper's online appendix.
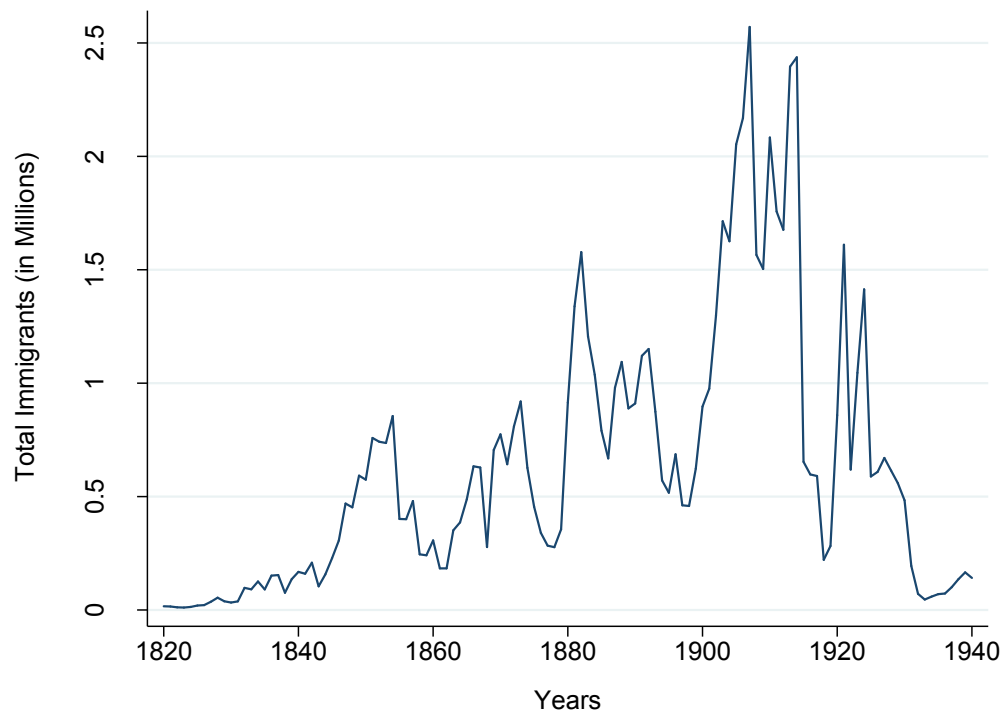
(a) Annual flow of immigrants to the United States, 1820–1940. *Source*: Migration Policy Institute.

(b) Decadal averages of annual flow of immigrants to the United States normalized by total U.S. population, 1820–1939. *Source*: Willcox (1929-1931).

Figure 1: Immigration into the United States during the Age of Mass Migration.

flows. Using Willcox (1929-1931), we digitized data for the total number of European immigrants entering the United States each year from 1820 to 1920.[12] Using this, we can calculate the total number of immigrants that arrived in the decade during our time period of interest.[13] Annual aggregate immigration inflows from 1820 to 1940 are shown in Figure 1a (Migration Policy Institute, 2016). It is clear from the figure that aggregate immigrant flows into the United States fluctuated significantly from year to year. As shown in Figure 1b, even after normalizing the flows by the current United States population and aggregating to the decade level (which is the unit of our analysis) one still observes significant variation over time.[14] This volatility, combined with the expansion of the railway network, is the variation that is the core of our identification strategy.

4. Empirical Strategy

A. *Estimating Equations*

Our identification strategy exploits two facts about immigration during the period from 1850 to 1920. First, the total inflow of immigrants fluctuated greatly across decades (recall Figure 1b). Second, the arriving immigrants tended to use the newly constructed railway to travel inland to their eventual place of residence (Faulkner, 1960, Foerster, 1969). Therefore, throughout the period of railway development, the timing of a county's connection to the railway network in relation to the aggregate inflow of immigrants at the time affected the number of immigrants that settled in the county. To capture this source of variation, our analysis begins with the following zero-stage equation:

$$\text{Migrant Share}_{it} = \alpha_t + \alpha_i + \gamma \text{Migrant Share}_{it-1} + \delta I_{it-1}^{\text{RR Access}} + \beta \text{Migrant Flow}_{t-1} \times I_{it-1}^{\text{RR Access}}$$
$$+ \theta \text{Industrialization}_{t-1} \times I_{it-1}^{\text{RR Access}} + \mathbf{X}_{it-1}\mathbf{\Gamma} + \varepsilon_{it}, \tag{1}$$

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[12]We use Willcox (1929-1931) rather than the already-digitized data available from Migration Policy Institute (2016) because Willcox (1929-1931) reports immigrants by sending country and Migration Policy Institute (2016) does not. This information is necessary for a robustness check where we predict immigration flows from a country that are due to sending country weather shocks.

[13]In our analysis, we only consider European immigrants, who comprised the vast majority of immigrants during this period. Our analysis does not therefore include immigrants from Latin America, Asia or Africa, since immigrants from these locations account for less than 5% of immigrants into the United States during our period of interest (see e.g., Abramitzky and Boustan, 2015, Figure 2).

[14]The figure reports immigrant flows by decade and normalized by the total United States population. Flows reported in decade t refer to flows during that year and the 9 years that follow. For example, 1820 in the figure refers to flows from 1820–1829. Throughout the paper we maintain this convention unless stated otherwise.

where i indexes counties and t indexes census years (1860, 1870, 1880, 1890, 1900, 1910, 1920); α_t denotes decade fixed effects and α_i county fixed effects.[15] The outcome of interest, $\text{Migrant Share}_{it}$, is the share of the population in county i that are foreign born during census year t. $\text{Migrant Share}_{it-1}$ denotes a one-decade lag of the dependent variable, which captures the mechanical relationship between the previous decade's population of immigrants and this decade's population of immigrants.[16] $\text{Migrant Flow}_{t-1}$ is the flow of all European immigrants arriving in the United States normalized by total U.S. population in the decade prior to year t (e.g., if $t = 1860$, then $\text{Migrant Flow}_{t-1}$ measures immigrants arriving from 1850–1859), and $I_{it-1}^{\text{RR Access}}$ is an indicator variable that equals one if county i is connected to the railway network in decade $t-1$ (e.g., if $t = 1860$, then $I_{it-1}^{\text{RR Access}}$ is an indicator variable for 1850).

The core of our identification strategy is the interaction between the aggregate flow of immigrants into the U.S. and whether a county was connected to the railway network: $\text{Migrant Flow}_{t-1} \times I_{it-1}^{\text{RR Access}}$. The interaction captures the differential effect that connection to the railway had on immigrant settlement during periods of high aggregate immigration relative to periods of low aggregate immigration. Thus, we expect the estimate of β in equation (1) to be positive.

The two variables that comprise the interaction terms are also included in equation (1). The coefficient δ for the variable $I_{it-1}^{\text{RR Access}}$ reflects the estimated effect of access to the railway on immigrant settlement during a decade when there are no immigrants coming into the United States. Thus, we expect the estimate of δ to be zero. The variable $\text{Migrant Flow}_{t-1}$ is absorbed by the time period fixed effects, and thus does not appear explicitly in the equation.

Given the concern that the timing of connection of the railway may have a direct effect on long-term development by allowing specialization and industrialization, we also allow the effect of railway connection to vary differentially depending on the level of aggregate industrial development at the time: $\text{Industrialization}_{t-1} \times I_{it-1}^{\text{RR Access}}$. $\text{Industrialization}_{t-1}$ is the annual average during the ten years prior to census year t.[17] This interaction term captures any differential effects that connection to the railway network has depending on the level of aggregate industrial

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[15]We have 49 state fixed effects in total: 48 states (i.e., all states but Hawaii and Alaska) and Washington D.C.

[16]Due to the presence of a Nickel bias, there is concern that the estimate of γ may be biased, which could have some effect on the other estimates, and in particular, β. As we discuss below, and report in appendix Table A3, the estimates of equation (1) are nearly identical without the inclusion of a lagged dependent variable

[17]The level of industrialization is measured using the natural log of the annual industrial production index taken from Davis (2004). The data are shown in appendix Figure A13.

development at the time.

Equation (1) also includes a vector of additional control variables, $\mathbf{X}_{it-1}$, that are intended to capture the potential influence that cities and more populous counties had in attracting immigrants: log population density, a one-decade lag of an urbanization indicator, and an interaction of the urbanization indicator with the lagged aggregate immigrant flow variable. The controls are important given the potential effect that the railway had on population growth and urbanization.

After estimating equation (1), we construct our instrument by first calculating the immigrant share in each county and period that is predicted by the interaction between the aggregate inflow of migrants and whether the county was connected to the railway network: $\widehat{\text{Migrant Share}}_{it} = \widehat{\beta}\,\text{Migrant Flow}_{t-1} \times I_{it-1}^{\text{RR Access}}$, where $\widehat{\beta}$ is the estimate of β from equation (1).

We thus have predicted measures for each county and decade, $\widehat{\text{Migrant Share}}_{it}$. Using this, we construct a predicted migrant share that is averaged over the seven census years from 1860–1920. Thus, the measure is given by:

$$\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i = \frac{1}{T}\sum_{t=1}^{T}\widehat{\beta}\,\text{Migrant Flow}_{t-1} \times I_{it-1}^{\text{RR Access}},$$

where T is the total number of time periods. Since some counties were still in the process of being formed during this period, our panel is unbalanced with counties entering over time.[18] When constructing $\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i$, we use the average immigrant share for all census years from 1860 to 1920 for which the county is in existence.

We implement our IV procedure using 2SLS, with $\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i$ as an instrument for the actual average migrant share from 1860–1920. This procedure is an example of the use of a "generated regressor", e.g., a variable constructed from predictors of an estimated equation. When estimating 2SLS using generated instruments, under very weak assumptions, the point estimates are consistent and the 2SLS standard errors and test statistics are asymptotically valid. For more information see Pagan (1984) and Wooldridge (2002, pp. 116–117).

