Immigrant franchise and immigration policy: Evidence from the Progressive Era*

Costanza Biavaschi† and Giovanni Facchini‡

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Abstract

What is the role played by immigrant groups in shaping migration policy in the destination country? To study this question we exploit historical variation in access to the franchise induced by different residency requirements across U.S. states. We start by documenting that naturalized immigrants were more geographically mobile than natives. Next we show that congressmen representing districts with large numbers of naturalized U.S. citizens were more likely to support an open migration policy, but that more stringent residency requirements attenuate this effect. Our results indicate that congressmen electoral accountability to naturalized immigrants was a key factor in explaining this outcome.

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†Norwegian University of Science and Technology and IZA. Address: Department of Economics, NTNU, N-7491 Trondheim, Norway. Email: costanza.biavaschi@ntnu.no

‡University of Nottingham, CEPR, CES-Ifo, CReAM, GEP and IZA. Address: School of Economics, University of Nottingham, University Park, Nottingham, NG7 2RD, UK. Email: giovanni.facchini@nottingham.ac.uk.

1 Introduction

As immigrants settle in destination countries, the share of foreign born citizens and their electoral role are poised to increase. For example, the Pew Hispanic Center reports that Latinos will account for 40 percent of the growth in the U.S. electorate over the next two decades. By 2030, 40 million Latinos will be eligible voters, up from 23.7 million today. Being able to cast the ballot is likely to boost immigrant's civic engagement and assimilation. At the same time, naturalized citizens are likely to exhibit different political preferences compared to the native born: in 2013, 80% of naturalized U.S. citizens identified immigration reform as a salient issue, while this was true for only 57% of second generation immigrants and 49% of U.S. natives.[1] To what extent do these differences play a role in shaping the immigration policy in the destination country? This question remains to date largely unanswered, and the goal of this paper is to address it by taking advantage of the unique experience of the U.S. during the Progressive Era. Several reasons make this the ideal context.

First, between 1900 and 1920 the number of long term migrants,[2] increased substantially from more than 7.8 million to more than 10.8 million. Second, immigration reform was as hotly debated then as it is today. Two decades of legislative initiatives culminated in the Immigration Act of 1917, which introduced the Literacy Test Provision. Third, this was a period of strong engagement with Congressional action on this matter. Four actors played a key role – organized labor, capital- and land-owners and the immigrants themselves (Goldin, 1994), but "[...] for the immigrants it was a matter of self-defense. Every symptom of reviving nativism aroused a fiercer, more militant immigrant opposition. Through individual appeals to the public opinion, through organizations, and through political pressure, the immigrants fought back."(Higham, 2002, p.123). Crucially, "Dozens of House members relied on immigrants for their seats" (Benton-Cohen (2018), page 50). Fourth, the Progressive Era was characterized by significant variation across U.S. states in the rules shaping access to the ballot in national elections, which can be exploited to identify politician's incentives to respond to the immigrant voting bloc. In particular, the naturalized immigrants' ability to affect policy makers' choices varied, depending on an array of factors, ranging from the sheer size of the foreign born population in the constituency (Goldin 1994, Shertzer 2016), to the extent to which immigrants were materially able to exercise the voting franchise. In fact, as argued by Keyssar (2009), stringent residency tests, requiring citizens to live in the same state often for up to two years, led to the disenfranchisement of large, mobile sections of the population, estimated in 1960 to represent approximately 8 percent of the eligible voters (Schmidhauser, 1963). The role played by residency tests is understudied, and as migrants were two to three times as mobile as natives during the Progressive Era,[3] we exploit this variation to analyze the impact of the presence of a large foreign born constituency on the making of migration policy.

[1] See the recent report by the Pew Hispanic Center available at: `http://www.pewresearch.org/fact-tank/2014/06/02/top-issue-for-hispanics-hint-its-not-immigration/`

[2] I.e. in the country for 10 or more years.

[3] See the discussion in section 2.

To the best of our knowledge this paper represents the first systematic exploration of the link between the preferences of the foreign born and voting behavior of US representatives on immigration policy, exploiting variation in restrictions to the electoral franchise. To this end, we construct a novel dataset combining roll call votes on immigration policy cast between 1897 and 1924, with a wealth of county level economic and demographic characteristics from the full count of the 1900, 1910 and 1920 population censuses,[4] and individual level information for the representatives, obtained from Congressional Directories. Data on voting requirements at the local level were instead obtained from Keyssar (2009). In creating this dataset, we face the challenge that congressional districts might span several counties, and at the same time, a county might be split into different districts. Following Dorn et al. (2020), we address this issue by focusing on county-by-congressional-district cells as the unit of observation, an approach that allows us to introduce county fixed effects throughout the analysis, to capture a range of time invarying differences in local economic and political conditions.[5]

Our analysis delivers several interesting results. First, we find systematic evidence consistent with the idea that throughout the Progressive Era, foreign born Americans were in favor of keeping an open door policy: representatives elected in districts with a large share of naturalized citizens were less likely to support immigration restrictions than their counterparts for whom a foreign born constituency was less important. At the same time, whenever residency requirements were more stringent, the foreign born's ability to influence the behavior of U.S. congressmen declined, making them more likely to support restriction. Consistent with the fear of party managers at the time that proposing restrictions would "cost the immigrant vote" (Higham, 2002, p.104), we find evidence that the channel through which immigrant preferences affected policy choices was the electoral booth: congressmen were responsive to the immigrant constituency only if they were elected in a close race, or if they were not already ideologically committed to an open door policy.

Our benchmark specification is a fixed effect model, controlling for a variety of county and individual representative characteristics. Still, we are concerned that our findings might mask the impact of unobserved, time varying determinants that could affect both the presence of a large foreign born constituency and the voting behavior of the elected representative. To make progress on causal identification, we start by showing that is no evidence of naturalized migrants sorting into districts where representatives were more open to immigration. Next, we complement our baseline analysis with an instrumental variable strategy which exploits the idea that immigrants tend to settle in locations where earlier migrants from the same origin country have established themselves, as recently used in Mayda et al. (2021). Using this methodology, we confirm the sign and significance of our baseline findings. Additionally, we also provide a detailed discussion of the validity of the instrument in our context. For example, we show that the IV results are unaffected once we also control for the potentially

[4]Individual level data for the 1890 census are not available. As explained in Section 3, our dataset comprises of county-district cells that we weight in the regression in such a way to resemble district level aggregates. The construction of these weights required us to keep track both of counties split across multiple districts, and of periodic redistricting. For more details, see section 3.

[5]Congressional district boundaries, on the other hand, change after each decennial census. It would therefore not be possible to include district fixed effects.

different trajectories between districts with a high versus a low share of naturalized migrants as suggested by Borusyak et al. (2018) and Goldsmith-Pinkham et al. (2018); in addition, we also show that our findings are unaffected even when we deviate from the IV assumption of perfect excludability, following the approach proposed by Conley et al. (2012).

This paper is related to two main strands of literature. First, it contributes to the analysis of the political economy of U.S. immigration policy. Much of the existing quantitative studies in this tradition have focused on the post 1965 era, emphasizing the role played by both economic (Gonzalez and Kamdar 2000, Fetzer 2006, Facchini and Steinhardt 2011) and non-economic factors at the congressional district level (Gimpel and Edwards 1999, Milner and Tingley 2011). Few papers have instead analyzed the role played by local level drivers in earlier periods, and in particular during the Progressive Era. One important exception is represented by Goldin's (1994) pioneering contribution, which highlights the role played by shifting coalitions in shaping the introduction of the literacy test provision in 1917. Limited data availability constrained though her quantitative analysis of the determinants of congressional behavior to the subsample of representatives elected in U.S. cities, at a time when a large fraction of the voting population still lived in rural areas. For our analysis we have instead used individual level data from the U.S. census and detailed geographical information linking counties to congressional districts to construct new county-district level cells, which allow us to explore the role played by a rich set of factors affecting the voting behavior of *all* U.S. representatives, while still accounting for time invarying county level unobserved characteristics through fixed effects. Moreover, our rich census data allow us to identify more precisely the role played by a key set of actors, namely naturalized foreign born citizens. In a related paper, Tabellini (2020) studies the impact of recent immigration in the Progressive Era on natives' political views, whereas our focus is instead on the political accountability of elected policymakers to the naturalized foreign born, a constituency of growing significance in this period.[6]

Our paper is also related to the literature studying how the presence of legislated obstacles to the voting franchise affects the voting behavior of different subgroups of the population, and specific policy outcomes. Several papers have focused on the effects of poll taxes and literacy test provisions on the disenfranchisement of black and poor voters (Kousser, 1974, Naidu, 2012), while others have also investigated whether the removal of these restrictions had important policy consequences (Husted and Kenny, 1997, Cascio and Washington, 2014, Bernini et al., 2016). To the best of our knowledge little is known instead about the impact of residency requirements. One of the goals of this paper is to fill this gap, by focusing on the effect that such measures had on a highly spatially mobile subset of the population, namely naturalized immigrants.

The remainder of the paper is organized as follows. Section 2 provides a broad historical overview of international and internal migration during the U.S. in the Progressive Era, and of the access to the franchise for naturalized foreign born. Section 3 introduces the data used in

[6]In another recent paper Mayda et al. (2021) find that between 1990 and 2010 counties exposed to greater inflows of high (low) skilled immigrants experienced a decrease (increase) in support for the Republican party, the political force associated to a preference for more restrictive immigration policies in that period.

the analysis, whereas section 4 presents our benchmark results. Section 5 addresses possible threats to identification our spatial discontinuity design and section 6 presents a series of robustness checks. Section 7 concludes.

2 Immigration and the franchise in the Progressive Era

In this section we introduce the historical context of immigration policy making in the U.S. at the turn of the twentieth century. We start by providing an overview of international migration to the country, and we present next the legal context shaping immigrant's access to the franchise, focusing on residency requirements. We review then the patterns of interstate mobility of both migrants and natives and finally we provide a short overview of the legislative history of immigration policy making.

International migration. Between 1890 and 1920 nearly 18.7 million foreigners entered the U.S., or an average of 622 thousand migrants per year (Carter et al., 2006). Return migration was widespread (Taylor and Williamson, 1997, Bandiera et al., 2013), and as a result, the stock of foreign born grew only from 9.2 millions in 1890 to 13.9 millions in 1920, representing respectively 14.8 and 13.2 percent of the total population. Importantly, increasing numbers of immigrants settled permanently, becoming entitled to apply for citizenship:[7] by 1900, 7.9 million foreign born individuals had lived in the U.S. for more than 10 years, whereas by 1920 the corresponding figure reached 10.8 millions.

Turning to the human capital characteristics of the migrants, in 1900, 1910 and 1920 about 80% of the foreign born were able to read and write, against 90 to 94% of the natives.[8] Interestingly, naturalized citizens appear to be positively selected compared to other migrants, and their literacy rate is broadly similar to that of natives (see Appendix A and Figure A1 for details).

The geographical distribution of the foreign born varied substantially across different regions of the U.S. (see Appendix A and Figure A2 for details). By 1920 few immigrants lived in the South East; they represented instead a substantial share of the population in the North East, the region around the Great Lakes, California and some parts of Texas, reaching up to 30 percent of the total in some congressional districts. Naturalization rates varied also substantially. Consider for example district 11 in Wisconsin. In 1920, 23.5% of the population was foreign born, and 15.1% was made up by naturalized U.S. citizens - with a naturalization rate of approximately 65%. Turn now to Texas' district 15. In the same year 32% of its

[7] Throughout the Progressive Era, the citizenship acquisition process was substantially easier than it is today. In particular, any white male immigrant, who had spent a minimum of two years in the country, could file a declaration of intention to become a U.S. citizen ("first papers"). After being a resident for an additional three years, he could then take an oath of allegiance and file a petition of naturalization (filing the so called "second papers"), thus completing the process. Aliens could naturalize in any court of record; names and types of courts have varied during different periods of history, but include county supreme, circuit, equity, chancery, probate and common pleas courts and a few State supreme courts and Federal courts.

[8] The information collected in the census does not allow us to measure educational attainment more precisely. For a recent study on the role played by mandatory schooling in nation building in the U.S. during this period, see Bandiera et al. (2018).

population was made up by foreign born, but only 3% had naturalized (see Appendix A and Figure A2 for details).

Suffrage. Male suffrage was introduced with the 15th amendment to the Constitution, ratified in 1870. As argued by Keyssar (2009) though, the extension of the franchise was not a one–directional process, and throughout the second half of the nineteenth century, measures were introduced to limit access to the voting booth. In the South, poll taxes and literacy test provisions were targeted especially at Black voters. All over the U.S. another, less well known but equally effective tool to disenfranchise large swaths of the mobile population was the enforcement of residency requirements.

Defined in the first half of the nineteenth century, the notion of residency was aimed at capturing both the physical presence in a community and the "intention" to remain therein for what the courts ended up describing as "an indefinite period of time". Jurisdiction on the matter was left in the hands of individual states, and this principle was re-affirmed by the Supreme Court in 1904. Residency requirements to exercise the right to vote typically involved a minimum amount of time spent not only in the state, but also in the county and in the electoral precinct where the citizen wanted to cast his ballot. While one year was a fairly common requirement at the state level, substantial variation existed with some Southern states requiring a two-year stay, whereas some states in the Midwest chose instead a shorter period of six months (Keyssar, 2009). More variation existed at the county and precinct level, ranging from a few days to a year (see Table A1 in Appendix A for more details).[9]

As pointed out by Keyssar (2009), two main arguments were put forward by those in favor of these measures: first, voters needed time to become interested in local politics, and to identify with the interests of the local community; second, citizens of any precinct should be able to "protect themselves" against a floating population, made up by short term residents temporarily relocated in a certain area for employment reasons. In particular, great concerns existed with regard to election fraud (Schmidhauser, 1963). The result of these measures was that mobile Americans were disenfranchised in large numbers. On the eve of the Civil Rights era estimates suggest that 8 million people – out of a total of 104 million adult citizens of voting age – were kept from voting in the 1960 elections (Schmidhauser, 1963). Lengthy

[9]By the 1880s, most states had some form of voter registration in place (Harris, 1929, Argersinger, 1992) and registration requirements became more stringent at the turn of the century (Perez, 2014). Although rules varied substantially across states and over time, the length of residence in the state, city or county and precinct was usually self-reported by the individual seeking registration in a voter list (Harris, 1929). The information was covered by an oath or affidavit, which was made in front of a registration clerk. In large cities, including New York, Chicago, St. Louis, Kansas City, Milwaukee, Omaha and Detroit, the information given was cross-checked by registration officers or the police with house-to-house canvass, made prior important elections (Harris, 1929). In other areas such as Massachusetts, Connecticut or Baltimore a full census of all adult residents was carried out and, upon completion, the list of residents would be checked against the registration records by the election office. Voters who were not found would be deleted from the registration list after public notice and opportunity for hearing. States had also in place different rules defining the procedures for a transfer of registration. In some states, the voter had to register anew (e.g. Chicago, St. Louis and Kansas City); in others, the voter needed to provide a certificate of cancellation from the previous precinct before registering in the new one (e.g. Maryland, New Jersey, Ohio, Idaho, Utah, Virginia and West Virginia); finally, in other areas (e.g. San Francisco, Detroit, Portland) it was the responsibility of the central office for registration at the new address to ask for cancellation from the previous list (Harris, 1929).

residency requirements were removed only in 1972 by the Supreme Court, arguing that they violated the equal protection clause.

