A   Online Appendix (Not for Publication)

A.1   Additional Tables

Table A1: Republican Representation

Dep. Var.:	(1) republican	(2) republican
QE x post	2.196***	2.218***
	(0.671)	(0.602)
republican$_{start}$		0.0916
		(0.0680)
Observations	690	682
R-squared	0.800	0.801

Notes: *** p< 0.01, **p< 0.05, *p< 0.1. All specifications include district fixed effects and time period fixed effects. Standard errors are clustered by district. All columns estimated using OLS. Observations are weighted by that district's 1900 population level.

Table A2: Main Results, No Population Weighting

Dep. Var.:	(1) pro trade	(2) anti trade	(3) pro trade	(4) anti trade	(5) pro trade	(6) anti trade	(7) pro trade	(8) anti trade
QE x post	0.123	-0.330	0.443	-0.635	-2.401**	1.209	4.151***	-3.059***
	(0.673)	(0.559)	(0.516)	(0.404)	(1.013)	(1.058)	(1.168)	(1.038)
Republican$_{end}$			-0.884***	0.906***				
			(0.0379)	(0.0361)				
Republican$_{start}$			0.00889	0.0252				
			(0.0415)	(0.0372)				
Observations	674	674	672	672	528	528	84	84
R-squared	0.729	0.745	0.889	0.912	0.747	0.754	0.695	0.715

Notes: *** p< 0.01, **p< 0.05, *p< 0.1. All specifications include district fixed effects and time period fixed effects. Standard errors are clustered by district. Observations are weighted equally.

Table A3: Main Results, State-Year FE

Dep. Var.:	(1) pro trade	(2) anti trade	(3) pro trade	(4) anti trade	(5) pro trade	(6) anti trade	(7) pro trade	(8) anti trade
QE x post	-0.840	0.356	0.446	-0.991	-1.747*	0.308	2.520	-0.336
	(1.265)	(1.470)	(0.722)	(0.770)	(0.951)	(1.527)	(1.776)	(1.383)
$Republican_{end}$			-0.866***	0.906***				
			(0.0700)	(0.0477)				
$Republican_{start}$			0.000949	0.0607				
			(0.0486)	(0.0435)				
Observations	668	668	668	668	518	518	76	76
R-squared	0.848	0.861	0.919	0.940	0.858	0.867	0.844	0.878

Notes: *** $p< 0.01$, **$p< 0.05$, *$p< 0.1$. All specifications include district fixed effects and state-time period fixed effects. Standard errors are clustered by district. Observations are weighted by that district's 1900 population level.

A.2 Discussion of Additional Tables

In this Appendix I have included several robustness checks to the main results. First, Table A1 verifies that the quota restrictions had an effect on Republican representation for affected districts. The coefficients are strongly positive and significant, showing that more quota-affected districts were more likely to have Republican representation. This remains true even conditional for start-of-period representation. This implies that the mechanism of quotas leading to more Republicans who then vote anti-trade is plausible.

Next, Table A2 does not employ population weighting and re-estimates the main results. The main conclusions remain largely unchanged. Republican representation remains predictive of anti-trade sentiments, with its inclusion washing out much of the treatment effect from immigration restrictions. The restrictions also still increase polarization in terms of anti-trade voting, with anti-immigrant restrictions further increasing anti-trade legislation support in areas whose legislators who were anti-immigrant, and vice versa for initially pro-immigrant areas. Given the large disparities in some of the districts for population, population weighting still appears to be the more appropriate specification.

Finally, Table A3 reports results with state-year fixed effects. This removes a large amount of variation in the data, and the resulting coefficients are much less precisely estimated. The magnitudes of many of the coefficients, however, remain in the same range as the original estimates. In particular, the results still strongly show that Republicans were more likely to vote for anti-trade legislation and that anti-immigrant districts were the ones to respond to the quotas with anti-trade voting in Congress.

A.3 Crosswalking to 56th Congressional Districts

The data used in this paper are originally denoted in a variety of geographies over a long time span. The two key types of geographies are congressional districts and counties. Districts are substantially less numerous and thus larger in area than counties, with over 300 districts in the first time period; in contrast, there are 2000+ counties during this time. I therefore take congressional district boundaries during the 56th Congress (representing the earliest time period in my sample) to be the baseline geography to which I will convert all other boundaries.

I convert US counties from 1900, 1910, 1920, and 1930 to 56th Congress districts using a similar area-based crosswalk methodology as in Hornbeck (2010). I take the boundaries of the counties in a given year and overlay them on the 56th district boundaries. The two are then intersected using the Intersect tool in ArcMap. This produces an intersected shapefile that contains, for each county, the different "pieces" of each district that the county falls within. I then use this to divide each variable's value into area-based shares, where the shares denote the proportion of each county's area that falls within each different district. This approach is taken with the county crosswalks since all variables used in the county datasets are population-based, representing the sum of a particular group or value over that county. As such, the crosswalk simply apportions the total sum of that county over the different districts that overlap with that county.