Our 2SLS equations are given by equations (2) and (3), where equation (2) is the first stage and equation (3) is the second stage.

$$\text{Avg Migrant Share}_{is} = \zeta_s + \mu\,\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_{is} + \omega\,\text{RR Duration}_{is} + \mathbf{X}_{is}\boldsymbol{\Omega} + \epsilon_{is} \qquad (2)$$

$$Y_{is} = \xi_s + \psi\,\text{Avg Migrant Share}_{is} + \pi\,\text{RR Duration}_{is} + \mathbf{X}_{is}\boldsymbol{\Pi} + \nu_{is} \qquad (3)$$

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[18]In 1860, there are 1,600 counties in our sample, there are 1,974 counties in 1870; 2,216 in 1880; 2,468 in 1890; 2,728 in 1900; 2,797 in 1910; and 2,946 in 1920.

where i indexes counties and s states. Y_{is} is a contemporary outcome of interest; e.g., current per capita income, poverty, unemployment, education, etc. These variables are generally measured in 2000. $\text{Avg Migrant share}_i$ is the average migrant share in county i in census years from 1860 to 1920; and $\widehat{\text{Avg Migrant Share}}_{is}$ is the predicted average migrant share constructed from the zero-stage estimates of equation (1).

In equations (2) and (3), ζ_s and ξ_s denote state fixed effects, which are intended to capture broad differences between counties due to, for example, differences in geography or historical experience. RR Duration_{is} is the number of years, as of 2000, that a county has been connected to the railway network. The variable is included to address the possibility that our instrument may be correlated with early connection to the railway network, which could have an independent long-run effect on our outcomes of interest.

The vector $\mathbf{X}_i$ includes the remaining covariates. These include the latitude and longitude of a county's centroid, which account for potential relationships between our instrument and a county's east/west or north/south orientation relative to other counties in the state. Also included is a second generated regressor that is meant to account for any potential effects that the timing of a county's connection to the railway may have had due to the level of industrialization at the time. Thus, we include the following generated instrument from the zero stage estimates of equation (1): $\frac{1}{T}\sum_{t=1}^{T}\widehat{\theta}\,\text{Industrialization}_{t-1} \times I_{it-1}^{\text{RR Access}}$, where $\hat{\theta}$ is the estimated coefficient from zero-stage equation (1) and T is the number of census years from 1860–1920 for which county i is in the sample.

B. *Identification and Potential Threats to Inference*

Our IV strategy exploits the differential effect that a county's connection to the railway network has in decades with high aggregate immigration relative to decades with low aggregate immigration. During the period of analysis, once a county became connected to the railway network it generally stayed connected. Therefore, whether a county was connected during periods with relatively high aggregate immigration is primarily determined by whether a county became connected to the railway network just prior to a decade with high aggregate immigration rather than just prior to a decade with low aggregate immigration.

Thus, the primary source of variation that underlies our estimates is whether a county was first connected to the railway network prior to an immigration boom period or prior to an immigration

lull period. To provide a better sense of this variation, Figure 2 presents examples of pairs of counties that are within the same state (recall that we control for state fixed effects), but became connected to the railway at different times. Within each pair, one county became connected just prior to a high-immigration decade (i.e., a boom) and the other became connected just prior to a low-immigration decade (i.e., a lull). Counties connected just prior to a boom decade (1850s, 1880s, and 1900s) are shaded red (dark) and counties connected just prior to a lull decade (1860s, 1870s, and 1890s) are shaded yellow (light). Also reported in the figure is the subsequent average migrant share for the census years from 1860 to 1920. The examples illustrate how the exact timing of a county's connection to the railway network can have significant effects on the extent of subsequent immigration into a county.

An important question regarding the validity of our empirical strategy is the comparability of counties that were connected just prior to immigration booms and lulls. In Table 1, we compare baseline economic, demographic, and geographic characteristics that might have been correlated with the placement of the railroads or the settlement of migrants, and ultimately, with our outcomes of interest today. We find that the two sets of counties were very similar at baseline (i.e., 1840). Panel A reports differences in the share of foreign-born in 1820 and 1830. Panel B reports differences in a wide range of economic characteristics, including the share of the population in commerce, share of the population in agriculture, share of the population in mining, per capita investments of capital in manufacturing, value of agricultural output per capita, value of agricultural crops per capita, the number of post offices per 1,000 inhabitants, newspapers per 1,000 inhabitants, or the presence of a connection to a canal or naturally navigable waterway. In panel C, we examine geographic characteristics, namely whether a county is located in the Midwest/West, or in the South.

Overall, we find that for the vast majority of characteristics, there is little to no significant difference between the two groups. However, we do find statistically significant differences in how early the railway was connected and the share of counties in the Midwest or West. These differences underscore the importance of our inclusion of date of connection to the railway network and state fixed effects as controls in our 2SLS regression estimates.

A concern for our empirical strategy arises from the fact that the railways may have promoted long-term economic growth through mechanisms other than the transportation of immigrants. As the United States industrialized, counties that became connected to the railway network during
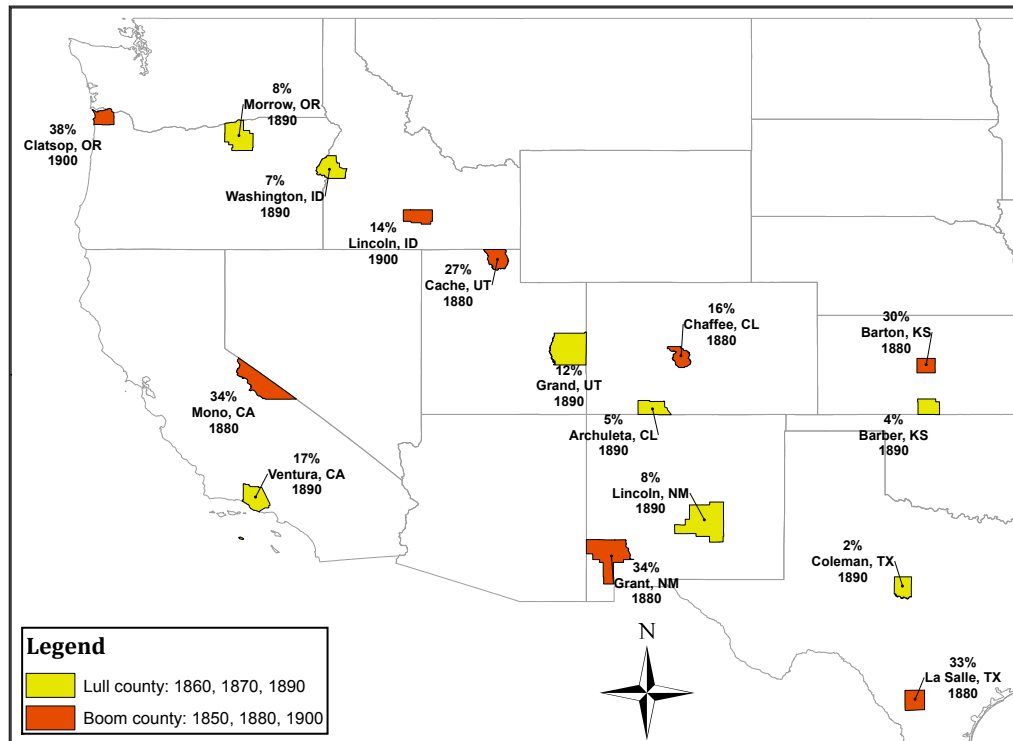
(a) Map of the Western United States.
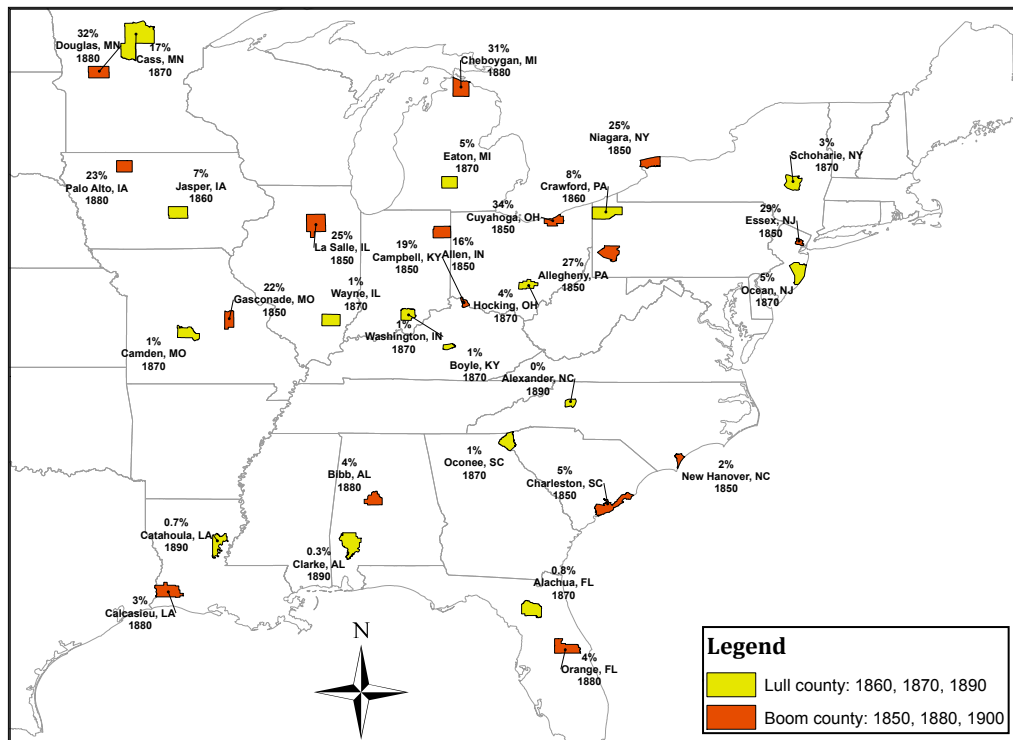
(b) Map of the Eastern United States

Figure 2: Illustration of the variation behind the identification strategy. Pairs of counties within the same state are shown. One county was connected just prior to an immigration boom and the other county was connected just prior to an immigration lull. Reported next to each county is the average immigration share from 1860–1920, the county name, and the first full decade in which the county was connected to the railway.