Internal mobility. As it was pointed out already by de Tocqueville (2003), Americans are an extraordinarily mobile people.[10] Measuring internal mobility during the Progressive Era is not an easy task. In particular, the US census only asked information on the place of birth, but did not ask a question on the previous place of residence. Hence, internal migration information cannot be obtained through responses to direct migration questions in the census, as in later periods.

To shed light on the mobility patterns of natives and migrants we adopt two alternative strategies: first, we infer net migration indirectly through the analysis of population data in successive censuses; second, we gather more detailed information at the individual level using the linked census dataset recently made available by Abramitzky et al. (2014).

Our first strategy derives net migration as the difference in the mortality– and natality–adjusted population increments between two successive census enumerations, and allows us to compare estimates of the mobility rates of natives and naturalized foreign born citizens. For native born men, we collect population information by state at time t and predict the surviving population at $t + 10$ using survival rates which we have digitalized from the U.S. Census' vital statistics report for the period 1900-1920. Any observed difference between the predicted population and the actual population at $t+10$ will stem from internal mobility. Calculating net internal migration for naturalized citizens is further complicated by the fact that the population increment for this subgroup includes the increase due to new naturalizations occurring between t and $t + 10$, and by permanent emigration. To account for this, we have used the number of new naturalizations in each State obtained by digitalizing information from the Annual Reports of the Immigration and Naturalization Services and additionally the number of foreign born who permanently left each state. A detailed discussion of the method adopted is reported in Appendix B.

[Insert Table 1 here]

Panel (a) of Table 1 shows estimated internal migration rates for these groups. One key interesting stylized fact emerges. Naturalized immigrants are substantially more likely to move than their native counterparts according to all our measures: between 1900–1910 and 1910–1920, 15% to 19% of the naturalized foreign born changed state of residence. These rates are about three times higher than those of natives.

Our second approach uses instead the linked census dataset recently made available by Abramitzky et al. (2014) and allows us to track the individual relocation decisions of both natives and migrants, but we are not able to separately identify naturalized citizens. In particular, using first and last name, age and country or state of birth the authors are able to follow male individuals from 16 origin countries, who were aged 18-35 in 1900, across

[10] Much of the early mobility documented by de Tocqueville (2003) was related to the presence of a westward moving frontier – whose long-run implications have been studied in a recent paper byBazzi et al. (2020).

three different censuses (1900, 1910 and 1920), and compare them to a sample of native born males.[11] Building on this dataset and using information on the state of residence of the individual, we can construct a measure of intercensual mobility, and compare native born and migrants. Note that this dataset does not allow us to infer mobility rates of the naturalized immigrants. Our results are reported in panels (b) and (c) of Table 1. Panel (b) illustrates the simple unconditional probability that an individual observed in the 1900 (respectively 1910) census has changed his state of residence ten years later. In panel (c), we report instead the predicted probability resulting from a probit model in which we control for a full set of age and year of immigration dummies and country/state of birth. As before, immigrants are substantially more likely to move than natives.[12] Additional insights are reported in Appendix A, where we show that relocating migrants were distributed across a large number or states, while natives mostly moved to the western states.

Immigration policy legislation. Throughout the nineteenth century, European migration to the U.S. was virtually unopposed. By the 1890's several forces started to coalesce around the idea that an intervention to curb European migration was needed (Goldin, 1994). The Republican Party made the issue part of its platform for the 1896 presidential election, focusing explicitly on the need for a literacy test provision.

The first version of the literacy test to see full congressional action was contained in H.R. 7864, and the bill cleared the House on January 27, 1897, with a close margin of 135 vs. 123, and 97 abstentions. The excludable class of illiterates was defined as "All persons physically capable and over 16 years of age who can not read and write the English language or the language of their native or resident country..." Some exceptions were made for elderly parents of qualified immigrants. The Senate amended the bill and introduced an exception also for "a wife or minor child not so able to read and write". The conference report cleared the House on February 9 with a large majority. On March 2 the bill was vetoed by President Cleveland, who questioned literacy as a basis for selection, and more generally the bill's restrictive attitude. The House voted to override the presidential veto on March 3 but as the Senate took no further action, the bill died.

[11] For more details concerning the sample construction, see the Appendix section of Abramitzky et al. (2014).

[12] Between 1900 and 1920, 50% to 75% of all immigrant arrivals occurred through the port of Ellis Island in New York (Bandiera et al. 2013). As a result, one might wonder whether the high internal migration rates that we have uncovered are driven by outmigration from New York. To address this concern, we have estimated the same models of Table 1 excluding the sample of migrants and natives who resided in the State of New York in 1900. The stylized facts are unaffected. In other words, the observed high mobility rates are not driven by arrivals into Ellis Island and subsequent movements out of New York City. The results based on the longitudinal data of panel (b) and (c) show mobility rates for natives that are higher than those obtained from the demographic accounting exercise presented in panel (a). Note that there a few important differences in the structure of the population considered in the two approaches. In the longitudinal sample, the focus is on the mobility of young male adults (i.e. individuals aged 18-35 in 1900), whereas in the demographic accounting exercise, we consider a much larger population - i.e. individuals aged 27-67 in each Census – that includes both females and older individuals, who are typically less likely to move. Moreover, in the construction of the longitudinal dataset, men with uncommon names are more likely to be successfully linked between Censuses, and the commonness of one's name could potentially be correlated with socio-economic status. Abramitzky et al. (2014) show that average occupational-based earnings for the native population in their dataset are non distinguishable from the average occupational-based earnings in the full population. Yet, for our purposes this statistic is silent about the mobility patterns of this selected subgroup.

A literacy test provision was reintroduced in H.R. 12199, which was discussed in the House in May 1902. The bill included also a provision to increase the head tax to two dollars and extended the list of excludable aliens to include epileptics, people who have been insane over the past five years, anarchists and prostitutes. After several rounds of negotiations between the House and the Senate, the literacy test was dropped from the text, and the bill was approved by both Houses and signed into law as the Immigration Act of March 3, 1903.

With migration continuing to grow, President Roosevelt repeatedly called the House to act on the matter. The 59th Congress introduced a variety of bills to amend and strengthen the provisions of the 1903 Act, and H.R. 18673 saw full congressional action. Among its various provisions, there were tougher measures to keep out "undesirable" aliens and an increase in the head tax to four dollars. An attempt to introduce a literacy test provision failed, but the bill called instead for the appointment of a commission to investigate the immigration issue. The bill cleared both houses and was signed into law as the Immigration Act of February 20, 1907. The commission was appointed immediately thereafter under the chairmanship of Senator Dillingham. Over the next three years, it produced a massive 42–volume report, highlighting the existence of large differences between recent and earlier immigrants and calling for a great reduction in their number.

Based on the report's result, Dillingham introduced in 1911 a new bill (S. 3175), including both a further increase in the head tax to five dollars, and a literacy test provision. The House approved a modified version of the bill on December 18, 1912, but the Senate disagreed with several of the amendments. A conference was called and the final report was approved by the House on January 25, 1913. President Taft vetoed the bill though on February 14, pointing out that he could not agree on it because of the literacy test provision. An attempt to override the veto in the House failed by a narrow margin on February 19.

A literacy test provision was introduced once again in the subsequent Congress as part of H.R. 6060. After amendments by the Senate, the bill cleared the House on January 15, 1915, but was vetoed by President Wilson. His main motivation was the presence of a literacy test, and the denial of political asylum. The House failed to override the veto on February 4, and this represented the third and last successful presidential veto of the literacy test provision.

Representative Burnett reintroduced a literacy test provision in H.R. 10384 in the subsequent Congress. The bill included also a further increase in the head tax to 8 dollars, and was passed by the House with a very large margin on March 30, 1916. A similar version was passed also by the Senate. President Wilson vetoed the measure once again, but this time the veto was comfortably overridden by both Houses. The bill became the Immigration Act of February 5, 1917. Twenty years after its first introduction on the congressional floor, the literacy test provision finally became law.

In the aftermath of the first world war migrant inflows gained rapidly momentum, highlighting the inability of the literacy test provision to effectively curtail new arrivals. To address this concern, Congressmen Johnson introduced H.R. 14461, suspending immigration for a period of fourteenth months. The initiative was approved by the House by a large majority on December 13 1920, but it was vetoed by President Wilson. The drive to limit immigration

continued during the 67th Congress, and led to H.R. 4075, which included a provision to limit immigration to 3 percent of the "the number of foreign–born persons of such nationality resident in the U.S. as determined by the census of 1910", with some important exceptions.[13] The measure cleared both houses and was enacted by President Harding on May 19, 1921. The bill was initially supposed to be in effect until June 30, 1922, but was later extended until June 30, 1924 (by H. J. Res 268 of 1922).

As the deadline of June 1924 approached, new legislation was required to make limits to the additional inflow of immigrants permanent. Representative Johnson was once again an important player, introducing H.R. 7995, which further reduced the national quota to 2 percent of the 1890 population. It cleared the House on April 12, 1924, with a large margin. The bill was then sent to the Senate, which retained its basic provisions but reduced the quota exempt "relatives" categories to wives and children of American citizens. The bill went to conference, and as a result the 2 percent quota was to be replaced, after July 1, 1927, by an overall limit of 150,000 immigrants per year. A system of quota preferences was also introduced. The bill was enacted by the President on May 26, 1924.

3 Data

Our dataset draws on a number of different sources. We collect information on all legislative votes on migration policy that took place in the U.S. House of Representatives between 1897 and 1924, identified using the detailed historical account by Hutchinson (1981). We restrict our attention to final passage votes, which determine whether a bill clears the House or not. In doing so, we exclude votes on amendments.[14] Fourteen bills are included in our sample and Table C1 in Appendix C summarizes their main characteristics.

We use the VOTEVIEW database of Poole and Rosenthal (1997) to identify the congressmen's name, party affiliation, state of residence and congressional district. Importantly for our analysis, this dataset also contains a record of how individual congressmen voted on the relevant migration policy measures (roll call votes). Our dependent variable, $Vote_{icdt}$ takes a value of one if the congressman has voted in favor of a restrictionist measure. In our benchmark analysis, it takes instead a value of zero if the representative has either voted against the measure, or has abstained from casting a ballot.[15] The rationale for this choice is that each bill put forward was aimed at changing the existing status quo, and in a context in which the presidency was not keen to introduce a restrictionist measure, and repeatedly vetoed the bill approved by Congress, an abstention had an effect equivalent to a "no" vote. We discuss the robustness of our results with respect to the choice of dependent variable in Appendix E

[13] For more details, see Hutchinson (1981), page 180.

[14] We follow this approach to avoid including multiple decisions on the same legislation. Moreover, votes on amendments are often strategic and less likely to reflect the interests of a legislator's constituency. Nonetheless, we show in Appendix E (see in particular Table E1) that the results are unaffected if we also include non-final passage votes.

[15] For simplicity we have treated congressmen who decided not to vote (i.e. they abstained) and those who voted but did not express a preference (i.e. they voted "present") as equivalent. The latter group represents a very small share of Congress, averaging one percent over our time period.

(see in particular Table E1).

Two sets of drivers are used to explain the voting behavior. The first is a group of individual representative characteristics; the second is a set of county-by-congressional-district level features which we explain below. Information on representatives' age (Age_{it}), party affiliation ($Democrat_{it}$, allowing also for possible regional differences using an indicator for whether the representative was elected in a Southern state[16] $Southern\ Democrat_{it}$), ideological orientation (using the first component of the DW nominate score[17] $Nominate_{it}$) tenure in office ($Tenure_{it}$), educational attainment (i.e. whether he attended an Ivy League institution $Ivy\ League_{it}$) has been obtained from the VOTEVIEW database and ICPSR Study number 3371.

[Insert Figure 1 here]

To relate congressman behavior to their electoral district characteristics, we define the geographic unit of our analysis to be the county-by-congressional-district cell following Dorn et al. (2020). The empirical challenge that we face is that congressional districts might span several counties, and at the same time, a county might be split into different districts. Figure 1, depicting counties and congressional districts of the state of Massachusetts in the 62nd Congress, illustrates the type of challenges we have encountered. Consider for example Worcester county. Portions of it belong to Congressional districts 2, 3 and 4. If in turn we look at Congressional district 1, we can see that it encompasses Berkshire county in its entirety, and portions of Franklin, Hampshire and Hampden counties. We obtain county-by-district cells and in the analysis we weight each cell by its share of the population in the district, such that each district has equal weight in the analysis. Using the example of Massachussetts, this means that our data contain a separate observation for Worcester county-district 2, Worcester county-district 3 and Worcester county-district 4, while for example Berkshire county will only appear in district 1. To each one of these cells we then attach information on the voting behavior of the elected representative of the corresponding district, and we weight each cell with the cell's share of the population of that (part of) county in its corresponding congressional district. County-level data were obtained from the full count (i.e. 100% sample) of the decennial censuses.

We have encountered an additional difficulty in gathering county-by-district level data. Congressional district boundaries change over time, following decennial censuses and the incorporation of new territories in the Union.[18] To deal with the problem of redistricting, we have kept track of changes in the boundaries of the constituencies that occurred after each of the censuses of 1900, 1910, and 1920. After each census, we have updated the weights to take into account the new geography following the adjustments to district boundaries. By doing so, the voting behavior of each representative in our dataset will always relate to his

[16]Several studies of U.S. congressmen's voting behavior distinguish between Northern and Southern Democrats (e.g. Peltzman 1985). We follow Brewer et al. (2002) and define the South as including: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[17]The DW-nominate score increases is higher for more conservative congressmen.

[18]In particular, Oklahoma became the 46th US state in 1907, whereas Arizona and New Mexico joined the Union in 1912.

correct constituency, whose characteristics are obtained by weighting its county-by-district components by a weight that accounts for redistricting.