In converting districts from other Congresses to 56th Congress districts, I take a similar area approach and intersect boundaries first. I then, however, use the intersected areas to calculate the shares of each other district that each 56th district intersects with. I then take that 56th district and calculate the weighted average, where the weights are the shares, of each variable. This crosswalk, in other words, calculates the weighted mean value of each variable for a given 56th district based on the values from the other districts that fall within that 56th district. This slightly different approach is taken with the district crosswalks because the district-level variables are not sums as in the counties above but rather represent average values or characteristics, such as the ideological stance of an elected congressperson.

A.4 Additional Literature Review

This paper relates to several different fields of research within economics. First, my work contributes to the literature that has studied the causes and consequences of the 1921 Emergency Quota Act and 1924 Immigration Act. Abramitzky et al. (2023), using a similar research design to mine, find that the quota restrictions led to immigrants from affected countries being replaced by other migrants and to some sectoral shifting. Tabellini (2020) finds that increased immigration in the early 20th century led to increased support for conservatives and congressional voting towards anti-immigration legislation. In contrast to this prior work, my paper focuses on the legislative consequences of anti-immigration legislation, focusing particularly on another important insular Act during this time: the Smoot-Hawley Tariff Act.

Another related body of work has studied the origins of the Smoot-Hawley Tariff Act. Douglas Irwin,

in a series of influential papers and books,[1] has documented and provided rich evidence for the evolution of US trade policy over time. Some of his earlier work, however, focuses further on the causes of the Smoot-Hawley Act. For example, Irwin and Kroszner (1996) argue that although voting along party lines played an important role, Senators acting in response to local producers and log-rolling were also important factors. More recently, Irwin and Soderbery (2021) show that political lobbying and terms-of-trade factors both played roles in tariff setting during the Smoot-Hawley era. I complement this existing literature by focusing on how a new factor, reduced immigration due to the quotas imposed by anti-immigration legislation in 1921 and 1924, also contributed to the passage of the Smoot-Hawley Act in 1930.

There is some existing work that studies the connection between immigration and trade policies, largely from political science. Peters (2015), in cross-country fixed effects regressions spanning 3 centuries, finds a negative association between immigration policy openness and trade policy openness, arguing that trade openness leads to anti-immigration policy. These ideas are further expanded upon in a follow-up book as well (Peters (2017)). In contrast to this work, I find, using within-country Congressional District-level regressions, that anti-immigration policy causes increased subsequent support for anti-trade policy; this suggests that context may play an important role in determining the relationship between immigration and trade policy. My results also contrast with the negative relationship between immigration and trade openness found by Peters (2015), which implies that context may be important in how the two types of policy relate to each other.

A large amount of research in international trade has argued that immigration causes more trade. Rauch and Trindade (2002), in an early example, show that Chinese migrants in a country foster trade between that country and China; the authors provide additional evidence suggesting that this is due to facilitation of matching between buyers and sellers. A substantial body of work has followed this paper, most of which has provided further supporting evidence of this immigration-trade linkage.[2] Relative to this work, my paper provides evidence of an additional linkage through which immigration can affect trade: changes to legislative support for anti-trade policy.

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[1]See Irwin (2019) and Irwin (2017) as a pair of more recent examples.
[2]For a recent example, see Egger et al. (2019).

Finally, my paper follows two parallel streams of research that have studied how immigration and trade have affected electoral outcomes. Papers such as Autor et al. (2020), Feigenbaum and Hall (2015), and Dippel et al. (2022) have made connections between trade shocks and election outcomes and legislator behavior. A similar set of papers, such as Dustmann et al. (2019) and Mayda et al. (2022), has linked immigration to political changes. Both strands of literature have typically found that increased immigration and import exposure lead to an upswing in conservatism and anti-immigration or anti-trade voting. My work connects these two separate areas by explicitly connecting the consequences of anti-immigration policy to the passage of anti-trade policy.

A.5 Context

A.5.1 Immigration

The period from 1881 up to 1919 was a period in which immigration to the United States was booming, with the period from 1900-1914 alone being responsible for bringing almost 900,000 new immigrants to American shores. These numbers are substantial for a country that numbered just under 50 million in 1880.

There are multiple major causes for this immigrant boom. Steamship technology had advanced sufficiently to allow the setup of multiple steamship passenger lines crossing both the Atlantic and Pacific Oceans. In the Atlantic, crossing the ocean could take up to six weeks by sail; this trip would be shortened to under 2 weeks with steam,[3] with further technological improvements further shaving down the crossing time. Other factors that played a role included economic conditions being better in the US relative to origin countries and a booming population in Europe. For a much more thorough discussion of the causes of the wave of immigration that occurred in the late nineteenth and early twentieth century US, see Hatton and Williamson (1998) or Cohn (2017).