Table 1: Examining differences in baseline characteristics between lull- and boom-connection counties.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
							Equality of Means	Chi Square
	Boom-Connection Counties			Lull-Connection Counties				
	Obs	Mean	Std Dev	Obs	Mean	Std Dev	*p*-value	*p*-value
Demographic Composition:								
Foreign Share of the Population, 1820	490	0.005	(0.011)	204	0.004	(0.010)	0.160	
Foreign Share of the Population, 1830	629	0.004	(0.0005)	286	0.003	(0.001)	0.070	
Economic Characteristics:								
Decades connected to the Railway (as of 2000)	1,421	134	(0.474)	1,090	123	(0.341)	0.000	
Urban Share, 1840	795	0.934	(0.372)	408	0.786	(0.210)	0.728	
Population Density, 1840	781	0.180	(1.107)	386	0.071	(0.107)	0.010	
Share of the Population in Commerce, 1840	763	0.005	(0.006)	316	0.004	(0.007)	0.452	
Share of the Population in Agriculture, 1840	781	0.247	(0.123)	386	0.256	(0.127)	0.252	
Share of the Population in Mining, 1840	781	0.0009	(0.0048)	386	0.0009	(0.0053)	0.990	
Capital Invested in Manufacturing per capita, 1840	776	10.26	(18.70)	385	9.29	(36.38)	0.625	
Value of Agricultural Output per capita, 1840	774	45.95	(28.57)	384	44.18	(32.07)	0.361	
Value of Agricultural Crops per capita, 1840	774	41.82	(28.11)	384	40.56	(31.96)	0.511	
Post Offices per 1,000 Inhabitants, 1840	846	0.665	(0.019)	448	0.636	(0.060)	0.644	
Newspapers per 1,000 inhabitants 1840	252	0.175	(0.020)	138	0.125	(0.026)	0.130	
Water Connection Indicator, 1840	782	0.515	(0.500)	386	0.469	(0.500)	0.136	
Geographic Characteristics:								
Share of Counties in the Midwest and West	1,421	42%		1,090	51%			0.000
Share of Counties in the South	1,375	44%		1,009	41%			0.277

Notes: "Boom-Connection Counties" are counties that we observe as connected to the railway for the first time in either 1850, 1880 or 1900. "Lull-Connection Counties" are counties that we observe as being connected for the first time in 1860, 1870 and 1890. Column 7 reports the p-value from a test of equality of means with unequal variances, while column 8 reports the *p*-value for a Chi-square test of equality of proportions.

certain periods may have disproportionately benefited, and this may have had long-term effects (Haines and Margo, 2008, Atack and Margo, 2010, Atack et al., 2010, Atack, Haines and Margo, 2011, Donaldson and Hornbeck, 2016). As explained above, to address this, we construct a control variable that accounts for these differential historical effects using the exact same logic and procedure as we use for our immigration instrument. In the zero-stage equation, we control for the interaction of the railway-connection indicator with a measure of aggregate industrial development. We use the zero-stage estimates to construct a predicted measure and include this as a control variable in our 2SLS equations.

A comparison of Figure 1b and appendix Figure A13 provides some intuition for the variation underlying our estimates. While aggregate industrial production is steadily increasing during the period of interest, aggregate immigration increases, then decreases, then increases, and then decreases. In part, it is these differences in aggregate trends that provide the identification for our estimates.[19]

[19]The logged industrialization index closely approximates a linear time trend. Thus, the estimates are very similar if one uses the interaction between a linear time trend and the railroad access indicator, rather than the industrialization index and the railway access indicator.

Another concern is that aggregate immigrant inflows may have been influenced by the nature of the railway network at the time. For example, the aggregate flow of immigrants may have increased when the railway became connected to counties with greater future growth potential. We address this concern by constructing a measure of aggregate immigrant flows that is solely due to sending-country weather shocks. As we report in Section 6, this alternative procedure generates estimates that are very similar to our baseline estimates.

5. Estimates

A. *Zero-Stage Estimates: Construction of the Instrument*

Estimates of the zero-stage equation (1) are reported in column 1 of Table 2. All standard errors are adjusted for spatial autocorrelation and we report Conley standard errors using a five-degree window. We see that the estimated coefficient for our interaction of interest – the railroad access indicator multiplied by aggregate immigrant inflows into the United States (normalized by total population) – is positive and statistically significant. In addition, the point estimate on the coefficient of the uninteracted railway-connection indicator is very close to zero, which indicates that connection to the railway is estimated to have no effect on immigrant settlement when aggregate immigration flows are zero. This is reassuring since it provides evidence that the estimates of the effect of railway access on immigrant settlement do not capture channels other than the one that we have in mind.

To illustrate the variation underlying the interaction term, we estimate a more flexible variant of equation (1), where we interact the indicator for whether a county had access to the railway network with decade fixed effects, rather than with the aggregate inflow of immigrants to the United States. This allows the importance of being connected to the railway to vary flexibly over time. We then examine the relationship between the coefficients of the interaction terms and the aggregate inflow of immigrants during the previous decade. As shown in Figure 3, we observed a strong positive relationship between the two variables (corr = 0.73). The decades in which connection to the railway network had the largest effects on county-level immigrant settlement are also the decades for which we observe the largest aggregate immigrant inflows.

Our baseline sample includes all counties. We recognize that one could argue that the logic of our identification strategy applies less well (or does not apply at all) to the Northeast of the

Table 2: Zero-stage OLS panel estimates.

Dependent Variable	(1)	(2)	(3)	(4)
	Migrant Share of Total County Population			
	All Counties	**Excluding Northeast**	**Excluding South**	**Midwest and West**
Interaction of Interest:				
Lag Rail Access	0.149***	0.153***	0.177***	0.197***
x Lag Migrant Inflow/Total US Population	[0.032]	[0.034]	[0.055]	[0.061]
Other Variables:				
Lag Rail Access	-0.006	0.002	-0.025**	-0.009
	[0.008]	[0.009]	[0.014]	[0.015]
Lag Rail Access	-0.003	-0.006**	0.003	-0.003
x Lag Log Industrialization Index	[0.003]	[0.003]	[0.006]	[0.006]
Lag Migrant Share	Yes	Yes	Yes	Yes
Lag Urban Indicator	Yes	Yes	Yes	Yes
x Lag Migrant Inflow/Total US Population	Yes	Yes	Yes	Yes
Lag Urban Indicator	Yes	Yes	Yes	Yes
Log County Population Density	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Decade Fixed Effects	Yes	Yes	Yes	Yes
Observations	16,729	15,706	11,591	10,568
R-squared	0.927	0.927	0.917	0.919
Mean of Dependent Variable	0.087	0.084	0.115	0.113

Notes: OLS estimates are reported. An observation is a county in a time period (1860, 1870, 1880, 1890, 1900, 1910 or 1920). The dependent variable "Migrant Share of Total County Population" is the proportion of a county's population that is foreign born in period t. "Lag Rail Access" is an indicator variable that equals one if a county has a railway in period $t-1$. Conley standard errors are reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

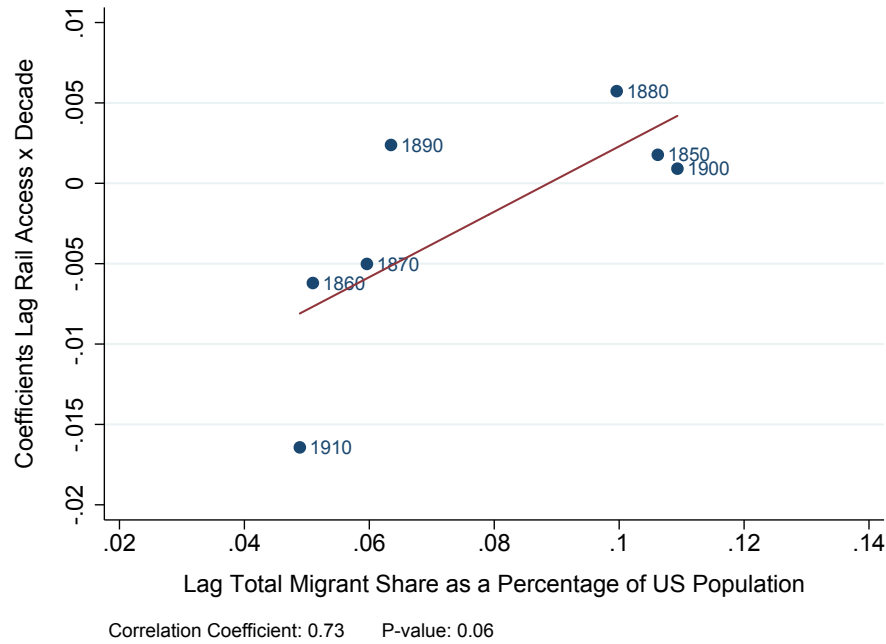Correlation Coefficient: 0.73   P-value: 0.06

Figure 3: Estimated effect of a county's connection to the railway on immigrant settlement in a decade and total immigration (as a share of total population) in that same decade.

United States, where there are many urban centers located on the coast, where travel distances are relatively short, and where the railway network was already developed prior to the first period in our analysis. Thus, as a robustness check, we re-estimate equation (1), but omit counties from the Northeast from the sample.[20] The estimates, which are reported in column 2 of Table 2, show that omitting the counties in the Northeast results in estimates that are nearly identical to our baseline full-sample estimates.[21]

A related concern is the applicability of the model to the U.S. South, which featured comparatively little immigration from Europe. In column 3, we report estimates, after omitting counties in the South. Again, we find that our estimates are similar. The point estimate increases slightly in magnitude and remains statistically significant. Lastly, column 4 reports estimates when we omit both the Northeast and South together; again the results remain robust.

Lastly, the zero-stage estimates are not sensitive to the inclusion of a lagged dependent variable. We obtain very similar estimates if this is omitted from the specification.[22]

B. *The Long-Term Economic Effects of Immigration*

Using the zero-stage estimates of Table 2, we use the method described in Section 4 to construct our predicted average migrant share. We then use this variable as an instrument for actual average migrant share. Estimates examining measures of the economic health of a county today are reported in Table 3. Panel A reports OLS estimates of second-stage equation (3), panel B reports the 2SLS estimates of equation (3), and panel C reports the first-stage estimates – i.e. equation (2). The reported standard errors are Conley standard errors adjusted for spatial correlation using a window of five degrees.[23]

As reported in panel C, our predicted-migrant-share instrument is strongly correlated with actual migrant share, resulting in a strong first stage. The Kleibergen-Paap F-statistics are approximately 10.4. According to the 2SLS estimates (panel B), counties with a greater share of immigrants from 1860 to 1920 have significantly higher average per capita income in 2000

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[20]We follow the regional definitions from the census. The Northeast includes Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island and Vermont.