Our objective is to investigate how the presence of voting restrictions might affect the ability of this constituency to shape the local representative's voting behavior. In practice, to study this relationship, we have constructed our key explanatory variable – $High\ Foreign_{cdt}$ – as an indicator taking a value of one if the share of naturalized foreign born individuals in the total population of the district was sufficiently high as to play an important role in congressional elections. In the baseline analysis we have set the threshold at the 75th percentile of the distribution across districts in our sample period or above 7 percent. We have also experimented with alternative thresholds – see in particular Table 7 – confirming that the immigrant constituency needed to be sufficiently large in order to affect the representative's voting behavior on migration policy.[19]

Our second key explanatory variable, $Residency_s$ measures the residency requirements (in years) that had to be fulfilled to exercise voting rights in state s as of 1890, taken from Keyssar (2009). Our baseline measure uses only state requirements, but in robustness checks presented in Table 8 we have experimented with an alternative definition where we sum the requirements at the state, county and precinct level.[20] Descriptive statistics on $Residency_s$, and its other components at the county and precinct levels, are reported in Table A1 (Appendix A).[21]

To capture the role played by economic drivers at the district level in our baseline analysis, we use information on county-district GDP (GDP_{cdt}), calculated as the standardized predicted personal income in each county. County or State level data on GDP are available only starting from the 1930s. To circumvent this problem, we use a measure of GDP based on state personal income combining the widely used estimates in Easterlin (1957) for 1920, with the newly created estimates for the 1890-1910 period as provided Klein (2013). We apportion total GDP at the state level to each county using the share of the county population in the state. Next, we control for the (standardized) share of individuals employed in agriculture ($Agriculture_{cdt}$). Finally, we control for the cell's urban population ($Urban_{cdt}$).

Table C2 in Appendix provides summary statistics for the sample used in our analysis.

[19]During our sample period, the franchise was restricted to citizens 21 years and older, and universal female suffrage was mandated only by the Nineteenth Amendment in August 1920, even though a few states had introduced it before (Lott and Kenny 1999). Correspondingly, the universe of the citizenship status question in the Census includes males only in 1900 and 1910, and males and females in 1920. We are therefore unable to construct an exact measure of eligible foreign born voters, but as long as male and female naturalized migrants share a similar voting behavior our results will not be affected. Still, to assess the robustness of our findings we experiment in Table E3 (Appendix E) with several additional specifications. We start in column (1) by dropping observations based on the 1920 Census, where, unlike for previous decades, our definition of *High Foreign* is based on both male and female naturalized foreign born. As a second check (column (2)), we exclude all those states that had female suffrage prior to 1920. Third, in column (3), we include a dummy variable for having female suffrage and its interaction with year of congress fixed effects. By doing so we capture time specific differences in the voting behavior of representatives elected in states with and without female franchise. Our results are not qualitatively affected.

[20]Note that residency requirements do not vary within state, as they are set at the state level.

[21]Residency requirements were changed in our sample period. Alabama, Colorado, Louisiana and Virginia change their requirements for residency in the state; Alabama, Delaware, Missouri, North Dakota, Virginia and Wisconsin change their requirement at the county level. In our benchmark specification we do not use this variation that could be endogenous to the presence of foreign born. However, in Table 8 of section 6, we show that our results are unchanged once we use residency requirements that vary over time.

The first stylized fact that emerges is the broad support for restrictionist measures. Slightly more than sixty percent of the recorded votes were in favor of increasing barriers to the entry of foreigners. Turning to our main explanatory variables, approximately 24 percent of the congressional districts in our sample were characterized by a high share of naturalized foreign born;[22] as for the residency requirement to be eligible to vote, the average total time an individual was required to spend in the same state in 1890 was 1.02 years or about 375 days.

4 Empirical analysis

Our goal is to study the determinants of a representative's voting behavior on immigration policy measures introduced between 1897 and 1924. As highlighted by Higham (2002), migration policy was salient for recent migrants in this period – as it is today: "... Every symptom of reviving nativism aroused a fiercer, more militant immigrant opposition. Through individual appeals to the public opinion, through organizations, and through political pressure, the immigrants fought back."(p.123) and politicians were acutely aware of the important role played by recent immigrants in swinging elections (Shertzer, 2016). For this reason, in our empirical analysis we are interested in exploring the role played by the immigrant voting bloc – which will be affected by both the size of the foreign born population entitled to vote and by how easy it is to materially exercise the franchise. More specifically, we estimate the following linear probability model:

$$\begin{aligned} Vote_{icdt} = \quad & \beta_0 + \beta_1 HighForeign_{cdt} + \delta Residency_s \times HighForeign_{cdt} + \\ & + \boldsymbol{X}'_{cdt}\gamma_1 + \boldsymbol{Z}'_{it}\gamma_2 + \alpha_c + I_t + \epsilon_{icdt}. \end{aligned} \tag{1}$$

where $Vote_{icdt}$ is a dummy variable taking a value of one if representative i of county-district cd has voted in favor of a bill restricting migration at time t, and zero otherwise. $HighForeign_{cdt}$ is an indicator variable capturing the presence of a high concentration of naturalized U.S. citizens in the county-district cell – in our benchmark analysis it takes a value of one if it exceeds 7%, i.e. if the district falls in the top 75th percentile of immigrant receiving congressional constituencies in our sample period;[23] $Residency_s$ is our index of how stringent the residency requirements are in state s as of 1890; $\boldsymbol{X}'_{cdt}$ is a matrix of additional controls at the district level, and $\boldsymbol{Z}_{it}$ is a matrix of individual characteristics. All our specifications include a set of congress (I_t) and county (α_c) fixed effects to account respectively for common time varying unobserved shocks and county specific unobserved determinants. As explained in the previous section, all regressions are weighted by the population share of the cell in the district. We cluster standard errors by district and time, to allow for correlation in the migration policy stance of district-cells represented by the same congressmen.[24] The construction of the dataset based on county-district-cells allows including county fixed effects (α_c). This baseline specification can therefore reduce the concern that liberal counties might

[22] This is by construction.

[23] We experiment with alternative definitions in Section 6. In particular, see the results in Table 7 and 8.

[24] Clustering the standard errors at the state-congress, state-census decade or county level does not affect our results.

be more likely to attract or mobilize immigrants and, at the same time, elect pro-immigrant representatives. While we further discuss these concerns in Section 5, much of the unobserved variation driving immigrant sorting as well as representative's preferences should already be captured in this baseline specification.[25]

[Insert Table 2 here]

We start our analysis by investigating the average impact of a high concentration of naturalized immigrants and of residency requirements across all districts. Our results are reported in Table 2. Column (1) contains a parsimonious specification, in which we only account for the presence of a large naturalized foreign born community. Our results indicate that representatives of high immigration districts were more likely to support restrictionist policies, and generalize earlier findings in Tabellini (2020) to all migration policy votes held during the Progressive Era. This aggregate effect, however, is likely to mask the work of two competing forces. On the one hand, U.S. congressmen elected in districts with large naturalized foreign born populations could simply oppose immigration. On the other, this opposition could be the result of stringent residency requirements that in practice disenfranchised the more geographically mobile naturalized foreign born, making politicians in office not responsive to their preferences.[26] To shed light on this possibility, in columns (2) – (4) we allow the effect of łarge share of naturalized foreign born to vary between constituencies characterized by different residency requirements. Column (2) focuses only on this additional effect, whereas in column (3) and (4) we incrementally control for county level socio–economic characteristics and individual level attributes of representatives. Our results indicate that for high-immigration districts, the presence of stricter residency requirements increased the likelihood that the local Congressmen would support more restrictionist migration policies. As for the role played by county level determinants, we find that representatives elected in constituencies characterized by higher GDP or a smaller agricultural sector were less likely to support migration–restrictive measures. These estimates are consistent with Goldin (1994), who suggests that throughout this period industrialists were actively involved in supporting open immigration policies and with the arguments put forward by Tichenor (2002), who points out that agrarian interests, especially in the South, opposed more open immigration after 1900. We also find that congressmen elected in urban districts were less likely to support immigration restrictions, although these results are not statistically significant. These findings resemble previous results obtained for different eras by Gimpel and Edwards (1999) and Facchini and Steinhardt (2011). Finally, the results in column (4) – our preferred specification – indicate that congressmen affiliated with the democratic party are less supportive of restrictionist measures than their republican counterparts. Still, there are significant differences between southern democrats[27] and mem-

[25] As already mentioned, while counties have essentially maintained the same boundaries over time, district boundaries are updated after each census. It is therefore not possible to include district fixed effects.

[26] It is not uncommon that aggregate correlations between migration shares and political outcomes hide different and heterogenous effects. For example, Mayda et al. (2021) show that low and high skilled migration have an opposite effect on the share of votes for the Republican party.

[27] As argued for instance by Peltzman (1985) during this period Southern Democrats tend to be more conservative than their Northern counterparts.

bers of the same party elected elsewhere in the country, with the former being more likely to favor anti-immigration legislations (see Tichenor, 2002). On the other hand other characteristics such as age and whether the representative attended an Ivy League institution have no statistical impact on voting behavior. To summarize, our results provide robust evidence that representatives elected in high–immigration district tend to support less restrictive migration policies, but that this effect is moderated by the presence of stronger residency requirements that disproportionately target geographically mobile subgroups of the population.

How large are these effects? Consider for instance district 1 in Michigan and district 14 in New York State. Both have high shares of immigrants, but while Michigan applies total residency requirements that are less stringent than the national average at 0.5 years, the opposite is true for New York State, where the residency requirement in this period are stable at 1 year. Our analysis indicates that, other things being equal, the representative of Michigan's district is 15 percentage points less likely to support the restriction of immigration than the representative of New York State's district, and that this difference is significant at all conventional statistical levels.

Residency requirements mattered also for policy outcomes. Consider for example the vote on H.R. 10384 that took place in the House on February 1, 1917. This represents a pivotal moment in the history of immigration policy during the Progressive Era, as it resulted in Congress overriding President Wilson's veto, ushering in the literacy test provision. As shown in Table C1 in the Appendix, 309 members of the House supported the measure, with only 117 voting against it and 8 abstentions. Given the two-third supermajority required to override a presidential veto,[28] 289 favorable votes were needed. In Figure 2 we use our baseline estimates of column (4) in Table 2 to predict the number of votes that would have been cast in favor of the measure, absent the residency requirements. Strikingly, our results indicate that only 238 favorable votes would have been cast, thus preventing the passage of the literacy test provision by a significant margin.

The evidence we have uncovered so far is consistent with the idea that elected representatives are less likely to support open immigration – even in the presence of a large potential immigrant voting bloc who favors an open door policy – in those constituencies that made it more difficult for mobile Americans to exercise their voting rights. One possible, plausible explanation for this finding is that longer residency requirements reduced immigrants' ability to influence congressional elections, making them less likely to play a key role in choosing a representative, and thus decreasing the elected official's accountability to this constituency's preferences. If electoral accountability plays a key role in explaining these findings, we expect the immigrant bloc to play a bigger role in "marginal" districts, i.e. in constituencies that were narrowly won in the previous election, than in those that were carried with a large majority. At the same time, the behavior of politicians ideologically committed to an open immigration stance should be less likely to be affected by changes in the electoral power of the immigrant voting bloc than the behavior of politicians with a more ambiguous position on this matter.

[28]The requirement is set at two thirds of Members present, provided that there is a quorum.

[Insert Table 3 here]

We assess these arguments in Table 3, and the findings of the analysis are illustrated in Figure D1 in Appendix D. We start by interacting our key variables with an indicator for whether a cell belongs to a "marginal" district, i.e. a district that was won in the previous election with a narrow margin.[29] With the introduction of these interaction terms, the main regressors in our baseline analysis (e.g. $HighForeign$ and $Residency \times HighForeign$) capture respectively the effect of a large share of naturalized foreign born – with and without residency requirements – in safe districts. The interaction terms between the marginal district indicator and our main regressors capture instead the effects of these variables in constituencies won with a narrow margin in the previous election. Our results indicate that restrictions to the franchise in high immigrant districts had an effect on a representatives' voting behavior only if he was elected in a contested election. If he instead won a safe seat, more stringent residency requirements did not have a significant impact on their voting behavior.

Next, we allow the effect of a large foreign-born constituency – in the presence or not of residency requirements – to vary depending on the level of a representative's ideological commitment to an open immigration stance. In practice, we interact our key variables allowing their effect to vary between members of the Democratic party and other representatives, as members of the former were traditionally more committed to an "open door" policy (Benton-Cohen, 2018). Our results, reported in column (2) indicate that – once residency requirements were in place – Democratic representatives were 26 percentage points less likely than those of other parties to respond to changes in the electoral power of the immigrant voting bloc.[30]

Finally, we check whether the baseline patterns are explained by the immigration background of the representatives. To this end, in column (3) we include an indicator capturing whether the congressman was born abroad.[31] Our results indicate that foreign-born congressmen are less likely to vote in favor of restrictions but, at the same time, controlling for the country of birth of the representative does not affect our baseline estimates. This suggests that accountability to the electorate remains a key determinant of our findings.

5 Threats to identification

The main possible threat to the identification of the effects we are interested in is represented by the potential endogeneity of the share of naturalized migrants. The latter could arise for two reasons. First, immigrants might *sort* into counties where political conditions are more favorable, and in particular where representatives are more likely to align with their political preferences, that is where representatives are more open to the migration policy stance. If

[29] A marginal district is one in which the margin of victory was below two standard deviations from the average margin of victory of that congress. This corresponds to a margin of victory below 24%. We exclude representatives at large.

[30] The effect of $Residency \times HighForeign$ for other parties is in fact 0.362 percentage points, while for Democratic representatives the total effect of this variable is 0.1 percentage points, that is 0.26 percentage points lower than other parties.

[31] 4.62% of the representatives in the sample are categorized as having birthplace "abroad or unknown".

sorting were to be at work, we would expect our fixed effect estimates of the two parameters β_1 and δ to be upward biased.

Second, local party organizations might seek the political *incorporation* of immigrants through naturalization as a mean to form a winning coalition (Shertzer, 2016). If this is the case, we would expect immigrant mobilization to disproportionately occur in places where representatives are in favor of restricting immigration, as those are the districts where greater efforts would be needed to counterbalance restrictionist views. Endogenous political mobilization would then lead to a downward bias in the estimation of the two parameters β_1 and δ in our benchmark fixed effect model. The overall direction of the bias in the baseline results is therefore ambiguous.

We tackle the possibility of endogenous sorting based on political preferences in three ways. First, using contemporary survey data it has been extensively documented that Americans are not well informed about some basic characteristics of the representatives (e.g. Delli Carpini and Keeter, 1989, Snyder and Strömberg, 2010) elected in the district where they live. This lack of detailed knowledge is likely to apply *a fortiori* to migrants in the early twentieth century when deciding to relocate within the United States. Second, in all our regression we have introduced county fixed effects, which reduce these concerns as long as areas tend to be characterized by permanent differences in orientation and incentives to politically incorporate migrants. Third, we can directly study whether naturalized migrants sorted into districts where representatives were more open to immigration during the period studied in our analysis. In particular, we can test whether past voting behavior on restrictive immigration measures affects the likelihood that a county-district cell attracts large number of naturalized foreign born over the following decade. To this end, in Table 4 we regress future changes in $HighForeign$ on current voting behavior on restrictive migration bills. Our results indicate that there is no significant relation between these two variables.[32]

Next, to further alleviate concerns about the potential endogeneity of the share of naturalized migrants, we implement an instrumental variable approach to investigate whether unobserved time-varying characteristics at the local level – that might be correlated with the presence of naturalized immigrants as well as a representative's voting behavior – are not driving our findings. In particular, we deploy an instrument which builds on earlier work by Card (2001) and exploits the idea that immigrants tend to settle in locations where earlier migrants from the same origin country have established themselves.