One factor influencing the ongoing arrival of immigrants to the US during this period was the relatively lax immigration policy, which persisted until World War I. There were some notable exceptions to this laxness, such as the 1882 Chinese Exclusion Act which effectively barred Chinese immigration. For more information on the Chinese Exclusion Act see Geloso and Peng (2021), who argue that policy competition

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[3]See Cohn (2005).

amongst Democrats and Republicans in California towards tougher anti-immigration policy contributed to the passage of the 1882 Act. The United States and Japan also entered into a "gentleman's agreement" in 1907 where Japan would voluntarily agree to prevent further Japanese migration to the US. This period would come to an end with the passage of the Immigration Act of 1917. One of the main features of this new legislation was the imposition of a literacy requirement for all immigrants over a certain age. This requirement would effectively act as a deterrent to immigrants from non-English speaking countries.

The 1917 Act would pale in comparison to the further anti-immigration policies of the 1920s. In 1921, Congress passed the Emergency Quota Act. The number of migrants from a given country was now limited by a set number (or quota), which was set as 3% of however many people were in the United States as of the 1910 Census that were from that same country. In addition to greatly limiting the total number of immigrants arriving to the US, this system also effectively discriminated against migrants from countries that had only relatively recently begun sending people to the US such as those from Eastern Europe. This Act was quickly followed by the 1924 Immigration Act,[4] which updated the quotas to be even smaller, based on 2% of each national origin's population in the US as of the 1890 Census. The use of the so-called National Origins Formula system, where immigrants were explicitly limited in number based on their country of origin, would continue until its elimination by the Immigration and Nationality Act of 1965. The Acts of 1921 and 1924 therefore set a precedent that would dramatically alter the course of American immigration policy for several decades.

A.5.2   International Trade Policy

The period from 1870 to just before the start of World War I is sometimes referred to by economists and historians as the First Golden Age of Globalization. International trade increased substantially during this period, more than tripling in per capita terms (Pascali (2017)). Multiple explanations have been proposed to account for the high levels of trade during this time, such as lower transport costs and the gold standard.

One important driver of trade during this period was trade policy, which was relatively pro-trade during this period. Irwin (2019), for example, reports that the average tariff charged against imports was on a clear downward trajectory over the entire period of the First Golden Age and even up to 1930. This could be seen

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[4]The Immigration Act was voted through the House of Representatives on April 12, 1924 by a vote of 323-71.

in the passage of legislation that liberalized trade, such as the Underwood Act of 1913. Although it also introduced changes to the US tax system, the Underwood Tariff Act principally dramatically lowered tariffs against imports.

This period of falling tariffs and pro-trade legislation would come to an end in 1930 with the passage of the Smoot-Hawley Tariff Act of 1930.[5] The Act was partly a response to a desire to protect domestic producers due to the onset of the Great Depression in 1929. Republican campaign promises to raise tariffs and the Republican candidate, Herbert Hoover, taking the White House in 1928 also played an important role. The Act overhauled the entire US tariff schedule and raised tariffs against imports for a broad range of products. In 1929, import duties divided by imports (a proxy for the average tariff) was 13.48%; by 1933, this value had increased to 19.80%. The change becomes even more dramatic when focusing only on dutiable imports.[6] For dutiable imports, the average tariff as calculated above goes from 40.1% in 1929 to 53.58% in 1933. Because of the comprehensive nature of the Act and its sharp reversal a decades-long trend of ongoing liberalization, the Smoot-Hawley Tariff Act remains well-known in the modern period. As one illustration of the Smoot-Hawley Act's long shadow, the New York Times search engine shows that "Smoot-Hawley" has been mentioned in 103 articles from January 1, 2000 to December 31, 2022.

A.6 Difference-in-Differences Methodology

In this subsection, I provide some graphical intuition for the assumptions required in this paper for the methodology to produce the causal effects as desired. The methodology used in this paper is a difference-in-differences setup, where the variable used to denote treatment status is not binary as in the classic case. Instead, the treatment variable is a continuous variable that measures the extent of treatment intensity. In other words, this variable captures how much the quotas affect a given district. One implication of this setup is that there are no non-treated districts; all districts are affected in some way by the quotas and the variation between them is only in how much they are affected.

This setup is commonly used in many empirical applications in economics. Pierce and Schott (2016), for example, use variation across US industries in exposure to Chinese imports to estimate import competition's

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[5]The Smoot-Hawley Act passed through the House of Representatives on May 28, 1929, with a vote of 264-147.