[21]These characteristics of the Northeast also provide an opportunity for a placebo test to check whether other omitted factors are driving our estimates. In particular, looking at the Northeast only, we should not observe the same effects as we do for the rest of the country. As we show in appendix Table A2, this is exactly what we find.

[22]These estimates are reported in appendix Table A3.

[23]The results are very similar when we use smaller or larger windows, e.g. one degree or ten degrees.

Table 3: OLS and 2SLS estimates of the effects of historical immigration on the health of the economy today.

Dependent Variable	(1) Log Average per Capita Income, 2000	(2) Prop of Population Below Poverty Line, 2000	(3) Unemployment Rate, 2000	(4) Urbanization Share, 2000	(5) Average Years of Schooling, 2000
	A. OLS Estimates				
Average Migrant Share,	0.183**	0.015	0.036***	0.930***	-0.210
1860-1920	[0.080]	[0.016]	[0.013]	[0.081]	[0.206]
	B. 2SLS Estimates				
Average Migrant Share,	4.080***	-0.599**	-0.606**	6.234***	12.302***
1860-1920	[1.463]	[0.288]	[0.239]	[2.222]	[4.345]
	C. First Stage Estimates				
	Dependent Variable: Average Migrant Share, 1860-1920				
Predicted Avg. Migrant Share,	4.423***	4.423***	4.423***	4.423***	4.423***
1860-1920	[1.357]	[1.357]	[1.357]	[1.357]	[1.357]
Kleibergen Paap F-statistic	10.43	10.43	10.43	10.43	10.43
Controls (in all Panels):					
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	2,935	2,935	2,935	2,935	2,935
Mean of Dep. Var. (2nd-Stage and OLS)	10.02	0.136	0.047	0.401	11.45

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

(column 1). The magnitude of the coefficient suggests that moving a county's average historical migrant share from zero to the 50th percentile of the sample – a change of 0.049 or 4.9% – results in an increase in average income of $4.08 \times 0.049 = 0.20$ or 20%.[24] We view this as a sizeable, but plausible, effect.

The difference between the OLS and 2SLS estimates for per capita income (panels A and B) is consistent with negative selection by immigrants. The OLS correlation between historical migrant share and current per capita income is much smaller than the 2SLS estimate. An explanation for this is that migrants tended to move to "worse" places that counterfactually would have had lower long-run economic growth. This selection results in OLS estimates that are biased towards zero and understate the positive effect of immigrants on long-term economic growth.[25] This could have been because better places were congested or because of discrimination against immigrants

[24]In reporting magnitudes, we focus on the median rather than the mean because the distribution of average migrant share is noticeably right skewed, with a large number of counties with very low levels of average migrant share, and a small number of counties with high levels (see appendix Figure A14). The mean of average migrant share is 0.098 and the standard deviation is 0.111. The median is 0.049, the 25th percentile is 0.007, and the 75th percentile is 0.163.

[25]It is also the case that relative to the OLS estimates, the 2SLS local average treatment (LATE) estimates place more weight on regions that experienced new railroad development during our period of analysis, such as the West and Midwest. This is another potential explanation for the difference in magnitudes. To get some sense of the importance of this, we re-estimate the regressions of Table 3 separately for the the Midwest and West, and for all other counties (i.e., the Northeast and South). As appendix Tables A4 and A5 show, the OLS and IV estimates are very similar in the two samples, as are their relative magnitudes.

from natives.

We next consider alternative measures of the strength of a county's economy: the proportion of the population living below the poverty line (column 2) and the unemployment rate (column 3). We estimate a negative effect of historical migrant share on both poverty and unemployment. According to the estimates, moving a county with no historical immigration to the 50th percentile of the distribution (0.049) is associated with a decrease in the proportion of people living under the poverty line by 3 percentage points and a decrease in the unemployment rate by 3 percentage points. These findings are consistent with the long-run increase in income found in column 1. In addition, comparing the OLS to the 2SLS estimates again provides evidence that migrants may have selected into locations with worse long-run growth potential.

In columns 4 and 5, we consider two last measures of economic development: the urbanization rate and average years of schooling. We estimate a large positive effect on both urbanization and education. An increase in average migrant share from zero to the 50th percentile (0.049) is associated with a 31 percentage-point increase in the urbanization rate and 0.6 additional years of schooling.

Overall, the estimates show that within the U.S. historical context, immigration had large positive effects on long-run economic growth and prosperity.

C. *The Long-Term Social Effects of Immigration*

Having estimated the long-term economic benefits of immigration, we now turn to an examination of the potential long-run social effects of immigration. It is possible that although immigration had positive economic benefits, these coincide with long-run social costs, such as an erosion of social cohesion, civic mindedness, or an increase in crime. Thus, we also estimate the long-term effects of immigration on these social outcomes.

The first factor that we consider is a composite index of social capital that is taken from Rupasingha and Goetz (2008). The measure was created using principal component analysis applied to a range of variables such as the total number of associations and not-for-profit organizations per 10,000 people, as well as census mail response rates and voter turnout. The final variable ranges from −3.9 to +17.5 in our sample. The 2SLS estimates are reported in column 1 of Table 4. We find a statistically insignificant effect of historical immigration on social capital today. The

Table 4: OLS and 2SLS estimates of the effects of historical immigration on social outcomes.

Dependent Variable	(1) Social Capital, 2000	(2) Voting Turnout, 2000	(3) Total Crime Rate, 2000	(4) Crimes Against Persons, 2000	(5) Crimes Against Property, 2000
	A. OLS Estimates				
Average Migrant Share,	-1.293***	-0.076***	0.006***	0.001***	0.003***
1860-1920	[0.344]	[0.026]	[0.001]	[0.0003]	[0.001]
	B. 2SLS Estimates				
Average Migrant Share,	0.880	0.424	0.023	0.004	0.016
1860-1920	[4.716]	[0.395]	[0.018]	(0.004)	(0.012)
	C. First Stage Estimates				
	Dependent Variable: Average Migrant Share, 1860-1920				
Predicted Avg. Migrant Share,	4.423***	4.423***	4.423***	4.423***	4.423***
1860-1920	[1.369]	[1.369]	[1.369]	[1.369]	[1.369]
Kleibergen Paap F-statistic	10.43	10.43	10.43	10.43	10.43
Controls (in all Panels):					
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	2,934	2,925	2,935	2,935	2,935
Mean of Dep. Var. (2nd-Stage and OLS)	-0.004	0.540	0.006	0.001	0.004

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

estimated effect, in addition to being imprecise, is also small in magnitude.[26]

We next turn to alternative measures of social cohesion: political participation and crime. Column 2 of Table 4 reports 2SLS estimates of the long-term effects of immigration on political participation, measured by voter turnout in the 2000 presidential election. We find a positive, but small and insignificant, effect of historical immigration on voter turnout.[27] Columns 3–5 report estimates of the effects of immigration on crime, measured as the crime rate (crimes per year per 10,000 inhabitants) for any crime, crimes against persons, and property crimes.[28] We estimate positive, but small and statistically insignificant, effects of historical immigration on each type of crime.[29] Overall, we find no evidence of historical immigration having an effect on social capital,

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[26] An increase in historical immigration from zero to the 50th percentile (0.049) is associated with an increase in the social capital index of 0.04, a small effect given the range of the index.

[27] According to the estimated magnitude, an increase in historical immigration from zero to the 50th percentile (0.049) is associated with an increase in voter turnout of 2 percentage points, which is small when compared to the mean turnout rate of 54 percent.

[28] The measures are from 2000, and are taken from the *County and City Data Book*, which is produced by the U.S. Census Bureau.

[29] According to the point estimate from column 3, an increase in historical immigration from zero to the 50th percentile (0.049) is associated with an increase of 0.0011 crimes per year per 10,000 inhabitants, which is equal to 18% of the mean.

political participation, or crime.

6. Robustness Checks

Having reported our baseline estimates of the effects of historical immigration on long-run economic and social outcomes, we now examine the robustness of the estimates.

A. *Endogeneity of Immigrant Supply*

One concern with our estimates is that the timing of aggregate immigration booms could have been endogenous to the connection of the railway to economically attractive counties. Once the railway expanded to these counties, the flow of European immigrants might have increased in response. To address this, we check the robustness of our results to the use of a measure of aggregate immigrant flows to the United States that is driven only by origin-country weather shocks. This strategy is motivated by the existing evidence of a strong link between climate and agricultural output in Europe during the Age of Mass Migration (Solomou and Wu, 1999), as well as the presence of a strong relationship between weather shocks and international migration in the contemporary time period within developing countries (Feng, Krueger and Oppenheimer, 2010).

To construct measures of origin-country weather shocks, we use historical temperature data from Luterbacher, Dietrich, Xoplaki, Grosjean and Wanner (2004) and historical precipitation data from Pauling, Luterbacher, Casty and Wanner (2006). Both sets of data are measured annually (for each of the four seasons within a year) and at a 0.5-degree spatial resolution. Because the emigration data are at the country-level, we create country-averages of our weather variables by taking an average over all grid-cells in a country that were under cultivation at the time.[30] Our sample includes the sixteen European countries for which we have immigration, temperature, and crop data.[31] These sixteen countries account for 75% percent of European immigration into the United States from 1860–1920 as captured in Willcox (1929-1931)

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[30]The information on land under cultivation historically is taken from estimates constructed by Ramankutty and Foley (1999), who provide annual estimates at a 5 arc minute (approx. 10 kilometer) resolution.

[31]Our sample includes the following countries: Belgium, Denmark, France, Germany, Greece, Hungary, Ireland, Italy, Netherlands, Norway, Poland, Portugal, Russia, Spain, Sweden, and Switzerland.