Assume that the total number of immigrants in the U.S. from a given origin country does not depend upon the specific political conditions prevailing in a specific cell. We can then decompose the observed inflow of immigrants from a specific source country to a given cell into two components: an exogenous supply shock – based on the total number of immigrants to the U.S. from the given source country and the share of earlier immigrants from that country that live in that cell – and a residual component, reflecting short term fluctuations from the long term patterns. Card's shift-share instrument is based on the idea that the exogenous supply component represents the supply shifter that can be used as an instrumental variable. More

[32]See Appendix D fore more details on this exercise.

precisely, consider the share of naturalized migrants born in country o living in the United States at time t, nat_{ot}, and the share of those migrants living in the county-district cell cd, i.e. nat_{ocdt}. Moreover, let $\lambda_{ocd} = \frac{nat_{ocd1890}}{nat_{o1890}}$ be the share of the migrants originating in o living in district d in 1890.[33] To build our instrument, we re-apportion the U.S. wide figure of newly naturalized immigrants from country o at time t ($nat_{ot} - nat_{ot-1}$) proportionally to how the naturalized foreign born from country o where geographically distributed in 1890, that is, seven years prior the first vote in the House used in our analysis. Hence, our instrument is given by:

$$\text{Instrument}_{cdt} = nat_{cd1890} + \sum_{t=1900}^{1920} \sum_{o} \lambda_{ocd}(nat_{ot} - nat_{ot-1}) \tag{2}$$

The cell by year variation is therefore driven only by national-level trends in the number of naturalized immigrants by country of origin.

[Insert Figure 3 here]

Figure 3 provides a graphical illustration of the predictive power of our instrument. The panel highlights the presence of a strong, positive relationship between our measure of the supply driven naturalization rate and the actual share of naturalized foreign born.[34] We are now ready to use this measures for our analysis – and as with our baseline model, we create indicator variables which are equal to one whenever Instrument_{cdt} is in the 75th percentile of the distribution across districts in our sample period.

[Insert Table 5 here]

Table 5 reports the results from two different specifications. In column (1), we start by re-estimating our baseline model using our instrument. The high values of the two first-stage F-statistics for the excluded instruments reported at the bottom of the Table indicate that the instruments are strong.[35] The second stage estimates confirm our main findings: while representatives of districts with a high share of naturalized foreign born on average favored an open door policy, their behavior changed when the highly mobile foreign born were disenfranchised through residency requirements. Comparing the magnitudes of the OLS and the IV estimates, we can see that in absolute value the IV estimates are larger than those obtained with the fixed effect estimation – suggesting that if anything our baseline results might be downward biased due to endogenous mobilization.

Identification via shift-share instruments has recently received attention in the literature (Borusyak et al., 2018, Goldsmith-Pinkham et al., 2018, Jaeger et al., 2018, Adao et al., 2019). One concern that has been highlighted is that the instrument conflates short and long-run responses to immigration (Jaeger et al., 2018). This concern is valid in our context if one believes

[33] As the 1890 census got destroyed, we use the distribution of the naturalized immigrants who were in the country by 1890.

[34] Dropping from the analysis the few outliers present in the figure do not affect our results.

[35] The first stage results are reported and discussed in details in Table D1 in Appendix D.

that immigration flows could, on the one hand, decrease support for immigration restrictions in the short run and, on the other, trigger a long-term response in the opposite direction, for example due to an increase in nativist sentiments among the native-born population. To address these dynamic effects, Jaeger et al. (2018) suggest adding lagged immigrant flows to the baseline analysis, and instrumenting them with a lagged Bartik instrument. Given that our data have decadal frequency, we implement this idea by including one lag of the endogenous variable in our specification. Column 2 in Table 5 reports the results from this analysis. Our findings are similar to those obtained in column (1), indicating that immigration has a long-term effect on voting that is not different from its short-term impact.[36]

A second concern pointed out in the analysis by Borusyak et al. (2018) and Goldsmith-Pinkham et al. (2018) is that naturalization shares in 1890 might predict congressmen voting behavior through other channels, such as unobserved district characteristics that made an area attractive in the late 1800s and correlate to voting behavior and subsequent migration patterns in the early 1900s. We address this problem in two ways. First, we control for the interaction between decade and initial-period economic and political characteristics. In particular we interact indicators by decade with the share of Democratic votes in the initial-period election and share of employed in agriculture in the initial period. These variables aim to capture potentially different trajectories between districts with high versus low share of naturalized migrants. The results are reported in column (3) and (4) of Table 5,[37] and confirm our baseline findings.

More generally, we can relax the exogeneity assumption for the instrument and bound the effects following the approach proposed by Conley et al. (2012). In particular, let $\boldsymbol{X}$ be the matrix of endogenous variables[38] and $\boldsymbol{Z}$ the matrix of instruments.[39] Consider now the equation:

$$\boldsymbol{Vote} = \boldsymbol{X\beta} + \boldsymbol{Z\theta} + \boldsymbol{u} \tag{3}$$

The results reported in Table 5 impose $\boldsymbol{\theta} = \mathbf{0}$, in other words they assume no direct effect of the instruments on representatives' voting behavior, once the full set of controls is included in the regression. Conley et al. (2012) relax the assumption that $\boldsymbol{\theta}$ is exactly zero, and allow for some deviations from the exclusion restriction. In particular, following the local-to-zero approach proposed by Conley et al. (2012), we assume that the direct effect of the instruments on voting behavior – that is $\boldsymbol{\theta}$ – is distributed according to a normal $N(\eta, \eta^2)$. We focus on $\eta \geq 0$, as this implies – on average – an overestimation of the baseline effects in the OLS model, and we visualize bounds based on a range of possible values of η, i.e. $\eta = 0, 0.1, 0.2, 0.3$. Figure 4 shows bounds for the key effects of interest. The case $\eta = 0$ corresponds to the baseline results of the IV estimation, i.e. we are under the assumption that the exclusion restriction holds. By changing η we induce an increasingly large bias in our estimates. The figure shows that the sign and significance of the estimated parameters are

[36] See Appendix D1 for the first stage results.
[37] See Appendix D1 for the first stage results.
[38] I.e.$[HighForeign_{cdt}, Residency_s \times HighForeign_{cdt}]$.
[39] I.e. $[Instrument_{cdt}, Instrument_{cdt} \times Residency_s]$.

unaffected even at large deviations from perfect excludability, e.g. when the bias is as large as half of the main effect ($\eta = 0.3$).

[Insert Figure 4 here]

6 Robustness

We turn now to assess the robustness of our empirical findings by implementing a number of additional specifications. We start by investigating the mechanism through which restrictions of the franchise affected voting behavior on immigration policy; next we consider alternative definitions of our key explanatory variables and then the role played by additional controls. Finally, we summarize the results reported in Appendix E, where we carry out a range of additional analyses.

[Insert Table 6 here]

Our findings so far are consistent with the idea that residency requirements affected a representative's voting behavior on migration policy by having a disproportionate effect on naturalized Americans' ability to exercise the franchise. To provide further evidence for this mechanism, we carry out two placebo exercises, focusing on two subgroups of the population whose ability to affect policies was not directly shaped by the residency requirements: men aged 21 or less, and non–naturalized immigrants, e.g. two groups that were not allowed to vote in this period. The results of our analysis are reported respectively in columns 1 and 2 of Table 6 and confirm our expectations.

We turn now to assess the robustness of our main findings to different definitions of our key explanatory variables, starting with the indicator for high naturalized foreign born and turning next to alternative definitions of residency requirements.

[Insert Table 7 and Table 8 here]

In our baseline specification, a district was said to be characterized by a high proportion of naturalized foreign born if the latter's share fell above the 75th percentile of the distribution across our sample (i.e. it is above the 7 percent of the total population). In columns (1) to (3) of Table 7 we experiment with alternative thresholds. In column (1) we define as high immigration those districts that have a share of naturalized foreign born above the median. The results in this case are not statistically significant and the magnitudes much reduced. This confirms previous findings highlighting how the political power of immigrants was a function of their ability to join minimum winning coalitions (see Goldin (1994) and Shertzer (2016) for more on this issue). In column (2) and (3) we increase instead the threshold and include in the high-immigration districts all those that belong respectively to the 80th (the corresponding threshold is 7.5%) and and the 85th percentile (8.3%). Our main results continue to hold and in fact, the magnitude of the effects increases as the threshold becomes more restrictive. In column (4) and (5) we study instead whether more recent immigrants

were more in favor of an open door policy than their more established counterparts (Higham, 2002). To this end, in column (4) we replace our key explanatory variable with a dummy based on the share of naturalized foreign born who have been in the country for at least 10 years, and in column (5) we use a similarly defined indicator capturing whether the district is characterized by a high number of second generation migrants.[40]Our results focusing on long stayers are broadly in line with our baseline findings, while instead we uncover no effect of the disenfranchisement of the second generation on the representative's voting behavior. This suggests that over time migrants' preferences were perceived by elected politicians to be similar to those of native voters, providing direct evidence for the claim by Higham (2002) that, in the process of assimilation, an increasing number of immigrant descendants were leaning towards nativist sentiments.

Turning to the residency requirement, our main analysis used the number of years of residence in a state as of 1890. Yet, as shown in Keyssar (2009), residency requirements at the state level changed in four low–immigration states during our sample period.[41] In column (1) of Table 8 we show that using a time-varying definition of residency requirements does not affect our main conclusions.[42] Finally, residency requirements varied also at the county and precinct level. In column (2) we use an index that sums the total number of days of residence required in a state and county as of 1890, and in column (3) we experiment with an index that sums the number of required residency in a state, county and precinct as of 1890. Our findings remain stable also across these specifications.[43]

[Insert Table 9 here]

In Table 9 we assess the robustness of our empirical analysis to the introduction of additional controls, using the specification in column (4) of Table 2 as the benchmark. In that model we have accounted for a representative's party affiliation, age and educational attainment. In column (1) we further investigate the role played by the congressmen career path. We focus on his tenure in office and we find that it had no impact on support for immigration restrictions. In column (2) we control for the representatives' DW-NOMINATE score, to more precisely capture his ideological stance on economic matters. Accounting for this does not affect our baseline results. In columns (3)-(5) we introduce a set of additional district-level controls. Lee et al. (2004) argue that an elected representative's party affiliation is an imprecise proxy for a district's partisan leaning. Consequently, in model (3) we also control for the extent of party strength in the previous congressional election, but we do not find any significant effect. As argued by many observers, pressure groups have played an important role in shaping immigration policy making during the Progressive Era (Tichenor, 2002, Zolberg, 2006, Shertzer, 2016). In particular, Jewish and Catholic organizations have been at the forefront of the debate, typically lobbying in favor of maintaining an open door

[40]Second generation migrants are individuals with at least one parent born abroad.

[41]These states are Alabama, Colorado, Louisiana and Virginia.

[42]This finding is not surprising, as there is no correlation between the change in residency requirements and the share of naturalized immigrants. These additional results are available upon request.

[43]See Appendix Table A1 for a detailed breakdown of the residency requirements at the three different levels.

policy (Higham, 2002, p. 124). To account for their role, we have experimented by including the total share of Catholics and Jewish in the districts, taken from the Census of Religious Bodies (ICPSR 8), and report our results in columns (4). We find that a higher share of Catholics and Jewish is negatively related to restrictionist voting behavior, and this provides quantitative support for the existing historical narrative. At the same time, including these additional controls does not affect the findings of our benchmark analysis.

In columns (5) through (7) we further explore the role played by residency requirements on voting behavior. We start by investigating the impact of the disenfranchisement of native internal migrants in column (5),[44] but we find no statistically significant effect. We don't find any effect of residency requirements also for African Americans (column (6)). Importantly, including these drivers does not affect our main results. In column (7) we explore instead the role played by the diversity of the immigrant population. On the one hand, one might expect a more homogeneous migrant population to be able to exert greater influence on an elected politician. On the other, natives' views and perceptions of immigrants might be exacerbated by the presence of large, homogenous groups of foreigners. As a result, the effect of diversity on a representative's voting behavior is an empirical question. To capture its role, we introduce the indicator variable $HighDiversity$, which takes a value of one if the Birthplace Diversity index proposed by Alesina et al. (2016)[45] is larger than the 75th percentile of the distribution in our sample period. Our results in column (7) indicate that elected politicians tend to more actively oppose immigration restrictions in low-diversity areas rather than in high diversity areas and these effects are overturn once residency requirements are in place. On the other hand, while these effects persist also in high-diversity places, their magnitude is substantially reduced. These results are broadly compatible with the view that more diverse immigrant communities are less likely to be perceived as a threat by the native population.

To conclude, we report in Appendix E the results of a set of additional exercises in which we experiment with alternative definitions of our dependent variable, and with different sample structures. We briefly summarize here the most interesting findings, referring the interested reader to Appendix E for more details.

When we vary the definition of our dependent variable, for instance using a Heckman selection model to address the high abstention rates in congressional votes on migration policy bills, our main results are unaffected (see discussion in Appendix E of Table E1). In Table E2 we assess the robustness of our results to alternative samples. Rather than using the county-district cell dataset, which allowed us to introduce county fixed effects, we start in column (1) by presenting results for a district-level dataset. This is the unit of analysis used in previous studies (e.g. Baldwin and Magee, 2000, Facchini and Steinhardt, 2011, among others). District-specific data are not readily available for the period we are studying, and must instead be constructed by aggregating county-level data obtained from the full count

[44] During our sample period the only information available on internal mobility in the standard U.S. Census compilations concerns the state of birth. Thus, our indicator for High Natives internal migration focuses on the share of individuals living in a state different from the one where they were born.

[45] This index is defined as $Div_{pop} = 1 - \sum_i s_i^2$, where s_i is the share of the foreign born population originating in country i.

(i.e. 100% sample) of the decennial censuses. To obtain district-level data from county level information, we first extract county characteristics from the decennial censuses and then aggregate them at the district level. For those counties that were split across more than one district, we implement a "dasymetric interpolation" (Goplerud, 2016), following Baldwin and Magee (2000) among others, and construct weighted averages, which are based on the share of the population of a county assigned to that district. To deal with the problem of redistricting, we have kept track of changes in the boundaries of the constituencies that occurred after each of the censuses of 1900, 1910, and 1920. Our main findings are unchanged in this alternative analysis. Finally, we show the robustness of our results to the exclusion of vetoed bills and using the same sample as in Goldin (1994), and to the exclusion of the South and the Midwest, the two regions respectively characterized by the highest and lowest residency requirements.