[6]Some imports are classified as "free" imports.

effect on manufacturing employment. As in my setup, there are no non-treated industries and the strategy implicitly therefore compares lesser to more treated groups. This intuition is visually illustrated in the figure below, which compares the trend in the outcome variable of interest for two districts: a high-quota affected district (in dashed red) and a low-quota affected district (in solid blue). As one can see, both lines proceed in a parallel fashion until the vertical dashed line, which represents the time at which the post-period begins. As both districts are "treated", i.e. are affected by the quotas in some way, both lines jump upward as in the classic diff-in-diff case. The specification in this paper therefore relies on the size of the increase post-treatment in the two district types. In the illustrated example, the more quota-affected district jumps upward more, suggesting that quota-affectedness has a positive effect on the outcome. One implicit assumption that

Figure A1: Diff-in-Diff with Continuous Treatment
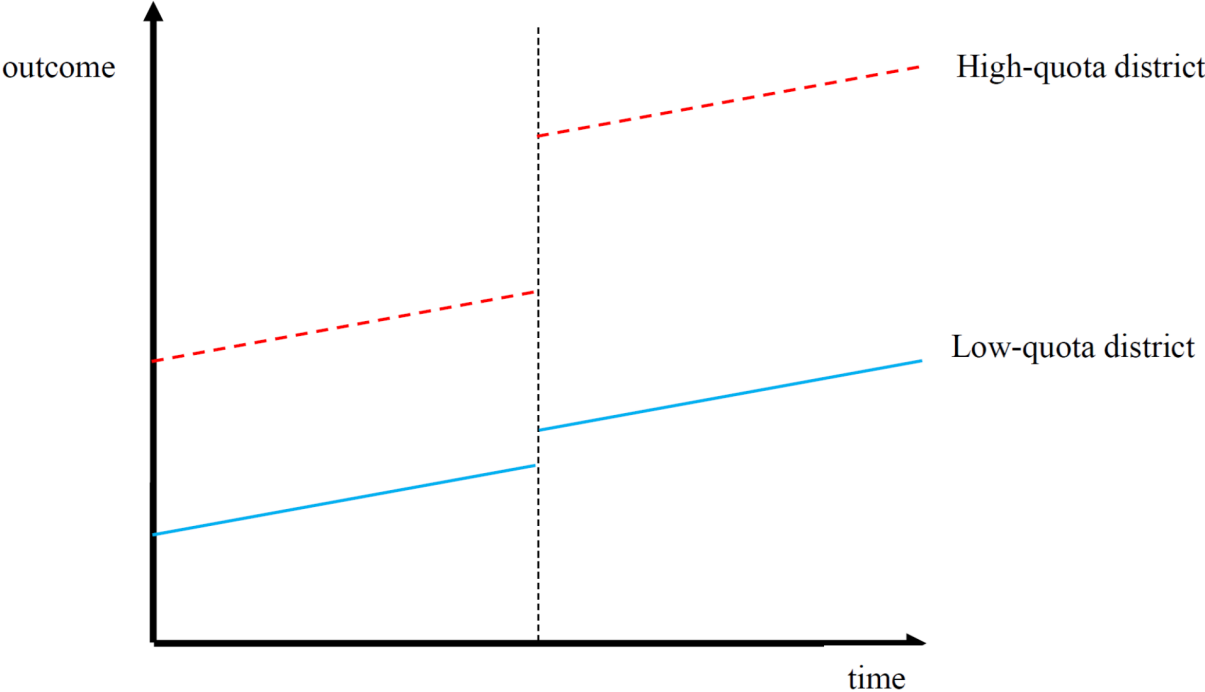
this paper requires, as in many diff-in-diff papers, is that parallel trends hold. In simple terms, it requires that in the absence of treatment that treated versus non-treated groups would have similar (or parallel) trends in the outcome variable. Callaway et al. (2024), however, argue that in the continuous treatment case a stronger parallel trends assumption is needed. In their paper, the strong trends assumption is that in the expected change in outcomes over time for a given treatment intensity is always equal to the actual changes

over time for that treatment intensity. This assumption abstracts from treatment heterogeneity.

While it is not possible to evaluate the likelihood that this assumption holds in my setting, I point to evidence provided in Abramitzky et al. (2023), who implement the same diff-in-diff design to show that the immigration restrictions led to differences in labor market outcomes. Abramitzky et al. (2023) show that quota exposure is not correlated with almost any of a large set of initial characteristics except for agriculture's employment share, suggesting that variation in the treatment variable is not inadvertently picking up any differences in trends caused by initial differences across districts in my setting. Abramitzky et al. (2023) also show that a placebo treatment period based on failed earlier anti-immigrant legislation did not have any effect on outcomes. For the purposes of my paper, this implies that there likely were not pre-trends underlying the analysis. As in Abramitzky et al. (2023), I therefore argue that quota-affected districts likely were not undergoing different trends during the pre-period and were not correlated with any confounding initial characteristics which may have then caused differences in outcomes in the post-period.

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