We estimate outflows of emigrants for our period of interest using the following equation:

$$\ln \text{Migrant Flow}_{c,t+1} = \sum_{s \in S} \sum_{k \in K} \beta_{c,s,k} I^{Temp,s,k}_{c,t} + \sum_{s \in S} \sum_{k \in K} \gamma_{c,s,k} I^{Precip,s,k}_{c,t} + \varepsilon_{c,t}, \tag{4}$$

where $\ln \text{Migrant Flow}_{c,t+1}$ is the natural log of the flow of immigrants from country c in year $t + 1$. $I^{Temp,s,k}_{ct}$ is an indicator variable that equals one if the average temperature in season $s \in \{\text{Spring, Summer, Winter, Autumn}\}$ falls within temperature range k, where k indexes a set K of six temperature categories: 3 or more standard deviations below the mean, 2–3 standard deviations below the mean, 1–2 standard deviations below the mean, 1–2 standard deviations above the mean, 2–3 standard deviations above the mean, and 3+ standard deviations above the mean. Thus, the omitted category is for temperatures that are within one standard deviation of the mean (i.e., the absence of a shock). Since there are six temperature categories and four seasons there are $6 \times 4 = 24$ temperature indicator variables in total. The precipitation indicator variables are structured in exactly the same manner. Thus, there are 24 precipitation indicators as well.

An important characteristic of equation (4) is that the coefficients for the shock variables are allowed to differ for each country in the estimation. In practice, we estimate equation (4) separately for each of the sixteen European countries in our sample. After estimating the $\beta_{c,s,k}$'s and the $\gamma_{c,s,k}$'s, we can calculate predicted log migrant flows for each country and year, $\ln \widehat{\text{Migrant Flow}}_{c,t}$. We find the predicted migrant flows are strongly correlated with actual migrant flows.[32] The relationship between the two measures for each of our 16 countries is shown in appendix Figure A15. We then aggregate the predicted migrant flows across countries to obtain an estimate of the total flow of emigrants from all 16 countries in a given decade: $\text{Agg} \widehat{\text{Migrant Flow}}_t = \sum_c \exp(\ln \widehat{\text{Migrant Flow}}_{c,t})$, where c indexes countries.

The 2SLS estimates of the effects of immigrants on our economic outcomes of interest using the weather shocks as predictors of immigrant inflows are reported in Table 5.[33] The second stage point estimates of interest are similar to the results that were obtained when using actual immi-

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[32] The correlation coefficients between the actual and predicted flows measures range from 0.54 (for Switzerland) to 0.91 (for Hungary).

[33] The zero-stage estimates of equation (1) using predicted migrant flows rather than actual migrant flows are reported in appendix Table A6. The estimates are very similar to the baseline estimates reported in Table 2. Note that the reported standard errors are slightly biased upwards due to the fact that predicted migrant flows is an estimated variable. However, due to the strong correlation between actual migrant flows and predicted migrant flows, we expect this difference to be small. In addition, estimates using predicted migrant flows to instrument for actual migrant flows yield unbiased standard errors that are very similar to those reported in appendix Table A6.

Table 5: OLS and 2SLS estimates of the effects of historical immigration on the health of the economy today, using immigrant inflows predicted by sending-country weather shocks rather than actual flows.

Dependent Variable	(1) Log Average per Capita Income, 2000	(2) Prop of Population Below Poverty Line, 2000	(3) Unemployment Rate, 2000	(4) Urbanization Share, 2000	(5) Average Years of Schooling, 2000
	A. OLS Estimates				
Average Migrant Share, 1860-1920	0.183** [0.080]	0.015 [0.016]	0.036*** [0.013]	0.933*** [0.080]	-0.208 [0.206]
	B. 2SLS Estimates				
Average Migrant Share, 1860-1920	5.424*** [2.067]	-0.986** [0.429]	-0.804** [0.326]	8.826*** [3.308]	16.361*** [6.131]
	C. First Stage Estimates				
	Dependent Variable: Average Migrant Share, 1860-1920				
Predicted Avg. Migrant Share, 1860-1920	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]
Kleibergen Paap F-statistic	8.31	8.31	8.31	8.31	8.31
Controls (in all Panels):					
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	2,935	2,935	2,935	2,935	2,935
Mean of Dep. Var. (2nd-Stage and OLS)	10.02	0.136	0.047	0.401	11.45

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

Table 6: OLS and 2SLS estimates of the effects of historical immigration on social outcomes, using migrant flows predicted by sending-country weather shocks rather than actual flows.

Dependent Variable	(1) Social Capital, 2000	(2) Voting Turnout, 2000	(3) Total Crime Rate, 2000	(4) Crimes Against Persons, 2000	(5) Crimes Against Property, 2000
	A. OLS Estimates				
Average Migrant Share, 1860-1920	-1.295*** [0.344]	-0.077*** [0.026]	0.006*** [0.001]	0.001*** [0.0003]	0.003*** [0.001]
	B. 2SLS Estimates				
Average Migrant Share, 1860-1920	-3.472 [5.475]	0.470 [0.445]	0.036 [0.022]	0.007 [0.005]	0.023 [0.015]
	C. First Stage Estimates				
	Dependent Variable: Average Migrant Share, 1860-1920				
Predicted Avg. Migrant Share, 1860-1920	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]	5.835*** [2.024]
Kleibergen Paap F-statistic	8.306	8.306	8.306	8.306	8.306
Controls (in all Panels):					
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	2,934	2,925	2,935	2,935	2,935
Mean of Dep. Var. (2nd-Stage and OLS)	-0.004	0.540	0.006	0.001	0.004

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

grant flows.[34] Table 6 reports the estimated effects of immigration on the social outcomes. Again, the estimates using predicted immigrant flows are very similar to the baseline estimates that use actual immigrant flows. We continue to find no relationship between historical immigration and any of the social outcomes of interest.

B. *Reverse Causality*

Another potential concern is the possibility that railroads tended to be built in locations and during times when migration was already occurring (and was expected to continue). If this was the case, then our use of the timing of the building of the railway relative to the timing of immigration booms and lulls is potentially problematic.[35] To directly test for this possibility, we estimate a variant of equation (1), where the outcome variable is an indicator for the presence of a railroad in a county in decade t, and the independent variable of interest is the share of immigrants in the total population in the previous decade $t-1$. The estimates, which are reported in appendix Table A7, show that the coefficient on the lagged immigrant share is close to zero and statistically insignificant. Thus, railroad placement does not appear to have been endogenous to the presence of prior immigrant populations.

C. *Potential Correlation of the Instrument with Length of Time Connected to the Railroad*

In our baseline specification, we control for the length of time a county has been connected to the railroad network (as of the year 2000) to account for any potential relationship between our instrument and how late a county became connected to the railway network. Counties that were connected late will tend to have low values of the instrument since their predicted migrant share will be zero for many time periods.

To be thorough, we check the robustness of our results to the use of an alternative strategy that accounts for the relationship between the instrument and how early a county was connected to the railway network. Recall that our baseline instrument is: $\text{Avg}\,\widehat{\text{Migrant Share}}_i = \frac{1}{T}\sum_{t=1}^{T}\widehat{\beta}\,\text{Migrant Flow}_{t-1} \times I_{it-1}^{\text{RR Access}}$. Periods without railway access, $I_{it-1}^{\text{RR Access}} = 0$, mechanically

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[34]Although the predicted migrant flow variable is a predicted regressor, because it enters our 2SLS equations as part of a generated instrument, the 2SLS point estimates continue to be consistent and the test statistics remain asymptotically valid (Pagan, 1984; Wooldridge, 2002, pp. 116–117).

[35]This is also one motivation for including a lagged dependent variable in our zero-stage equations. If the presence of a pre-existing immigrant population had such effects, this should be captured by a measure of the pre-existing immigrant population.

reduces the value of $\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i$. Given this, we construct an alternative predicted migrant share instrument that is the mean of predicted migrant share, but only in the periods from 1860 to 1920 for which the county was connected to the railway network. Specifically, the alternative measure is: $\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i = \frac{1}{N_i^{\text{RR}}} \sum_{t \in T_i^{\text{RR}}} \widehat{\beta}\,\text{Migrant}\,\text{Flow}_{t-1} \times I_{it-1}^{\text{RR Access}}$, where N_i^{RR} is the number of time periods for which $I_{it-1}^{\text{RR Access}} = 1$ in county i, and T_i^{RR} is the set of census years for which $I_{it-1}^{\text{RR Access}} = 1$ for county i. Because periods without connection to the railway network are not included in the average, not being connected to the railway, $I_{it-1}^{\text{RR Access}} = 0$, no longer mechanically reduces $\text{Avg}\,\widehat{\text{Migrant}}\,\text{Share}_i$.

Appendix Tables A8 and A9 report estimates using this alternative instrument.[36] The estimated effects of historical immigration on economic and social outcomes are qualitatively similar.[37]

D. *Changing County Boundaries*

One challenge when analyzing the historical effect of immigrants across counties is that for a number of counties, current boundaries were established after 1860 (the first period of our sample). Thus, our zero-stage panel is unbalanced, with counties entering over time as they are established.[38] Additionally, once counties are established, there can be changes to their boundaries. For our baseline analysis, we match counties across time using the nominally integrated series available in the NHGIS datasets (Minnesota Population Center, 2011).[39] We also check that our results are robust to only using counties that existed in 1860, and effectively had the same boundaries in 1860 as in 2000. This is the case for 1,596 counties or approximately 55% of our sample. As shown in appendix Tables A10 and A11, the results using this more restrictive sample are qualitatively similar to our baseline estimates. The magnitude of the estimates actually increases, and the point estimates remain statistically significant.

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[36]Since the predicted average immigrant share instrument for counties that are never connected to the railway network is zero, the specifications include an indicator variable for whether the county was never connected to the railway.

[37]Under this specification, the effect of historical migration on total crime and crimes against property (but not crimes against persons) becomes marginally significant, although the point estimates remain very small in magnitude.

[38]In 1860, there are 1,600 counties in our sample, there are 1,974 counties in 1870; 2,216 in 1880; 2,468 in 1890; 2,728 in 1900; 2,797 in 1910; and 2,946 in 1920.

[39] For a detailed explanation of NHGIS' matching strategy see https://nhgis.org/documentation/time-series#geographic-integration.