7 Conclusions

In this paper we have studied the role played by naturalized U.S. citizens in explaining congressional support for restrictive immigration policy measures during the Progressive Era. Our analysis has delivered several interesting results. First, we find systematic evidence consistent with the idea that throughout the Progressive Era, foreign born Americans were in favor of keeping an open door policy: representatives elected in districts with a large share of naturalized citizens were less likely to support immigration restrictions than their counterparts for whom a foreign born constituency was less important. At the same time, as residency requirements became more stringent, the foreign born's ability to influence the behavior of U.S. congressmen declined, making them more likely to support restriction. Interestingly, we find evidence that the channel through which immigrant preferences affected policy choices was the electoral booth as congressmen were responsive to the immigrant constituency only if they were elected in a close race, or if they were not ideologically committed to an open door policy.

The immigration experience of many Western destination countries today resembles that of the U.S. at the beginning of the twentieth century. Over the past two decades large inflows have taken place and it is not uncommon to observe foreign born populations representing more than ten percent of the total. At the same time, lively debates are ongoing concerning the ease with which immigrants should be allowed to naturalize. On the one hand, the extension of the franchise is likely to favor the assimilation and integration of foreign born nationals, by making them more willing to carry out "country specific" investments in the host society (Cox and Posner, 2009). On the other, our analysis has shown that migrants preferences might well differ from the rest of the host country polity, and these preferences can in turn affect policy outcomes. This tradeoff will be crucial for the design of optimal integration policies.

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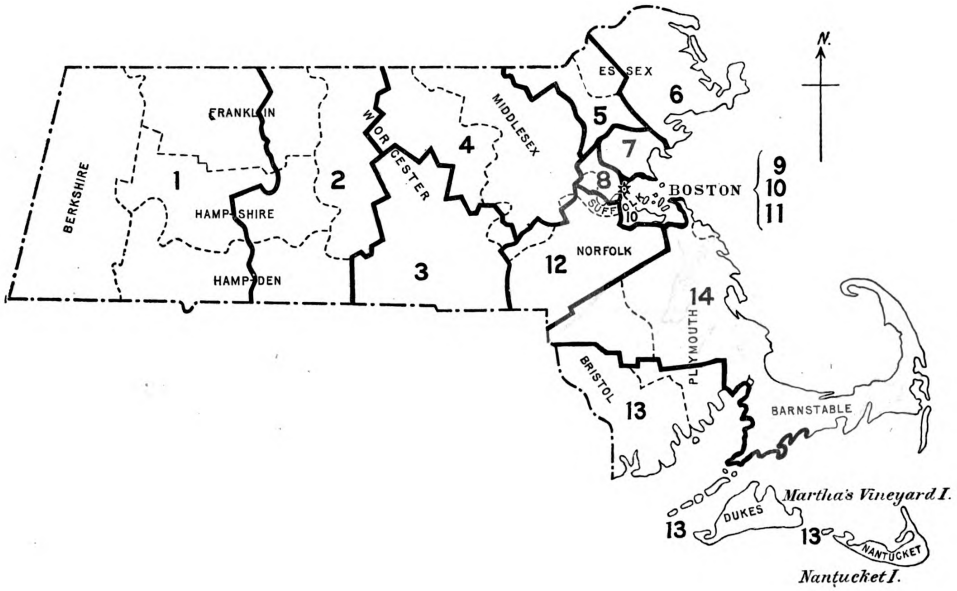
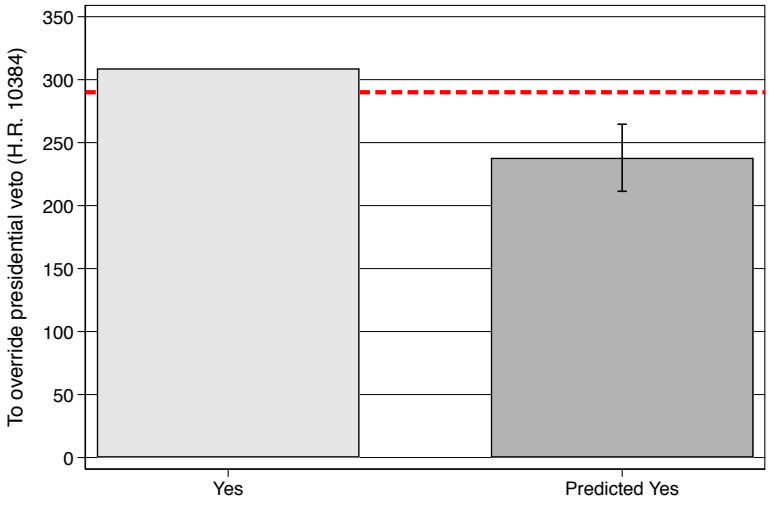
Figures

Figure 1: Counties and congressional districts



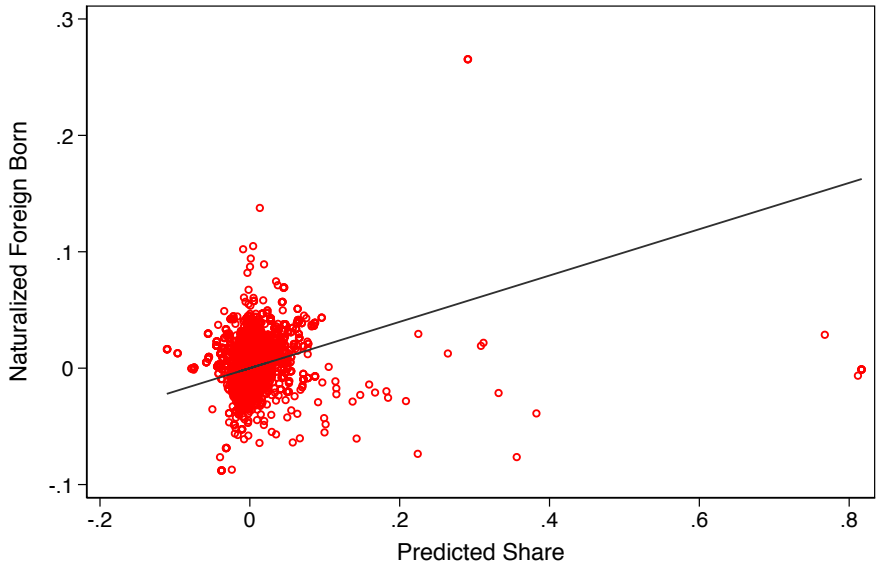
Source: Congressional Directory, Congress 62

Figure 2: Actual and predicted votes on H.R. 10384 on February 1, 1917



Notes: The chart shows the number of votes in favor of passing H.R. 10384 actually cast (left) and predicted (with 95% percent confidence intervals) to be cast if residency requirements were removed (right).

Figure 3: Partial correlations

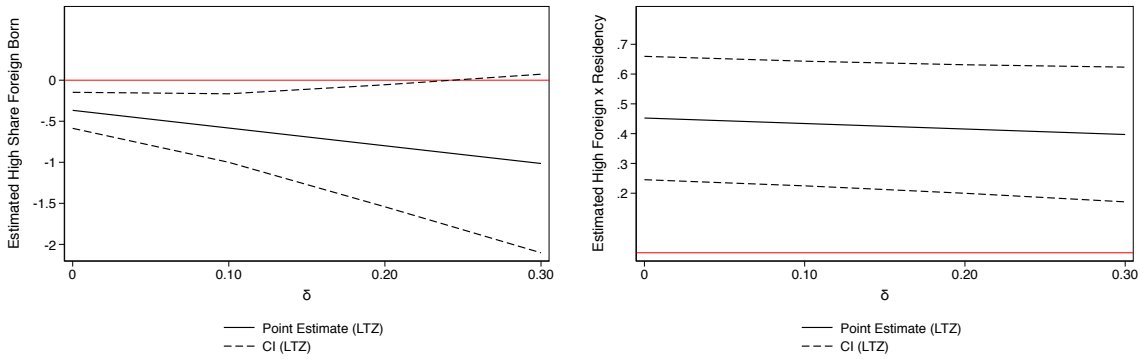


Notes: The figures report the partial correlations between the share of naturalized foreign born and the instrument.

Figure 4: Conley Bounds

(a) Effect on HIFB

(b) Effect on HIFB x Residency



Source: Both figures assume a Gaussian prior for θ in the local-to-zero approach of Conley et al. (2012), however varying the mean and variance. Bounds at each point on the graph are based on the assumption that $\theta \sim N(\eta, \eta^2)$, with $\eta = 0, 0.1, 0.2, 0.3$. The dotted lines indicate the 90% confidence intervals.

Tables

Table 1: Interstate Migration, by Nativity and over Time

	1900-1910	1910-1920
	(a) Aggregate Net Migration Rate (Survival Method)	
Native	0.0538	0.0394
Naturalized	0.1425	0.1709
Naturalized (adj.)	-	0.1863
	(b) Average Probability (Longtudinal data)	
Native	0.2413	0.2259
	(0.4280)	(0.4183)
Migrant	0.4024	0.4102
	(0.4904)	(0.4919)
	(c) Predicted Probability (Longitudinal Data)	
Native	0.2165	0.239
	(0.0203)	(0.0167)
Migrant	0.3956	0.4201
	(0.0676)	(0.0723)
Observations	20775	20836

Source panel (a): own calculation from the Censuses, see also Appendix B for details. The first panel shows average migration rates across cohorts, by nativity, based on a census survival method. *Naturalized (adj.)* adjusts the aggregate net migration rate for permanent emigration of naturalized migrants from the U.S. during the 1910-1920 period. See text and Appendix B for details. *Source panel (b):* data by Abramitzky et al. (2014). The second panel shows the average and standard deviation of an indicator that equals one if the state of residence in a Census differ from the state of residence in the previous Census. *Source panel (c):* data in Abramitzky et al. (2014). The second panel shows predicted probabilities and standard errors, from a probit model in which the probability of changing state of residence in two subsequent censuses is explained by a full set of age dummies, year of immigration dummies and country/state of birth dummies.

Table 2: Baseline specification

	(1)	(2)	(3)	(4)
High Share of Foreign-Born	0.140***	–0.100	–0.106*	–0.141**
	(0.032)	(0.063)	(0.063)	(0.062)
High Foreign x Residency		0.274***	0.272***	0.298***
		(0.069)	(0.070)	(0.069)
Urban			–0.103	–0.093
			(0.105)	(0.103)
GDP			–0.106***	–0.103***
			(0.020)	(0.018)
Agriculture			0.171***	0.155***
			(0.026)	(0.025)
Democrat				–0.261***
				(0.021)
Southern Democrat				0.302***
				(0.067)
Age				–0.001
				(0.001)
Ivy League				0.011
				(0.028)
Adj. R-Square	0.21	0.21	0.22	0.25
N	40617	40617	40617	40617

Robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
All columns include county fixed effects and congress fixed effects.
The dependent variable is a dummy variable which takes a value of one if the representative of cell *cd* has voted in favor of a bill restricting migration at time t, and zero otherwise. *Residency* measures the number of years of residency in the state as of 1890. *High Share of Foreign-Born* (and *High Share Foreign*) is an indicator that equals one if the share of naturalized citizens is above the 75th percentile of the distribution of our sample period. *Urban* represents the share of the population living in cities and incorporated places with 2,500+ inhabitants. *GDP* represents standardized predicted personal income in each county. *Agriculture* represents the standardized share of the population employed agriculture. *Democrat* is an indicator of the representative belonging to the democratic party. *Age* is the age of the representative at the beginning of the congress. *Ivy League* is an indicator of the representative having attended an Ivy League school.

Table 3: Electoral channels

	Marginal District	Democrats	Rep. Born Abroad
	(1)	(2)	(3)
High Share of Foreign-Born	0.059	–0.154**	–0.139**
	(0.117)	(0.065)	(0.063)
High Foreign x Residency	–0.201	0.362***	0.296***
	(0.156)	(0.074)	(0.069)
Marginal district × High Foreign	–0.215*		
	(0.111)		
Marginal district × High Foreign × Residency	0.519***		
	(0.152)		
Marginal district	0.028		
	(0.062)		
Democrat × High Foreign		0.053	
		(0.089)	
Democrat× High Foreign × Residency		–0.262***	
		(0.096)	
Born Abroad			–0.055
			(0.037)
Democrat	–0.256***	–0.254***	–0.262***
	(0.021)	(0.048)	(0.021)
Adj. R-Square	0.26	0.25	0.25
N	40468	40617	40617

All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
All columns include region trends, state and time fixed effects; columns (2)-(6) additionally include all controls of Table 2, column 4.
The dependent variable is a dummy variable which takes a value of one if the representative of cell *cd* has voted in favor of a bill restricting migration at time t, and zero otherwise. *Residency* measures the number of years of residency in the state as of 1890. *High Share of Foreign-Born* (and *High Share Foreign*) is an indicator that equals one if the share of naturalized citizens is above the 75th percentile of the distribution of our sample period. *Marginal district* indicates districts in which the margin of victory is two standard deviations above the mean of the sample period. We exclude districts with representatives at large, for which margin of victory are not available. *Democrat* indicates districts with a representative from the democratic party. This regression exclude indicators for being a Democrat in the South.
Rep. Born Abroad further controls for an indicator that takes value of one if the representative is born abroad.

Table 4: Reverse causality

	Dep. Variable: Δ High Foreign$_{cdt+1}$			
	(1)	(2)	(3)	(4)
$Vote_{icdt}$	0.007	0.007	0.003	0.004
	(0.010)	(0.010)	(0.009)	(0.009)
Residency	Yes	Yes	Yes	Yes
Urban	No	Yes	Yes	Yes
Economic Characteristics	No	No	Yes	Yes
Individual Characteristics	No	No	No	Yes
Adj. R-Square	0.78	0.78	0.79	0.80
N	29816	29816	29816	29816

Robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one. All columns include county fixed effects and congress fixed effects.
The dependent variable is a dummy variable that takes a value of one if the cell becomes a high-foreign born cell between t and $t+1$, i.e. Δ High Foreign$_{cdt+1}$. $Vote_{icdt}$ is a dummy variable which takes value of one if the representative i of cell cd has voted in favor of a bill restricting migration at time t, and zero otherwise. Economic characteristics include GDP and employment in agriculture. Individual characteristics include Southern Democrat, age and ivy league. See Table 2 for variable explanation.

Table 5: Instrumental variable regression

	Baseline	Dynamics	Agr. Trends	Pol. Trends
	(1)	(2)	(3)	(4)
High Share of Foreign Bon	–0.320**	–0.336**	–0.335**	–0.318**
	(0.133)	(0.145)	(0.133)	(0.131)
High Foreign × Residency	0.487***	0.298**	0.498***	0.470***
	(0.120)	(0.130)	(0.120)	(0.118)
Lag High Share of Foreign-Born		–0.111		
		(0.072)		
Lag High Foreign x Residency		0.311**		
		(0.128)		
Share Empl. in Agr. in 1896× Time f.e.	No	No	Yes	No
Dem. Share in 1896 × Time f.e.	No	No	No	Yes
F 1^{st} stage - High FB	223.796	95.750	206.194	205.981
F 1^{st} stage - High FB x Restr	239.132	102.355	193.463	191.165
Partial R^2 - High FB	0.606	0.161	0.167	0.164
Partial R^2 - High FB x Restr	0.596	0.218	0.233	0.230
Wooldridge test p-value	0.125	0.411	0.154	0.239
Adj. R-Square	0.09	0.08	0.09	0.09
N	40617	32554	40617	40569

Robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
All columns include county and congress fixed effects and all controls of Table 2, column 4.
The dependent variable is a dummy variable which takes a value of one if the representative of cell cd has voted in favor of a bill restricting migration at time t, and zero otherwise. All columns use as instrument the variable $Instrument_c dt$, as described in the text. Column (1) is a baseline IV regression. Column (2) includes the adjustment in suggested in Jaeger et al. (2018). Column (3) controls for the share of the workforce employed in agricolture in 1896, or the first year in which the district appears in the sample, interacted with census-decade fixed effects. Column (4) controls for the share of Democratic votes in 1896, or the first year in which the district appears in the sample, interacted with census-decade fixed effects.

Table 6: Placebo tests

	High Young	High Foreign Born Non-Citizen
	(1)	(2)
Placebo Variable	–0.044	0.098
	(0.058)	(0.068)
Placebo Variable × Residency	–0.049	–0.087
	(0.057)	(0.078)
GDP	–0.097***	–0.100***
	(0.019)	(0.019)
Agriculture	0.169***	0.170***
	(0.025)	(0.025)
Urban	–0.124	–0.133
	(0.104)	(0.104)
Democrat	–0.261***	–0.262***
	(0.021)	(0.021)
Southern Democrat	0.303***	0.305***
	(0.068)	(0.068)
Age	–0.001	–0.001
	(0.001)	(0.001)
Ivy League	0.016	0.016
	(0.028)	(0.028)
Adj. R-Square	0.25	0.25
N	40617	40617

All columns include county and congress fixed effects, and all controls of Table 2, column 4. All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
High Young is an indicator for the share of individuals less than 21 years old exceeding the 75th percentile of our sample period in each cell.
High Foriegn-Born Non-Citizens indicates a regression in which $High\ Foreign$ is an indicator that equals one if the share of non-citizens foreign-born in the cell is above the 75th percentile of the distribution for our sample period.

Table 7: Robustness checks: alternative definitions of foreign born

	50th Percentile	80th Percentile	85th Percentile	Long Stay	Second Generation
	(1)	(2)	(3)	(4)	(5)
High Share of Foreign-Born	–0.092	–0.100**	–0.083*	–0.119*	–0.126
	(0.072)	(0.042)	(0.046)	(0.064)	(0.100)
High Foreign x Residency	0.106	0.263***	0.223***	0.236***	0.152
	(0.092)	(0.045)	(0.047)	(0.070)	(0.116)
Adj. R-Square	0.25	0.25	0.25	0.25	0.25
N	40394	40394	40394	40617	40617

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4.
All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
50th Percentile defines *High Share of Foreign-Born* (and *High Foreign*) as an indicator that equals one if the share of naturalized citizens is above the 50th percentile of the distribution of our sample period. *80th Percentile* defines *High Share of Foreign-Born* (and *High Foreign*) as an indicator that equals one if the share of naturalized citizens is above the 80th percentile of the distribution of our sample period. *85th Percentile* defines *High Share of Foreign-Born* (and *High Foreign*) as an indicator that equals one if the share of naturalized citizens is above the 85th percentile of the distribution of our sample period. *Long Stay* defines *High Foreign* as an indicator that equals one if the share of naturalized citizens in the country for at least 10 years is above the 75th percentile of the distribution of our sample period. *Second Generation* uses indicators for the share of second generation migrants being in the 75th percentile of the distribution of our sample period and the interaction of this variable with Residency.

Table 8: Robustness checks: alternative definitions of residency requirements

	Time-Varying State Req.	State+County	State+County+Precinct
	(1)	(2)	(3)
High Share of Foreign-Born	–0.174**	–0.132**	–0.134**
	(0.086)	(0.064)	(0.064)
High Foreign x Residency	0.311***	0.238***	0.233***
	(0.089)	(0.058)	(0.057)
Adj. R-Square	0.25	0.25	0.25
N	40617	40617	40617

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4.
All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one. *Time-varying State Req.* indicates a regression in which residency requirements at the state level are time-varying during our sample period. *State*, *County* and *Precinct* indicate n. of years of residency required in the state, in the county and in the precinct.

Table 9: Robustness checks: alternative controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Share of Foreign-Born	–0.140**	–0.127**	–0.146**	–0.148**	–0.113*	–0.142**	–0.213***
	(0.062)	(0.061)	(0.065)	(0.062)	(0.063)	(0.062)	(0.082)
High Foreign × Residency	0.297***	0.288***	0.246***	0.302***	0.255***	0.299***	0.400***
	(0.069)	(0.068)	(0.071)	(0.069)	(0.075)	(0.069)	(0.087)
Tenure	–0.002						
	(0.002)						
DW-nominate		0.119**					
		(0.056)					
Share Democrat			–0.020				
			(0.064)				
Share of Catholic/Jewish				–0.330*			
				(0.171)			
Natives Internal Migration					0.046		
					(0.056)		
Natives Internal Migration× Residency					–0.094		
					(0.061)		
High Share of Blacks						0.120	
						(0.086)	
High Share of Blacks× Residency						–0.101	
						(0.092)	
High Diversity							0.041
							(0.036)
High Diversity × High Foreign							0.162*
							(0.097)
High Diversity × High Foreign × Residency							–0.282***
							(0.106)
High Diversity × Residency							–0.004
							(0.033)
Adj. R-Square	0.25	0.26	0.29	0.25	0.20	0.25	0.25
N	40617	40617	35223	40617	40787	40617	40617

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4. All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.

The dependent variable is a dummy variable which takes a value of one if the representative of cell *cd* has voted in favor of a bill restricting migration at time t, and zero otherwise. *Residency* measures the number of years of residency in the state as of 1890. *High Share of Foreign-Born* (and *High Share Foreign*) is an indicator that equals one if the share of naturalized citizens is above the 75th percentile of the distribution of our sample period. *Tenure* measures the cumulative years of service in the House as of the beginning of the congress. *DW-Nominate* is the representative's DW nominate score. *Share Democrat* is the share of votes casted to the democratic party in the last election. *Share of Catholics/Jewish* indicate the share of Catholics and Jewish in a district. *Natives Internal Migration* is an indicator that equals one if the share of natives in the district whose state of birth differ from the state of residence is above the 75th percentile of the distribution for our sample period. *High Share of Blacks* is an indicator that equals one if the share of blacks is in the 75th percentile of the distribution in our sample. *High Diversity* is an indicator that equals one if the diversity within the migrant population is in the 75th percentile of the distribution in our sample.

Online Appendices

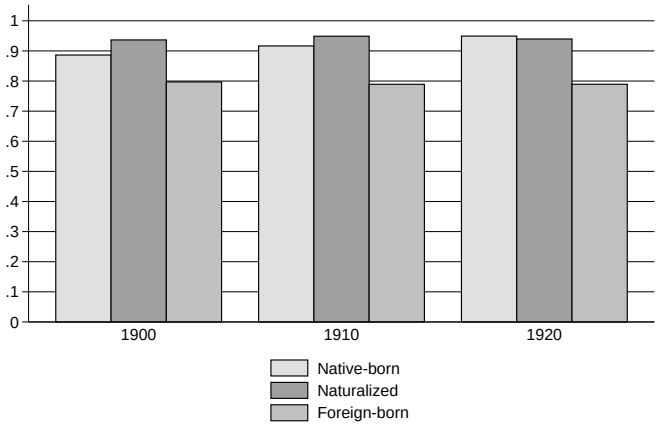
(For Online Publication)

A Additional Background

We report in this section several descriptive tables and figures on the characteristics of the foreign born citizens in the U.S.

Literacy Rates of Natives, Migrants and Naturalized Migrants. In Figure A1 we compare the literacy rates among the natives and foreign born in 1900, 1910 and 1920.[46] While it increased among natives from slightly less than ninety percent in 1900 to 94 percent in 1920, it remained constant at about 80 percent among foreign born. Interestingly, naturalized citizens appear to be positively selected compared to other migrants, and broadly comparable to natives.

Figure A1: Literacy rates, by citizenship, over time.



Source: Authors' calculations based on the 1900-1920 IPUMS Census data. The three censuses all had questions for whether the respondent was able to read or write. The instructions to the enumerators indicated to write "Yes" for all persons 10 years of age and over who could read any language, whether English or some other, and "No" for all persons who could not read any language. For persons under 10 years of age, the question were left blank.
Notes: Literacy is defined as being able to read and write. Native born are individuals born in the U.S., foreign born are individuals born abroad, individuals born abroad who have received first papers but have not naturalized yet. Naturalized individuals are foreign born who have taken U.S. citizenship.

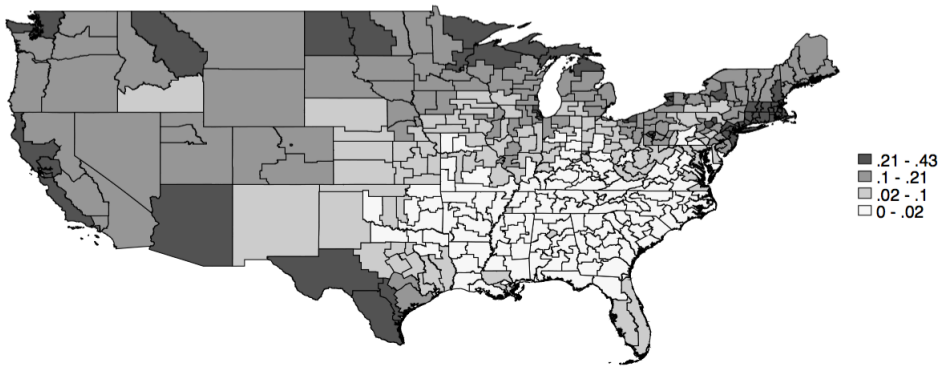
Geographical distribution of the foreign born. Figure A2 shows the geographical distribution of immigrants and foreign born citizens across US districts in 1920. Here it can be seen that few immigrants and few naturalized citizens lived in the Southern states. Naturalization rates varied substantially across geographic areas, as discussed also in section 2.

Suffrage. Table A1 shows the length of the residency requirement in each state and over time. As mentioned also in section 2, the total residency requirements (in the state, county

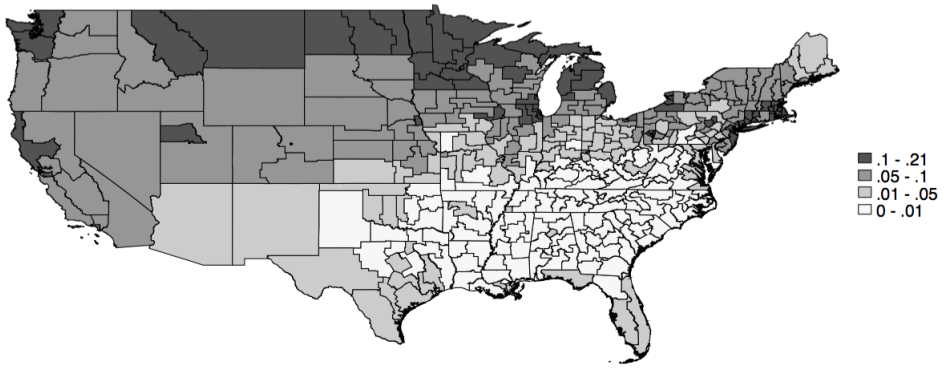
[46]The three censuses all had questions for whether the respondent was able to read or write. The instructions to the enumerators indicated to write "Yes" for all persons 10 years of age and over who could read any language, whether English or some other, and "No" for all persons who could not read any language. For persons under 10 years of age, the question were left blank.

Figure A2: Naturalized and non-naturalized foreign-born population in 1920

(a) Share of foreign born by district



(b) Share of naturalized foreign born by district



Source: Authors' calculations based on the 1900-1920 IPUMS Census data.

and precinct) varied from 0 days in New Hampshire to 1215 days in South Carolina. During the Progressive Era, the requirements were mainly strengthened.

Table A1: Residency Requirements, in Days, by State, County and Precinct.

State	Requirement in 1890, in days		
	State	County	Precinct
Alabama	365	90	30
Arizona	365	0	0
Arkansas	365	180	30
California	365	0	30
Colorado	180	90	10
Connecticut	365	0	0
Delaware	365	30	0
Florida	365	180	0
Georgia	365	180	0
Idaho	180	30	0
Illinois	365	90	30
Indiana	180	0	30
Iowa	180	60	0
Kansas	180	0	30
Kentucky	365	180	60
Louisiana	365	0	30
Maine	0	0	0
Maryland	365	180	0
Massachusetts	365	180	0
Michigan	180	0	0
Minnesota	180	0	90
Mississippi	730	365	0
Missouri	365	0	20
Montana	365	30	0
Nebraska	180	40	10
Nevada	180	30	0
New Hampshire	0	0	0
New Jersey	365	150	0
New Mexico	365	90	30
New York	365	120	30
North Carolina	730	180	120
North Dakota	365	180	90
Ohio	365	30	20
Oklahoma	365	180	30
Oregon	180	0	0
Pennsylvania	365	60	0
Rhode Island	730	0	0
South Carolina	730	365	120
South Dakota	365	180	30
Tennessee	365	180	0
Texas	365	180	0
Utah	365	120	60
Vermont	365	0	0
Virginia	365	90	0
Washington	365	90	30
West Virginia	365	60	0
Wisconsin	365	30	0
Wyoming	365	60	0

B Measuring Internal Mobility

To quantify internal mobility we have adopted two approaches: the first uses a residual technique and survival data applied to census enumerations (e.g. see Bell et al., 2002); the second is instead based on longitudinal data obtained from Abramitzky et al. (2014). We provide here more details on the implementation of these two approaches.

Inferring mobility from the 1900-1920 Censuses. Our goal is to develop estimates of net migration by nativity during the 1900-1920 period. We restrict the analysis to individuals aged 27-67, i.e. we disregard the problems related to measuring births and deaths at the extremes of the age distribution. We further restrict our analysis to white males, as immigrants at the time were mostly Europeans and in order to avoid further complications stemming from differential survival rates between whites and blacks.

We then collect from the U.S. Life Tables the survival rates by age/nativity for the period 1900-1920.[47] For each cohort we then define the net migration rate as the difference between the Census' reported population change between 1900 and 1910 (and respectively between 1910 and 1920). More precisely, let c be an age cohort, t be the Census enumeration at time t, $t+10$ be the census enumeration ten years later and finally, let s indicate the relevant state. The net migration of natives towards state s is given by:

$$NF^{n}_{cst} = Native_{c+10,t+10,s} - \gamma^{n}_{c} Native_{cts},$$

where NF indicates net flows, i.e. the difference between inflows and outflows and γ^{n}_{c} are the survival rates by cohort for white native-born males obtained from the Vital Statistics. We then repeat the exercise to construct similar figures for naturalized foreign born and immigrants.