7. Mechanisms

Up to this point, we have shown that counties with more immigrant settlement from 1860–1920 today are more prosperous economically and no different socially. We now attempt to gain a better understanding of the mechanisms that underlie the long-run economic effects.

A. *Evidence for the Reallocation of Economic Activity*

We begin by testing whether the long-run benefits that we estimate are due to immigrants having a positive effect on the creation of economic prosperity or whether immigrants resulted in the movement of economic activity from other locations. To assess the extent to which our estimates reflect such displacement effects, we test whether being close to a county with more historical immigration resulted in *less* long-term economic development today. We would expect such a relationship to be present if immigration caused economic activity to relocate to counties with more immigrants at the expense of nearby counties.

We do this by estimating the effect that immigration to neighboring counties had on a county. We first construct a measure of average migrant shares of all neighboring counties, where we weight each neighboring county in proportion to the length of the shared border. We denote this as $\text{Nearby Migrant Share}_{i,s}$. We then estimate the following set of equations using 2SLS. The two first stage equations are:

$$\text{Avg Migrant Share}_{is} = \alpha_s + \alpha_1 \widehat{\text{Avg Migrant Share}}_{i,s} + \alpha_2 \widehat{\text{Nearby Mig Share}}_{is} + \mathbf{X}_{is}\mathbf{\Omega} + \varepsilon_{is}, \quad (5)$$

$$\text{Nearby Mig Share}_{is} = \gamma_s + \gamma_1 \widehat{\text{Avg Migrant Share}}_{is} + \gamma_2 \widehat{\text{Nearby Mig Share}}_{is} + \mathbf{X}_{is}\mathbf{\Pi} + \mu_{is}. \quad (6)$$

And, the second stage equation is:

$$Y_{is} = \alpha_s + \beta_1 \text{Avg Migrant Share}_{is} + \beta_2 \text{Nearby Mig Share}_{is} + \mathbf{X}_{is}\mathbf{\Gamma} + \nu_{is}, \quad (7)$$

where i indexes counties and s states, and $\text{Avg Migrant Share}_{is}$ is the average share of a county population that were immigrants from 1860–1920. The new term, $\text{Nearby Migrant Share}_{is}$, is the average share of population of nearby counties that were immigrants, 1860–1920.

The estimates are reported in Table 7. Columns 1 and 2 report estimates for income and education. Due to space constraints, the estimates for all outcomes are reported in appendix Table A12. Panel A reports the OLS estimates of equation (7), panel B reports 2SLS estimates of equation (7), and panel C reports estimates from the two first stage equations – i.e., equations (5) and (6).

Although the spillover coefficients are imprecisely estimated (and not statistically different from zero), they provide no indication for the presence of negative spatial spillovers. Instead, the signs of the coefficients suggest that the spillovers may even positive. That is, being close to a county with more historical immigration may be economically beneficial. Most importantly, we also find that the point estimates of the own-county effects remain robust. Although the precision of the estimates decline slightly, the point estimates are very similar to the baseline estimates.

A concern with these results is that adjacent counties may be too close to each other to generate negative spillover effects, especially since contiguous counties today are often part of the same city, commuting zone, or economic region. Motivated by this possibility, we examine the effects of immigration to a county on all other counties in the same state. Thus, the measure of $\text{Nearby\,Mig\,Share}_{is}$ used in equations (5)–(7) is the average of historical immigrant share for all other counties within the same state. We create two versions of the measure, one where we exclude contiguous counties when constructing the state average and another where we include them.

The estimates for income and education are reported in columns 3–6 of Table 7, and the estimates for all outcomes are reported in appendix Tables A13 and A14. We continue to find no evidence for negative spillovers. As well, the estimated own-county effects remain robust to allowing for the presence of within-state spillovers. Overall, the evidence suggests that it is unlikely that the estimates we find are due to a reallocation of economic prosperity across space. This said, an important caveat is that we have tested for this by necessarily making assumptions about the particular form of the spillovers.

B. *Are the Effects Working Through Current Immigration?*

We next consider the possibility that the effects we estimate are due to an effect of historical immigration on current immigration. To test for this possibility, we examine the effects of historical immigration on migration in each decade since 1920. The estimates, which are reported in appendix Table A15, show that immediately following the Age of Mass Migration, historical immigration from 1860 to 1920 is (mechanically) associated with a greater share of foreign-born within the population. However, this relationship fades over time, and by 1950 it becomes statistically insignificant and close to zero. This provides suggestive evidence that contemporary

Table 7: OLS and 2SLS estimates, accounting for spatial spillovers (for income and education only).

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Nearby County: All Contiguous Counties		**Nearby County: All other Counties in the Same State**		**Nearby County: All Non-Contiguous Counties in the Same State**	
	Log Avg. per Capita Income, 2000	Avg. years of Schooling, 2000	Log Avg. per Capita Income, 2000	Avg. years of Schooling, 2000	Log Avg. per Capita Income, 2000	Avg. years of Schooling, 2000
A. OLS Estimates						
Average Migrant Share, 1860-1920	0.109	-0.120	0.140	-0.273	0.141	-0.270
	[0.101]	[0.256]	[0.096]	[0.260]	[0.096]	[0.260]
Average Migrant Share in Nearby Counties, 1860-1920	0.137	-0.095	-3.448	-6.110	-3.404	-5.911
	[0.137]	[0.327]	[2.644]	[7.782]	[2.640]	[7.731]
B. 2SLS Estimates						
Average Migrant Share, 1860-1920	4.425	13.363*	4.375***	12.954***	4.265***	12.634***
	[3.229]	[7.660]	[1.549]	[4.622]	[1.508]	[4.507]
Average Migrant Share in Nearby Counties, 1860-1920	5.982	10.280	19.293	43.922	11.804	20.859
	[3.872]	[9.616]	[22.543]	[65.399]	[21.575]	[63.110]
C. First Stage Estimates						
Dep. Var.: Avg Migrant Share in County, 1860-1920						
Predicted Average Migrant Share, 1860-1920	3.879***	3.879***	2.759	2.759	3.433*	3.433*
	[1.475]	[1.475]	[2.068]	[2.068]	[2.073]	[2.073]
Predicted Avg Migrant Share in Nearby Counties, 1860-1920	-0.313	-0.313	-95.214	-95.214	-56.938	-56.938
	[2.997]	[2.997]	[106.9]	[106.9]	[110.586]	[110.586]
Angrist-Pischke F-statistic	7.81	7.81	9.72	9.72	10.07	10.07
Dep. Var.: Avgerage Migrant Share in Nearby Counties, 1860-1920						
Predicted Average Migrant Share, 1860-1920	-1.114	-1.114	0.402**	0.402**	0.379***	0.379***
	[1.193]	[1.193]	[0.160]	[0.160]	[0.161]	[0.161]
Predicted Avg Migrant Share in Nearby Counties, 1860-1920	6.346***	6.346***	27.982***	27.982***	27.598***	27.598***
	[2.014]	[2.014]	[9.809]	[9.809]	[10.280]	[10.280]
Angrist-Pischke F-statistic	9.89	9.89	7.53	7.53	10.07	10.07
Controls (in all Panels):						
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (years as of 2000)	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes	Yes
Nearby Counties: Avg. Date of RR Connection	Yes	Yes	Yes	Yes	Yes	Yes
Nearby Counties: Average Latitude	Yes	Yes	Yes	Yes	Yes	Yes
Nearby Counties: Average Longitude	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,931	2,931	2,934	2,934	2,934	2,934
Mean of Dep. Var. (2nd-Stage and OLS)	10.02	11.44	10.02	11.44	10.02	11.44

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panels C reports the first-stage estimates from the 2SLS. Weighted Average Migrant Share in Contiguous Counties corresponds to the share of migrants in contiguous counties weighted by the length of the shared border with the county. Coefficient estimates are reported, with Conley standard errors in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

immigration is unlikely to be an important channel for our findings.[40]

C. *Evidence from Short-Run Estimates*

Industrialization: Based on various accounts of the historical effects of immigration, a potential explanation for the long-run economic benefits of immigration is that, during the early stages of industrial development, immigration provided a large supply of labor that was necessary for the take-off of industry and sustained modern economic growth (Goldin, 1994, Hatton and Williamson, 1998, Hirschman and Mogford, 2009). Several historians have documented that immigrants were disproportionately represented in the industrial workforce (Engerman and Sokoloff, 2000, Alexander, 2007). For example, in 1880, despite only accounting for approximately 10% of the total population, immigrants accounted for 57% of the manufacturing workforce (Hirschman and Mogford, 2009).[41]

Given this, we test whether the data are consistent with immigrants helping to spur early industrialization by estimating the effects of immigration on manufacturing output during the Age of Mass Migration and immediately afterwards. The estimates are reported in Table 8. In column 1, we examine the natural log of real manufacturing output per capita, measured as an average of census yeas from 1860–1920 and in 1930. We find that the presence of immigrants caused a large and significant increase in manufacturing output both during the Age of Mass Migration (1860–1920) and immediately afterwards (1930). According to the magnitude of the estimated effects, moving a county with no historical immigration to the 50th percentile (an increase of 0.049) led to a 50% increase in average manufacturing output per capita from 1860–1920 and a 57% increase in 1930.

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[40]As an informal check for whether part of our estimated effects of historical immigration is due to its relationship with current immigration, we control for the share of the population that is foreign-born in 2000 when estimating equation (3) with our measures of economic prosperity as the dependent variable. Keeping in mind the standard concerns and necessary caution when interpreting estimates that control for an endogenous covariate, we report the estimates in appendix Table A16. We find that the estimates of interest are nearly identical when we condition on current immigration.

[41]A related argument is that immigrants were not only a supply of labor, but that they provided labor at lower costs than native-born workers. Recent evidence in the literature appears to weigh against such a cheap-labor hypothesis. Abramitzky et al. (2013) analyze panel data on immigrant assimilation during the Age of Mass Migration in the United States and argue that the average immigrant did not face a substantial occupation-based earnings penalty upon first arrival. They also find that immigrants experienced occupational advancement at the same rate as natives during this period. However, their findings are consistent with immigration lowering wages in an industry and/or location for all workers, both native- and foreign-born Goldin (1994).

Table 8: OLS and 2SLS estimates of the effects of historical immigration on manufacturing output.