Our measure of internal mobility for naturalized migrants is given by:

$$NF^{nat}_{cst} = Naturalized_{c+10,t+10,s} - \gamma^{fb}_{c} Naturalized_{c,t,s} - \sum_{k=1}^{10} NewCert_{c+10,t+k,s}.$$

As before, NF_{nat} indicates net flows and γ^{fb}_{c} are survival rates by cohort for white foreign-born males.[48] To obtain a credible measure for the migration flows of naturalized foreign born, we need to account for new naturalization that occurred between Censuses. In particular, let $NewCert_{c,t+k,s}$ be the number of new naturalization certificates issued in state s at time $t+k$ for cohort c.[49] Given that we observe the stock of naturalized only at the time of the Census enumeration, we need to account for all the naturalization that occurred during the decade. To account for this, we have digitized the total number of naturalization certificates by state and year, obtained from the Annual Reports of the Immigration and Naturalization Services.[50]

[47] These rates are reported only by a subset of states (the so called registration states), namely: Connecticut, Illinois, Indiana, Kansas, Kentucky, Maryland, Massachusetts, Michigan, Minnesota, Missouri, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, South Carolina, Tennessee, Utah, Virginia, Washington, Wisconsin.

[48] Note that the vital statistics do not distinguish the survival rates by citizenship status.

[49] The Annual Reports contain information on the number of certificates awarded by state, but not by state and cohort. We predict the number of certificates for each cohort as being proportional to the size of that cohort in the total naturalized population of the state.

[50] The Annual Reports contain tables indicating the number of certificates awarded by state starting from 1908. For the 1900-1910 decade we might therefore be overestimating the number of naturalized internal migrant. As the internal migration rate for this group is higher in the second decade, this should not be a major concern.

Finally, for the 1910-1920 decade, we make a second adjustment. We account for the additional possibility that naturalized migrants could leave permanently the U.S. between t and $t+10$. If so, an observed change in the stock of naturalized migrants living in state s at time $t+10$ would not be attributable to internal migration, but rather to permanent emigration. To control for this possibility, we have digitized the total number of foreign-born emigrants by state and year, obtained from the Annual Reports of the Immigration and Naturalization Services.[51] For 1920, we can then adjust the stock of naturalized migrants to include also those naturalized who have emigrated, $NatEmigr_{c+10,t+10,s}$:

$$Naturalized^{adj}_{c+10,1920,s} = Naturalized_{c+10,1920,s} + NatEmigr_{c+10,1920,s}$$

We use $Naturalized^{adj}$ in calculating NF^{nat}_{cst} for 1910-1920.

We can now construct the aggregate net migration rate (ANMR) as suggested in (Bell et al., 2002) for each cohort:

$$ANMR^{j}_{ct} = \frac{1}{2}\frac{\sum_s |NF^{j}_{cst}|}{\sum_s P^{j}_{s}}$$

where P_s is the total expected surviving population in state s by the following decade,[52] and $j = n, nat$.

For comparison purposes, in Table 1 we have further aggregated the results by cohort calculating a national weighted average, where the weights are given by the share of the population in that particular age group, by nationality:

$$ANMR^{j}_{t} = \sum_c \frac{P^{j}_{c}}{P^{j}_{t}} ANMR^{j}_{ct}$$

where, similar to before, P^{j}_{c} indicates the expected surviving population of cohort c by the following decade, P^{j}_{t} the expected surviving population of the whole U.S. by the following decade, and $j = n, nat$.

Mobility with Longitudinal Data. Using the dataset constructed by Abramitzky et al. (2014), we can also illustrate the main state of destination of different subgroups of the population. In particular, focusing on the 1910-1920 decade, in Figure B1 we report the share of internal migrants in the total population who have moved over this period, split by immigration status. Natives' internal migration[53] (top panel) exhibits a clear westward pattern: recent migrants represent fifty percent or more of the population in California, Oregon and Washington, whereas outside of these states recent arrivals represent a large share of the population only in Oklahoma. Recently relocated migrants play instead an important role in a larger number of states, including not only the West, but also the North-East and the Southern US (bottom panel).

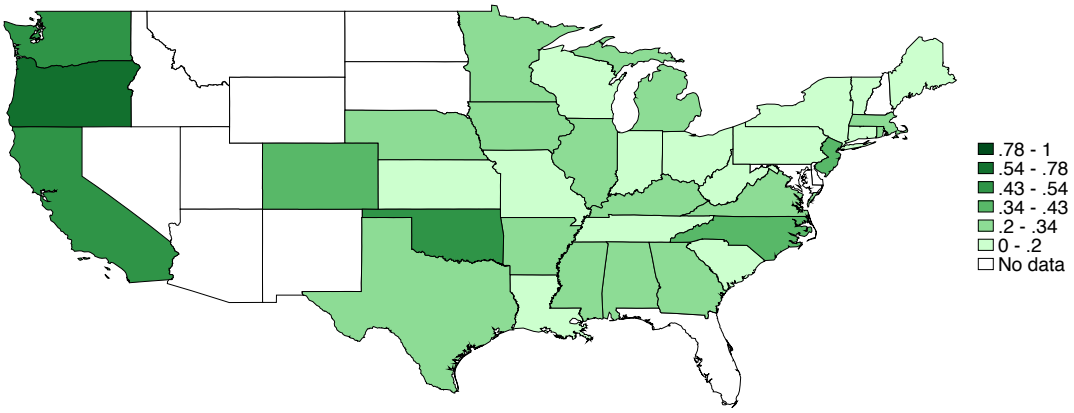
[51] The Annual Reports contain tables indicating the number of "departing aliens whose permanent residence has been in a given state who intend to reside permanently abroad" (INS Annual Report 1910, p.8). For the 1900-1910 decade these data were not collected and so we cannot perform such adjustment. In addition, the Annual Reports contain information on the number of foreign-born emigrants by state, but not the number of naturalized emigrants. We predict the number of naturalized emigrants in state s as being proportional to the naturalization rate of the foreign born residing in a given state.

[52] This is $\gamma^{fb}_{c} Naturalized_{c,t,s}$ for natives and $\gamma^{fb}_{c} Naturalized_{c,t,s}$ for the naturalized foreign born.

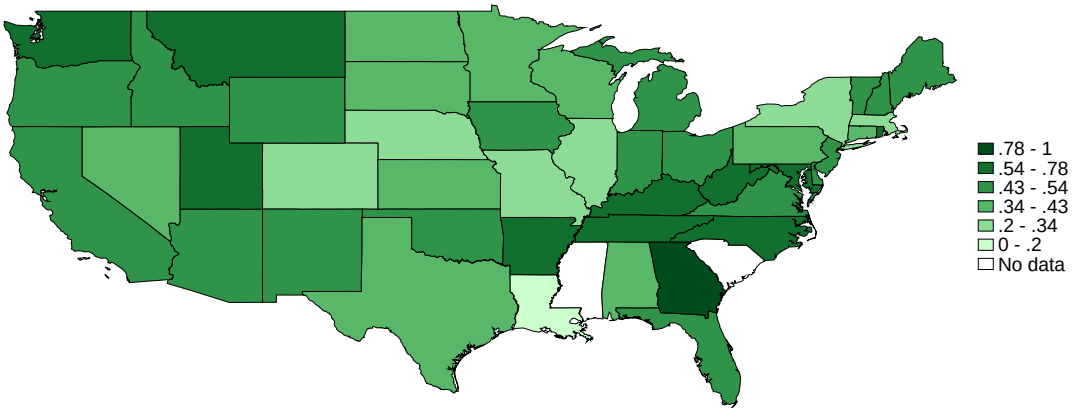
[53] Unfortunately due to the limited size of Abramitzky et al. (2014) dataset, we do not have enough observations to construct reliable figures for a group of states in the Midwest and Western U.S.

Figure B1: Share of internal migrants, by states and nativity, in 1920

(a) Share of native internal migrants, 1910-1920, by state



(b) Share of foreign born internal migrants, 1910-1920, by state



Source: Authors' calculations based on Abramitzky et al. (2014). We constructed an indicator that equals one if the state of residence in 1920 differ from the state of residence in 1910, by nativity. *Notes:* The maps represent the 1920 share of natives and foreign-born migrants who arrived in state s in the last ten years (i.e. between 1910 and 1920). Unfortunately due to the limited size of Abramitzky et al. (2014) dataset, we do not have enough observations to construct reliable figures for a group of states in the Midwest and Western U.S. We have dropped the states for which statistics are based on less than 10 observations.

C Additional Information on Data

Bills. Table C1 report all the bills included in the analysis. As explained in the text, we have excluded votes on amendments and restricted our attention to final passage votes (see section 3).

Table C1: U.S. House Votes on immigration legislation, 1897-1924

	Cong.	Date	Bill	Dir	Brief Description	Yes	No	Abstained	Valid Records	Law?
(1)	54	09-Feb-97	H.R. 7864	Pro	Additional changes in the text of the literacy test provision	218	37	100	355	No
(2)	54	03-Mar-97	H.R. 7864	Pro	To override presidential veto	195	40	120	355	No
(3)	57	03-Mar-03	H.R. 12199	Pro	Increases and extension of the head tax	194	11	146	351	Yes
(4)	59	18-Feb-07	H.R. 18673	Pro	Further increases in the head tax andcalls for a commission to study immigration	193	101	83	377	Yes
(5)	62	25-Jan-13	S. 3175	Pro	Restrictive bill that includes literacy provision	183	84	118	385	No
(6)	62	19-Feb-13	S. 3175	Pro	To override presidential veto	223	119	40	382	No
(7)	63	15-Jan-15	H.R. 6060	Pro	Increases and extension of the head tax	241	108	86	435	No
(8)	63	04-Feb-15	H.R. 6060	Pro	To override presidential veto	269	141	25	435	No
(9)	64	30-Mar-16	H.R. 10384	Pro	Restrictive bill that includes literacy provision, raises head tax	321	100	12	433	No
(10)	64	01-Feb-17	H.R. 10384	Pro	To override presidential veto	309	117	8	434	Yes
(11)	66	13-Dec-20	H.R. 14461	Pro	Suspension of immigration	316	60	58	434	No
(12)	67	13-May-21	H.R. 4075	Pro	Introduction of a quota system, based on the 1910 census figures	285	41	104	430	Yes
(13)	67	02-May-22	H.J.R. 268	Pro	Extension of operation of the immigration act of 1921	264	32	134	430	Yes
(14)	68	15-May-24	H.R. 7995	Pro	Introduction of quotas system	318	72	43	433	Yes

Cong and *Date* describe the congress/date in which/when the vote took place. *Bill* shows the name under which the bill is originating in the House of Representatives (H.R.) or Senate (S.). *Dir* shows whether the bill is pro or contra restricting immigration. *Brief description* provides some basic information about the content of the legislation. *Yes/No/Abstained/Present* show the overall number of yes/no/abstained and present votes. *Total votes* include the count of present individuals.

Our Sample Statistics. Table C2 reports the full summary statistics for the sample. As mentioned in section 3, the first stylized fact that emerges is the broad support for restrictionist measures.

Table C2: Summary statistics

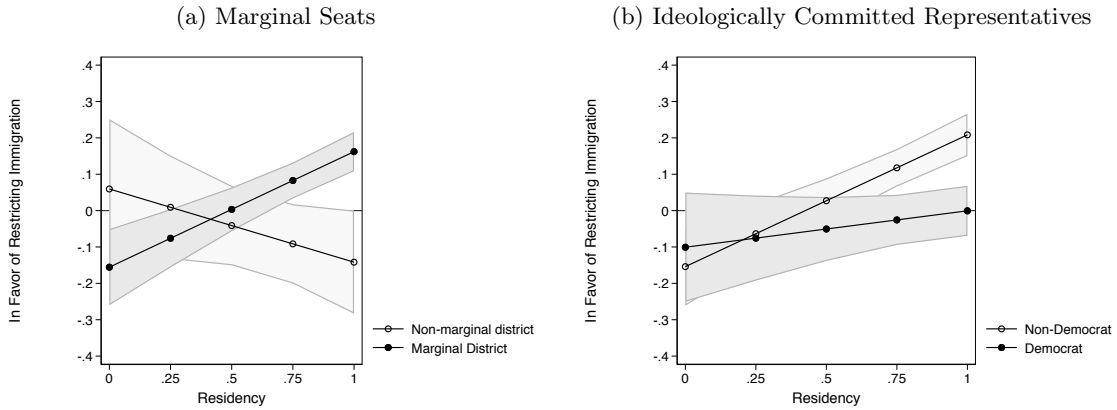
	Mean	Standard Deviation
$Vote_{icdt}$	0.617	0.486
VoteAlt	0.749	0.434
Residency	1.028	0.421
Residency County in 1890	0.295	0.275
Residency Precinct in 1890	0.066	0.096
High Foreign Born	0.241	0.428
Urban	0.421	0.360
GDP	0.359	1.000
Agriculture	1.233	1.000
Democrat	0.460	0.498
Southern Democrat	0.249	0.432
Age	49.937	9.296
Ivy League	0.093	0.291
Born Abroad	0.046	0.209
High Young	0.250	0.433
High Foreign Non-Citizen	0.249	0.432
DW-Nominate	0.055	0.430
Share Democrat	0.527	0.247
Share of Catholics/Jewish	0.177	0.201
Natives Internal Migration	0.250	0.433
High Diversity	0.235	0.424
N	40787	

All statistics have been weighted for the cell share of the population of that (part of) county in its corresponding congressional district. They can therefore be interpreted as district-level statistics. $Vote_{icdt}$ is a dummy variable which takes a value of one if the representative of district d has voted in favor of a bill restricting migration at time t, and zero otherwise. *VoteAlt* is a dummy variable which takes a value of one if the representative of district d has voted in favor of a bill restricting migration at time t, and zero if he voted against. *Residency* measures the number of years of residency in the state as of 1890. *High Foreign Born* is an indicator that equals one if the share of naturalized citizens is above the 75th percentile of the distribution of our sample period. *Urban* represents the share of the population living in cities and incorporated places with 2,500+ inhabitants. *GDP* represents standardized predicted personal income in each county. *Agriculture* represents the standardizedshare of the population employed agriculture. *Democrat* is an indicator of the representative belonging to the democratic party, and *Southern Democrat* is an indicator of the representative belonging to the democratic party in the South. *Age* is the age of the representative at the beginning of the congress. *Ivy League* is an indicator of the representative having attended an Ivy League school. *Born Abroad* is an indicator that equals one if the representative is born abroad. *High Young* is an indicator for the share of individuals less than 21 years old exceeding the 75th percentile of our sample period. *High Foreign-Born Non-Citizen* is an indicator that equals one if the share of non-citizens foreign-born in the country is above the 75th percentile of the distribution for our sample period. *DW-Nominate* is the representative's DW nominate score. *Share Democrat* is the share of votes casted to the democratic party in the last election. *Share of Catholics/Jews* indicate the share of Catholics and Jewish in a district. *Natives Internal Migration* is an indicator that equals one if the share of natives in the district whose state of birth differ from the state of residence is above the 75th percentile of the distribution for our sample period. *High Share of Blacks* is an indicator that equals one if the share of blacks is in the 75th percentile of the distribution in our sample. *High Diversity* is an indicator that equals one if the diversity within the migrant population is in the 75th percentile of the distribution in our sample.

D Details on Results

Graphs on results. Figure D1 illustrates the estimates of Figure 3. We have plotted the marginal effect of a high share of naturalized immigrants as a function of residency requirements. The first panels illustrate results of column (1) of Table 3. The solid line shows the effects in marginal districts, i.e. seats that were won with a low margin, whereas the other line considers seats won with a high margin. The second panels of Figure D1 illustrates results of column (2) of Table 3. Here the solid line represents the effect in districts represented by a congressmen not ideologically committed to the anti-immigration stance.

Figure D1: Marginal effects



Notes: Panel (a) shows the marginal effect of *High Foreign* in districts which were won in the previous election with a high and low margin of victory respectively. Panel (b) show the marginal effect of *High Foreign* in districts with a Democrat (ideologically committed) or conservative (non-ideologically committed) representative. See text for additional explanations.

Endogenous sorting of the naturalized foreign born. In Table 4 we perform a falsification exercise to address the concern that naturalized migrants might sort into areas depending on the local representative's voting behavior on immigration restriction. To this end, we estimate the following empirical model:

$$\Delta HighForeign_{cdt+1} = \beta_0 + \beta_1 Vote_{cdt} + \beta_2 Residency_s + \boldsymbol{X}'_{cdt}\gamma_1 + \boldsymbol{Z}'_{it}\gamma_2 + I_t + \alpha_c + \epsilon_{dt},$$

where $Residency_s$, $\boldsymbol{X}, \boldsymbol{Z}', I_t, \alpha_c$ are the variables included in column (4) of Table 2 and described in Section 3 of the paper. As in Table 2 the regression is weighted by the share of population in each cell relative to that of the district, adn these weights are updated following redistricting as described in 3. Finally, because $\Delta HighForeign_{cdt+1}$ changes only with every new census enumeration, we have to restrict the analysis to congress 62 to 67, where $\Delta HighForeign_{cdt+1}$ is based on the change in the share of naturalized foreign born between the 1910 and 1900 censuses, and congress 68, where $\Delta HighForeign_{cdt+1}$ is based on the change in the share of naturalized foreign born between the 1920 and 1910 censuses. Table 4 shows our result. Across all specifications, there is no significant relationship between naturalization patterns in district d and previous voting behavior on migration restrictions, and the coefficient estimates are also small in magnitude. Thus endogenous sorting of naturalized foreign born due to the past voting behavior of local representatives on migration policy issues does not appear to be a concern.

First-stage results. Table D1 shows the first stage results. We report a number of diagnostics. We reject that each single instrument is weak at standard conventional levels and we reject under- and weak identification of the full model at all conventional levels, across all specifications. We refer the reader also to the the discussion of bounds of the effects in the main text.

Table D1: Instrumental variable regression - First Stage

	First Stage - Baseline		First Stage - Dynamics			
	High Foreign Born	High Foreign × Residency	High Foreign Born	High Foreign × Residency	Lag High Foreign Born	Lag Lag High Foreign × Residency
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument	0.327***	–0.233***	0.316***	–0.230***	0.034*	–0.015
	(0.042)	(0.044)	(0.053)	(0.049)	(0.019)	(0.033)
Instrument × Residency	0.208***	0.813***	0.220***	0.809***	0.051**	0.106**
	(0.044)	(0.057)	(0.055)	(0.065)	(0.025)	(0.045)
Lag Instrument			–0.055	0.184***	0.760***	–0.105***
			(0.071)	(0.028)	(0.039)	(0.019)
Lag Instrument × Residency			–0.330***	–0.588***	–0.247***	0.636***
			(0.095)	(0.074)	(0.071)	(0.065)
Kleibergen-Paap LM statistic	124.067				81.375	
Kleibergen-Paap p-value	0.000				0.000	
Kleibergen-Paap rk Wald F statistic	237.406				42.909	
Stock-Yogo (2005) critical value at 10%	7.56				7.56	
F-statistic	223.796	239.132	95.750	102.355	148.234	43.169
Sanderson-Windmeijer F	108.367	322.626	145.831	105.482	636.312	653.550
Partial R^2	0.606	0.596	0.161	0.218	0.276	0.186
Adj. R-Square	0.31	0.34	0.32	0.35	0.46	0.38
N	40617	40617	32554	32554	32554	32554

Robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
All columns include region trends, state and time fixed effects and all controls of Table 2, column 4.
The dependent variable is indicated on top of each comulmn. The Kleibergen-Paap LM statistic refers to an underidentification test. The Kleibergen-Paap rk Wald F statistic presents a weak identification test. Note that the Stock-Yogo critical values are reported for i.i.d. errors. *Instrument 2* is an index of ruggedness.

E Additional robustness checks

In this section, we assess the robustness of our empirical findings by implementing a number of additional specifications. We start by experimenting with alternative definitions of our dependent variable and by explicitly modeling the decision to cast a ballot on an immigration policy measure. Next, we further investigate the mechanism through which restrictions of the franchise affected voting behavior on immigration policy.

As discussed in section 3, in our benchmark analysis our dependent variable $Vote_{icdt}$ takes a value of one if the representative has voted in favor of a restrictionist measure, and zero if he has either voted against it or if he has abstained. The rationale for this choice is that each bill put forward was aimed at changing the existing status quo, in a context in which the presidency was not keen to introduce a restrictionist measure and repeatedly vetoed the bills approved by Congress. In this environment an abstention had an effect equivalent to a "no" vote. Still, as shown in Table C1, representatives often chose to abstain – on average 19 percent of the House did not cast a vote on the proposals considered in our analysis. As a result, it is important to assess the robustness of our empirical findings to an alternative definition of our dependent variable and to explicitly accounting for the decision to cast a ballot. Table E1 reports our results. In column (1) we use an alternative dependent variable, $VoteAlt_{icdt}$ which takes a value of one if the congressman voted in favor of the bill, and zero if he voted against, and our results are comparable with those obtained in our benchmark specification in column (4) of Table 2. In columns (2) and (3) we report instead the results of a Heckman selection model, which is specified as follows:

$$Vote_{icdt} = \mathbf{X}\beta + I_t + \alpha_c + u_{icdt} \quad (4)$$

$$CastBallot_{icdt} = 1 \text{ if } \mathbf{Z}\gamma + I_t + \alpha_c + e_{icdt} \geq 0 \quad (5)$$

where β and γ are parameter vectors, $\mathbf{X}$ and $\mathbf{Z}$ are vectors of controls (with potentially common elements), u_{icdt} and e_{icdt} are normally distributed error terms and $Corr(u_{icdt}, e_{icdt}) = \rho$. Equation 4 is the main model in which we are interested, whereas equation 5 captures the possible presence of sample selection. In particular, note that $Vote_{icdt}$ is observed only if $CastBallot_{icdt} = 1$. Moreover, if $\rho = 0$, selection is not a concern, and equation 4 can be estimated consistently on its own (see column (1) of Table E1 for the results under this assumption). To identify the possible effect of selection, we need to include in equation 5 at least one additional control that is not included in equation 4 and that, conditional on $\mathbf{X}$, affects the probability of casting a ballot without directly affecting the vote on the migration initiative.

To this end, for each representative we have constructed a proxy for his propensity to cast a ballot in that Congress, $ShareVoted_{icdt}$, using the share of times he has voted "Yes" or "No" on all the measures for which roll call votes are available, with the exclusion of the immigration bills included in our analysis. Column (2) reports our estimates of the main equation, whereas column (3) presents the results for the selection equation. We cannot reject the null hypothesis of no sample selection bias, as the estimate for the correlation between these two equations (ρ) is statistically insignificant. Furthermore, the coefficient of $ShareVoted_{icdt}$ is negative and strongly significant in the selection equation, suggesting that a greater average propensity to vote does also decrease the likelihood that the representative will cast a ballot on migration bills. Still, our main results in column (4), Table 2 do not appear to be materially affected (see column 2).

Table E2 explores the robustness of our results using alternative samples. As explained in the paper, rather than using the county-district cell dataset, which allowed us to introduce county fixed effects, we have constructed a dataset at the electoral district level. District-

Table E1: Robustness checks: alternative specifications of the dependent variable

		Heckman	
	VoteAlt	Outcome	Selection
	(1)	(2)	(3)
High Share of Foreign-Born	–0.135**	–0.295***	–0.510***
	(0.058)	(0.087)	(0.119)
High Foreign x Residency	0.269***	0.403***	0.602***
	(0.066)	(0.086)	(0.062)
GDP	–0.091***	–0.056**	–0.010
	(0.021)	(0.025)	(0.056)
Agriculture	0.137***	0.126**	0.027
	(0.025)	(0.058)	(0.096)
Urban	–0.153	0.025	0.054
	(0.107)	(0.214)	(0.356)
Democrat	–0.279***	–0.348***	–0.006
	(0.022)	(0.039)	(0.082)
Southern Democrat	0.191***	0.368***	0.083
	(0.060)	(0.101)	(0.260)
Age	–0.000	–0.003*	–0.001
	(0.001)	(0.001)	(0.003)
Ivy League	–0.001	0.050	0.019
	(0.029)	(0.047)	(0.103)
ShareVoted	-	-	–1.582***
	-	-	(0.067)
Mills ratio		0.0100	
St. error Mills ratio		0.0101	
Rho		0.0259	
Adj. R-Square	0.37		
N	34001	34001	

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4. All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.
VoteAlt is a dummy variable which takes a value of one if the representative of cell *cd* has voted in favor of a bill restricting migration at time t, and zero if voted against. The Heckman selection model describes the voting process as a two-step decision: the selection equation estimates first the likelihood of an individual casting or not a ballot on a migration bill; then the outcome equation estimates the likelihood of a representative then decideing whether to support or not the initiative.

specific data are not readily available for the period we are studying, and must instead be constructed by aggregating county-level data obtained from the full count (i.e. 100% sample) of the decennial censuses. To obtain district-level data from county level information, we first extract county characteristics from the decennial censuses and then aggregate them at the district level. For those counties that were split across more than one district, we implement a "dasymetric interpolation" (Goplerud, 2016), following Baldwin and Magee (2000) among others, and construct weighted averages, which are based on the share of the population of a county assigned to that district. To deal with the problem of redistricting, we have kept track of changes in the boundaries of the constituencies that occurred after each of the censuses of 1900, 1910, and 1920. Our main findings are unchanged in this alternative analysis. Next in column (2) we exclude the vetoed bills from the sample, hence focusing on those bills that resulted in policy changes. We find that our man results are unaffected also when we drop

vetoed bills. Our analysis covers 14 final passage votes that took place in Congress between 1897 and 1924. Previous studies (e.g. Goldin 1994) have included also votes on four additional votes on bills that were subsequently amended by the Senate.[54] In column (3) we consider also these bills in our analysis, and the results are unaffected.

[54]These are the bills: H.R. 7864, 27-Jan-97; S3175, 18-Dec-12; H.R. 6060, 4-Feb-14 and H.R. 7995, 12-Apr-24

Table E2: Robustness checks: alternative samples

	ED level	Indicator	Include	Regions	
	Dataset	Vetoed bills	All Votes	Exclude South	Exclude Midwest
	(1)	(2)	(3)	(4)	(5)
High Share of Foreign-Born	–0.154***	–0.141**	–0.098*	–0.178***	–0.131*
	(0.054)	(0.062)	(0.054)	(0.058)	(0.078)
High Foreign × Residency	0.154**	0.298***	0.213***	0.284***	0.323***
	(0.060)	(0.069)	(0.062)	(0.064)	(0.079)
Adj. R-Square	0.20	0.25	0.27	0.26	0.29
N	5709	40617	52240	24106	26312

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4.
All regressions have robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one. *High Foreign Born* (and *High Foreign*)is an indicator that equals one if the share of naturalized citizens is above the 75th percentile of the distribution for our sample period.
ED Level Dataset indicates a district-level dataset.
Indicator Vetoed Bills includes an indicator for the following bills: H.R. 7864, S. 3175, H.R. 6060H.R. 10384. See text for details.
Include All Votes keeps all votes, including non-final passage votes. See text for details. In *Exclude South* we exclude all states that are in the South. In *Exclude Midwest* we exclude the Midwest. In *During Dillingham* the sample is restricted to the 1911-1917 period, while *Outside Dillingham* restricts the sample to all other years. *Residency* is an index capturing the number of years of residency in the state, county and parish required for voting.

As pointed out in Section 2, residency requirements varied a great deal across space, and were generally higher in the South, and lower in the Midwest. To insure that our results are not driven by specific subregions, in column (4) and (5) we respectively exclude the South and the Midwest from our analysis. While the qualitative findings are unaffected, the magnitudes of the coefficients obtained excluding the Midwest appear slightly larger and more precisely estimated.

Finally, as discussed in footnote 19, during our sample period, the franchise was restricted to citizens 21 years and older, and universal female suffrage was mandated only by the Nineteenth Amendment in August 1920, even though a few states had introduced it before (Lott and Kenny 1999). Correspondingly, the universe of the citizenship status question in the Census includes males only in 1900 and 1910, and males and females in 1920. We are therefore unable to construct an exact measure of eligible foreign born voters, but as long as male and female naturalized migrants share a similar voting behavior our results will not be affected. Still, to assess the robustness of our findings we experiment in Table E3 with several additional specifications. We start in column (1) by excluding observations using the 1920 Census. As a second check (column (2)), we exclude all those states that had female suffrage prior to 1920. Third, in column (3), we control for changes in the universe by including an indicator for female suffrage and its interaction with congress fixed effects. By doing so we capture time specific differences in the voting behavior of representatives elected in states with and without female franchise. Our results are not qualitatively affected.

Table E3: Checks on High Foreign due to Changes in Universe/Female Suffrage

	Exclude Census of 1920	Exclude States with Female Suffrage	Control for time/universe f.e.
	(1)	(2)	(3)
High Share of Foreign-Born	–0.151**	–0.014	–0.115*
	(0.074)	(0.097)	(0.061)
High Foreign x Residency	0.334***	0.202**	0.270***
	(0.081)	(0.100)	(0.068)
Adj. R-Square	0.26	0.26	0.25
N	37452	28472	40617

Robust standard errors in parenthesis, clustered at the district-congress level. Observations are weighted by a county-district cell's share in the total district population of a district, so that each district has a total weight of one.

All columns include region trends, state and time fixed effects and all controls of Table 2, column 4. See Table 2 for variable description. *Exclude Census of 1920* is a regression in which we exclude the observations based on the 1920 Census. *Exclude states with female suffrage* is a regression in which we exclude all states before 1920 where females could vote and include all states in 1920. *Control for time/universe f.e.* is a regression in which we include a dummy variable for having female suffrage and its interaction with year of congress fixed effects.