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Log Manufacturing Output per Capita		Log Manufacturing Output per Establishment		Log Number of Establishments per 10,000 Inhabitants	
	1860-1920	1930	1860-1920	1930	1860-1920	1930
	A. OLS Estimates					
Average Migrant Share,	3.079***	3.524***	2.788***	2.704***	0.346**	0.730***
1860-1920	[0.403]	[0.464]	[0.288]	[0.383]	[0.143]	[0.145]
	B. 2SLS Estimates					
Average Migrant Share,	10.093*	11.597*	1.935	8.727*	10.580***	1.630
1860-1920	[5.769]	[6.182]	[4.573]	[4.971]	[3.620]	[2.462]
	C. First Stage Estimates					
	Dependent Variable: Average Migrant Share, 1860-1920					
Predicted Avg. Migrant Share,	4.528***	5.064***	4.528***	5.064***	4.528***	5.064***
1860-1920	[1.354]	[1.530]	[1.354]	[1.530]	[1.354]	[1.530]
Kleibergen Paap F-statistic	11.19	10.95	11.19	10.95	11.19	10.95
Controls (in all Panels):						
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,805	2,463	2,805	2,463	2,805	2,462
Mean of Dep. Var. (2nd-Stage and OLS)	6.56	7.21	12.58	14.03	3.35	2.49

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

In columns 3–6, we probe specific channels further by examining the effect of immigrants on establishment size, measured using average output per establishment (columns 3 and 4), as well as the effect of immigrants on the number of establishments per 10,000 inhabitants (columns 5 and 6).[42] We find that during the Age of Mass Migration (1860–1920), the primary effect of immigrants was to increase the number of manufacturing establishments and not their size. After the Age of Mass Migration (1930), the primary effect of immigration is to increase the size of existing establishments.

Overall, the estimates show that immigrants had an immediate positive effect on industrialization. Our findings are consistent with historical accounts of immigrants bringing raw labor and manufacturing know-how, both of which were crucial for the growth of manufacturing during this time (Hirschman and Mogford, 2009).

Agriculture: We next turn to estimates of the short-run effect of immigrants on the agricultural

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[42]We measure establishment size using output per establishment. We use output rather than value added because value added data are only available for one year of our sample period, 1920. Using this alternative measure, we obtain estimates that are very similar to the estimates of columns 3 and 4.

Table 9: OLS and 2SLS estimates of the effect of historical immigration on farming.

Dependent Variable	(1)	(2)	(3)	(4)
	Log Total Farm Value (per Farm)		**Log Total Farm Value (per Acre)**	
	1860-1920	**1930**	**1860-1920**	**1930**
	A. OLS Estimates			
Average Migrant Share,	1.168***	1.927***	2.127***	2.422***
1860-1920	[0.207]	[0.197]	[0.223]	[0.271]
	B. 2SLS Estimates			
Average Migrant Share,	0.168	7.977**	4.470	11.758**
1860-1920	[3.476]	[3.261]	[3.297]	[4.640]
	C. First Stage Estimates			
	Dependent Variable: Average Migrant Share, 1860-1920			
Predicted Avg. Migrant Share,	4.279***	4.279***	4.279***	4.279***
1860-1920	[1.350]	[1.350]	[1.350]	[1.350]
Kleibergen Paap F-statistic	10.05	10.05	10.05	10.05
Controls (in all Panels):				
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,804	2,800	2,804	2,800
Mean of Dep. Var. (2nd-Stage and OLS)	10.42	11.51	5.91	6.56

Notes: An observation is a county. Log Total Farm Value corresponds to the following decades: 1860 and 1900-1930. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

sector. Our outcome of interest is total farm values, normalized using either the number of farms or the total acres of farmland.[43] Estimates are reported in Table 9, where columns 1 and 2 use farm value per farm (in 1860–1920 and 1930), while columns 3 and 4 use farm value per acre (in 1860–1920 and 1930) as the dependent variable. For both sets of estimates, we see modest positive effects of immigration on farm values in 1860–1920, with these effects becoming large and statistically significant in 1930. According to the estimates, moving a county with no historical immigration to the 50th percentile (0.049) is associated with a 39–58% increase in 1930 farm values depending on the method of normalization. Thus, immigration appears to have had large positive effects in the agricultural sector, but with the benefits primarily arising just after the end of the Age of Mass Migration.

Human Capital: We next turn to the possibility that immigrants may have resulted in a greater stock of technology and human capital. We examine this potential channel by first estimating the

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[43] All data are from the Agricultural Census. Acres of land are only reported as being within the following categories: less than 3 acres, 3–9 acres, . . . , 1000+ acres. We approximate total acreage by using the midpoint of each category, and 1000 for the 1000-or-more-acre category.

short-run effects of immigration on educational outcomes. Specifically, we consider the average share of children enrolled in school in the decades from 1870–1920. Column 1 of Table 10 reports these estimates. We find that counties with a higher share of immigrants actually had *lower* enrollment rates. We obtain a similar finding if we instead look at the average share of the total population that is illiterate from 1870–1920 (column 2). Thus, we find that immigration is associated with lower rates of literacy.[44]

Our finding that immigration resulted in less education in the short-run is consistent with the fact that immigrants tended to be less educated than native-born populations, particularly towards the end of the Age of Mass Migration. Examining the average rate of illiteracy of native-born and foreign-born populations in the censuses, we find that in 1850, 9% of immigrants were illiterate versus 4% of natives. In 1870, these figures are close to equal at 15% and 14%, respectively. However, from this point forward, the rates begin to diverge noticeably. In 1900, 13% of immigrants were illiterate compared to 3% of natives; in 1910, these figures were 12% and 2%; and in 1920 they were 12% and 1%.[45] The negative contemporaneous relationship between immigration and educational attainment could also arise, in part, due to the positive economic effects of immigration, which increased the opportunity cost of schooling.[46]

Comparing the short-run effects of immigration on education in columns 1–2 of Table 10 to the long-run education effects reported in column 5 of Table 3, it is clear that there has been a reversal of the effects of immigration on education. In the short-run, immigrants reduced average education, while in the long-run they increased it. There are several possible explanations for this. First, it may be that the effects arise due to the long-run effect of immigrants on income, and the fact that today higher incomes are associated with more education. A second explanation is the mechanism found in the recent study by Foged and Peri (2015). The presence of immigrants – and their supply of unskilled labor – in the long-run, could have led native workers to pursue less manual-intensive occupations and to obtain more schooling. Third, they could also be due, in part, to the mechanism present in the study by Bandiera et al. (2016), where it is shown that states with more immigration from European countries that were less exposed to compulsory education

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[44]These effects are particularly interesting given the existing evidence that the railroads increased educational attainment (Atack, Margo and Perlman, 2012).

[45]The fact that immigrants had less education than native populations differs from other countries. Immigrants that went to Brazil in the late 19th and early 20th centuries, on average, were more educated than the native populations. In this setting, the evidence suggests that immigration resulted in higher levels of education, which had a persistent effect, resulting in higher living standards today (Rocha et al., 2015).

[46]Such an effect has also been found in modern Mexico (Atkin, 2016).

Table 10: OLS and 2SLS estimates of the effects of historical immigration on historical human capital and innovation.

Dependent Variable	(1)	(2)	(3)	(4)
	Educational Attainment		Innovation	
	Share Enrolled In School, 1870-1920	Share Illiterate, 1870-1920	Log Total Patents per 10,000 Inhabitants 1860-1920	Log European Patents per 10,000 Inhabitants 1860-1920
	A. OLS Estimates			
Average Migrant Share,	-0.139***	0.139***	1.069***	2.731***
1860-1920	[0.008]	[0.021]	[0.332]	[0.225]
	B. 2SLS Estimates			
Average Migrant Share,	-0.568***	1.447***	28.070***	6.416**
1860-1920	[0.191]	[0.533]	[9.694]	[2.565]
	C. First Stage Estimates			
	Dependent Variable: Average Migrant Share, 1860-1920			
Predicted Avg. Migrant Share,	4.423***	4.423***	4.369***	4.369***
1860-1920	[1.369]	[1.369]	[1.367]	[1.367]
Kleibergen Paap F-statistic	10.43	10.43	10.18	10.18
Controls (in all Panels):				
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,935	2,935	2,929	2,929
Mean of Dep. Var. (2nd-Stage and OLS)	0.190	0.104	3.561	0.312

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

were more likely to adopt compulsory education under the belief that exposure to American public schools would instill the desired civic values that were missing among the immigrants. A final potential explanation is that although immigrants were (on average) less skilled than the native population, they may have had values and aspirational beliefs that facilitated the rapid accumulation of education among their children and/or future generations of children in their communities. This is consistent with the fact that although immigrants were less educated than native populations, their children tended to be more educated.[47]

Innovative Activity: Another mechanism through which immigrants could have affected early economic development is through innovative activities and knowledge creation (Fairlie and Lofstrom, 2015). Although most immigrants were unskilled, an important subset of immigrants

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[47]For example, the 1910 *Report of the Immigration Commission* studied 12,011 male iron and steel workers from the Midwest. It found that although the proportion of foreign-born men that could read and write was lower than for native-born men (81.6% versus 98.9%), native-born men with a foreign father had a higher literacy rate than native-born men with a native (and white) father (99.8% versus 98.2%) (Dillingham, 1911, p. 27).

were highly skilled and important innovators.[48] There are many examples of immigrants, who were involved in early industrialization in Europe, bringing over more advanced European technologies to the United States (Rosenberg, 1972). It has also been argued that the increased availability of unskilled labor due to immigration facilitated the introduction of technological and managerial innovations, such as assembly lines and the rise of the managerial firm (Abramovitz and David, 2000, Chandler, 1977, Denison, 1974, Hirschman and Mogford, 2009, Hounshell, 1984, Wright, 1990). Others have argued that the increase in the labor force enabled economies of scale in production, leading to increased profits that spurred innovation (Carter and Sutch, 1999).

As a test for whether innovative activity was affected by European immigration in the short-run, we examine patenting rates from 1860–1920, using utility patent data that were obtained from the *United States Patent and Trademark Office*. Estimates are reported in column 3 of Table 10. We find a positive and significant effect of immigration on innovation during this time. An increase in historical immigration from zero to the 50th percentile (0.049) results in a 0.7% increase in the number of patents per capita.

To assess the extent to which this increase in innovation is due to immigrants innovating themselves or due to their facilitating innovation by native-born Americans, we attempt to identify the country of birth of the innovators in the patent applications. The main challenge is that the citizenship of patent applicants was not consistently reported prior to 1880. Consequently, we are only able to identify the citizenship of the patent applicant in 50% of our sample of 1,297,086 applications. Moreover, per the Naturalization Act of 1798, immigrants could become United States citizens after only fourteen years of residence in the country. Therefore, it is possible that several patent applicants are registered as U.S. citizens, despite their being foreign-born. Another concern is that there were significant challenges and costs associated with obtaining a patent, which might have placed recently-landed foreigners with a limited understanding of English at a disadvantage.[49]

With these caveats in mind, we estimate the effect of immigration on the rate of patenting by

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[48]In fact, recent evidence suggests that immigrants coming from Western European countries, were, if anything, more skilled than the average of the home-country's population (Wegge, 2002, Long and Ferrie, 2013, Abramitzky and Boustan, 2015).

[49]While the Patent Act of 1793 might have benefited foreigners by removing the requirement of a thorough oral examination as part of the process of granting patents, the cost of a patent was $35 in 1861, which corresponds to about $891 in 2010 USD. Note, however, that the 1869 Report of the Commissioner of Patents compared the $35 fee for a U.S. patent to the significantly higher charges in European countries such as Britain, France and Russia ($450); Belgium ($420); and Austria ($350).

inventors that report themselves as being foreign-born. The estimates are reported in column 4 of Table 10. We find a positive and statistically significant effect of immigration on foreign patents. However, the magnitude is much smaller than for total patents. According to the estimates, an increase in historical immigration from zero to the 50th percentile (0.049) results in an increase in foreign patenting by 0.01%. This suggests that the direct effect of immigrants on foreign patents was lower than the indirect effect of immigrants on innovation by native-born inventors. Such an indirect effect of immigrants on native inventiveness is consistent with the findings of Moser, Voena and Waldinger (2014). Although the authors examine a slightly later period than our analysis (post-1920), they show that innovations by German-Jewish immigrants had a significant effect on the rate of innovation of U.S.-born inventors.

A closer analysis of the types of patents that tended to be registered by European-born inventors suggests that while they were fewer in number, it is possible that many of these patents represented contributions that were particularly important for industrialization. The importance of their contribution can be inferred by relative citation rates. Of the patents in our sample, 16% are cited by patents in the *NBER Patent Citation Database*, which contains patents from 1975–1999. Among the cited patents, 12% are historical patents held by individuals that are European-born, a figure that is significantly higher than the share of all patents that are registered by European-born inventors (which is 3%). Thus, while European patents may have been small in number, they may have been disproportionately influential.

D. *Examining Effects Over Both the Short- and Long-Run*

Our analysis to this point has provided estimates of the long-run economic effects of immigration, as well as for the short-run effects of immigrants on industrialization, agricultural productivity, and innovation. We now attempt to connect the short- and long-run effects by examining the full range of effects from immediately after the Age of Mass Migration until today. To do this, we examine urbanization, which has the benefit of being positively associated with income and is available at regular time intervals during our time span of interest. We use our IV strategy to estimate equation (3) with urbanization measured in each decade from 1920 to 2000 as the outcome of interest.

The estimates are reported in Table 11. We find that by 1920 one already observes a large positive effect of immigration on urbanization. This effect remains stable until about 2000, when it

Table 11: OLS and 2SLS estimates of the effect of historical immigration on urbanization.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Urban Population Share in:								
	1920	**1930**	**1940**	**1950**	**1960**	**1970**	**1980**	**1990**	**2000**
				A. OLS Estimates					
Average Migrant Share,	0.929***	0.940***	0.940***	0.890***	0.883***	0.887***	0.894***	0.941***	0.930***
1860-1920	[0.085]	[0.085]	[0.083]	[0.086]	[0.088]	[0.091]	[0.088]	[0.084]	[0.081]
				B. 2SLS Estimates					
Average Migrant Share,	5.173***	4.615***	5.050***	4.174**	4.995***	4.764**	5.088***	5.424***	6.234***
1860-1920	[1.714]	[1.663]	[1.783]	[1.690]	[1.939]	[1.917]	[1.960]	[2.017]	[2.222]
				C. First Stage Estimates					
			Dependent Variable: Average Migrant Share, 1860-1920						
Predicted Avg. Migrant Share,	4.423***	4.423***	4.423***	4.423***	4.423***	4.461***	4.423***	4.423***	4.423***
1860-1920	[1.369]	[1.369]	[1.369]	[1.369]	[1.369]	[1.370]	[1.369]	[1.369]	[1.369]
Kleibergen Paap F-statistic	10.43	10.43	10.43	10.43	10.43	10.60	10.43	10.43	10.43
Controls (in all Panels):									
Industrialization-Based Predicted Migrant Share	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date of RR Connection (Years as of 2000)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,935	2,935	2,935	2,935	2,935	2,933	2,935	2,935	2,935
Mean of Dep. Var. (2nd-Stage and OLS)	0.195	0.219	0.236	0.286	0.324	0.345	0.362	0.365	0.401

Notes: An observation is a county. Panels A and B reports OLS estimates and 2SLS estimates, respectively. Panel C reports the first-stage estimates from the 2SLS. Coefficient estimates are reported, with Conley standard errors reported in square brackets. ***, **, and * indicate significance at the 1, 5 and 10% levels.

increases slightly.[50] Thus, the estimates indicate that the economic benefits of immigrants were felt early and persisted over time. This is consistent with immigration affecting early industrialization, which due to increasing returns or lock-in effects, cause a persistent and long-run increase in urbanization.

Unfortunately, unlike urbanization, the other measures are not available during the full time span. For education and per capita income, we can examine how the effects evolve over time, but only in the post-WWII era. These estimates, which we report in appendix Tables A17 and A18, show that we observe the same basic trend for education and income as we do for urbanization. In the medium- and long-runs, we see that the effects of immigrants persist over time. For both outcomes, we find that the benefits not only persist, but also grow overtime.

Our findings of a persistent, and even growing, effect of historical immigration on economic outcomes is consistent with the recent findings from Bleakley and Lin (2012), who find evidence of lock-in effects in the context of historical U.S. portage sites. We find, as they do, that historical factors affected the initial locations of economic activity, which generated persistent and even

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[50]We also continue to find evidence that is consistent with the negative selection of immigrants. The 2SLS estimates are consistently larger in magnitude than the OLS estimates.

diverging differences in incomes across locations.

8. Conclusions

The goal of this study was to make progress on understanding the long-run effects of large-scale immigration. We examined the effects of the largest wave of immigration in U.S. history, the Age of Mass Migration, which occurred from roughly 1860 to 1920. To help identify the causal effects of immigrants on the locations in which they settled, we used an IV strategy that exploited the significant decade-by-decade fluctuations in aggregate immigrant flows to the United States that occurred during this era, the fact that immigrants typically used the railway to travel to their eventual destination, and the gradual expansion of the railway network over time.

We find that immigration resulted in large long-run economic benefits. Counties with more immigrant settlement from 1860 to 1920, now have higher incomes, less unemployment, less poverty, more education, and more urbanization. We also found that these economic benefits do not come at the cost of social outcomes. Places with more historical immigrant settlement today have similar levels of social capital, civic participation and rates of crime.

Throughout the analysis, comparisons of the OLS and 2SLS estimates revealed evidence consistent with negative selection by immigrants. The benefits inferred from the OLS correlations were always much smaller than the benefits inferred from the 2SLS estimates. This is consistent with migrants moving to "worse" places that counterfactually would have had lower long-run economic growth, causing OLS estimates to understate the positive effect of immigrants on long-term growth. The nature of selection is important since it may shed light into why casual observation often associates immigration with poorer outcomes, even when the true causal effects of immigrants may be positive.

It is possible that the long-run benefits to locations that received more immigrants came at the cost of other locations. The positive effect of immigrants could be due to a relocation of economic activity rather than the creation of economic activity. We directly test for the presence of such spatial spillovers. We estimated the effects that immigration to nearby counties had on a county. We found no evidence of immigration reducing economic prosperity in nearby counties (i.e., negative spillovers). Although we are unable to test for all possible forms of spillovers (e.g., spillovers between distant locations), the evidence suggests that the long-run benefits of

immigrants are likely due to the creation of economic activity, rather than to the relocation of economic activity.

To better understand mechanisms, we turned to an examination of the short-run effects of immigration. We found that immigrants resulted in an immediate increase in industrialization. Immigrants first contributed to the establishment of more manufacturing facilities and then to the development of larger facilities. We also found large positive effects of immigrants on agricultural productivity and innovation as measured by patenting rates.

Having examined the short-run effects of immigration, we then turned to an examination of the dynamic effects of immigrants over the short-, medium- and long-runs. Examining urbanization rates from 1920 to 2000, we found that large effects on urbanization were felt immediately and persisted over time. We also examined income and education, but for the more limited time period for which data are available (post WWII). We found a similar pattern for these outcomes as well.

Taken as a whole, our estimates provide evidence consistent with an historical narrative that is commonly told of how immigration facilitated economic growth. The less skilled immigrants provided the labor force necessary for industrial development. A smaller number of immigrants brought with them knowledge, skills, and know-how that were beneficial for industry and increased productivity in agriculture. Thus, by providing a sizeable workforce and a (smaller) number of skilled workers, immigration led to early industrial development and long-run prosperity, which continues to persist until today.

Despite the unique conditions under which the largest episode of immigration in U.S. history took place, our estimates of the long-run effects of immigration may still be relevant for assessing the long-run effects of immigrants today. According to our estimates, the long-run benefits of immigration can be large, and need not come at high social cost. In addition, the economic benefits can be realized quickly and can be highly persistent. This suggests the importance of taking a long-run view when considering the current immigration debate. Thus, as Abramitzky and Boustan (2015) have argued, we believe that looking backwards and learning from our past experience with immigration is important when moving forward and thinking about immigration policy today.

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