EMIGRATION RESTRICTIONS AND ECONOMIC DEVELOPMENT EVIDENCE FROM THE ITALIAN MASS MIGRATION TO THE U.S.*

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ABSTRACT

We study how immigration restriction policies influence technology adoption in countries sending migrants. Between 1920 and 1921, Italian emigration to the United States dropped by 85% after Congress passed the Emergency Quota Act, a severely restrictive immigration law. In a difference-in-differences setting, we exploit variation in exposure across Italian districts to this large restriction on human mobility. Using novel individual-level data on Italian emigration to the US and newly digitized historical censuses, we show that this policy substantially hampered technology adoption and capital investment. This evidence is consistent with directed technology adoption theory: an increase in the labor supply dampens the incentive for firms to adopt labor-saving technologies. To validate this mechanism, we show that more exposed districts display a sizable increase in overall population and manufacturing employment. We provide evidence that "missing migrants," whose migration was inhibited by the Act, drive this result.

KEYWORDS: Age of Mass Migration, Emigration, Technology Adoption.

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I Introduction

Technology adoption is a key driver of economic growth in developing countries, which operate far from the technology frontier (Suri, 2011; Bryan *et al.*, 2014).[1] This paper examines how out-migration—a typical feature of industrializing economies—influences the incentive for firms in developing countries to adopt productivity-enhancing technologies. Specifically, we investigate how restrictions to human mobility imposed by immigration countries influence technology adoption in the emigrants' countries of origin.[2]

The effects of out-migration on technology adoption are *ex-ante* ambiguous and potentially conflicting. On the one hand, emigration entails a loss of human capital—a "brain drain"—that may hamper the ability of countries to adopt new technologies (Kwok and Leland, 1982; Gibson and McKenzie, 2011). On the other, however, higher emigration rates may incentivize the adoption of labor-saving technologies by increasing the relative cost of labor (e.g., see Habakkuk, 1962; Hicks, 1932 [1963]). From the directed technical change theory perspective, one can interpret immigration restriction policies (henceforth, IRPs) as "passive" labor market policies that increase the labor supply in targeted countries. These positive labor supply shocks would, in turn, prompt the substitution of capital with more abundant—hence cheaper—labor, thus depressing investment in capital-intensive technologies. Which effect prevails is, ultimately, an empirical question.

We study the Italian mass migration, the largest episode of voluntary migration from one single country in recorded history (Choate, 2008). Between 1876 and 1924, approximately thirteen million emigrants left Italy (nearly 70% of the average Italian population in 1900); about half never returned. Italy had one of the highest emigration rates and, since the 1890s, it was the leader in sheer emigration numbers (Hatton and Williamson, 1998). On average, 40% emigrants headed toward the United States, the single most common destination country and the focus of this paper. The Italian mass migration to the United States abruptly ended in 1921 when Congress passed the first of two restrictive IRPs that we collectively refer to as the "Quota Acts." The Quota Acts defined numerical quotas on yearly arrivals from European countries, which drastically reduced the number of Italians entering the United States.[3] Between 1920 and 1921, the inflow of Italians in the US

[1]Economic historians have famously recognized that countries that industrialized relatively late, such as Germany or Italy, relied heavily on innovation produced abroad to catch up with the core industrial countries (Gerschenkron, 1962; Rosenberg, 1982). Recent literature within the tradition of endogenous growth theory embeds technology diffusion dynamics and quantifies its—substantial—contribution to productivity growth (e.g. Eaton and Kortum, 1999; Buera and Oberfield, 2020).

[2]For brevity, we refer to those policies as immigration restriction policies (IRPs). Data from de Haas *et al.* (2015) suggest that IRPs have become increasingly common since the 1970s and currently account for 40% of the entire corpus of migration laws.

[3]The 1921 Emergency Quota Act restricted the annual number of immigrants admitted into the US to no more than 3% of the number of residents from that country, as recorded in the 1910 census. The 1924 Johnson-Reed Act reduced the quota to 2% and pegged the reference date to the 1890 census. These laws explicitly targeted Southern and Eastern European countries, which until the early 1900s hardly took part in the Age of Mass Migration and whose immigrants were perceived by the public as a threat to America's economic welfare and cultural values (Higham, 2002).

dropped by 85% and never recovered (see Figure 1). A follow-up tightening policy was imposed in 1924. We exploit variation arising from this sharp and massive restriction to human mobility.

Throughout this period, a group of countries, including Italy, underwent a first wave of industrialization and structural change as part of a broader transformational phenomenon known as the "Second Industrial Revolution" (Mokyr, 1998). The Italian economy, in particular, outperformed the leading industrial countries for the first time between 1895 and 1913. The postwar years, especially the Fascist period, however, were marked by economic stagnation and languishing productivity growth. The economic divide between Northern and Southern areas, which had started to narrow during the economic boom, severely widened during the 1920s and 1930s (Cohen and Federico, 2001). Previous scholarship documents that insufficient investments, especially in the South, hampered the adoption of productivity-enhancing technologies. In this paper, we argue that scarce investments partly came as a consequence of the post-1921 restrictive US immigration policy, which, therefore, plausibly contributed to the widened economic gap between the North and the South of the country and, more generally, to the disappointing performance of the Italian economy.

To identify the effect of the American immigration restriction policy shock on Italian economic development, we define a district in Italy as more exposed to the Quotas if a larger proportion of its emigrants had moved to the United States before 1921, conditional on the overall number of international emigrants. In a difference-in-differences setting, we thus compare districts located in areas with similar emigration outflows and leverage variation among destination countries.[4] Formally, our identification assumption requires that districts with similar volumes of international emigrant outflows but whose emigrants headed toward different destinations would not have undergone diverging development trajectories in the absence of the Quota Acts. We provide a battery of checks to ensure the plausibility of this assumption, but ultimately, we cannot test the conditional exogeneity of treatment intensity. We thus adapt the shift-share instrumental variable design developed by Card (2001) to construct plausibly random variation in the intensity of exposure to the Quotas across Italian districts. The instrument predicts district-level emigration to the US over time by interacting the initial migration flows between Italian districts and US counties with subsequent non-Italian immigration inflows across US counties. The "naïve" and the instrumented difference-in-differences designs yield quantitatively highly consistent results.

The historical setting allows us to overcome several limitations of contemporary scenarios. First, emigration seldom flows into only a few destinations; hence, observing significant restrictive policy shifts is challenging. Second, migration dynamics are often affected by co-evolving regulations enacted by both receiving and sending countries, which were absent during the period we study (Abramitzky and Boustan, 2017). Third, it is often

[4] This intensity-of-treatment design is closely related to the conceptual framework adopted by Abramitzky *et al.* (2023) to study the effect of the Quota Acts on the US labor market.

difficult to retrieve information on emigrants in their home country (Dustmann *et al.*, 2015).

Existing data from official statistics are not suitable for this exercise because (i) digitized US and Italian censuses and complementary historical statistics do not report the origin of Italian migrants at a granular level of spatial aggregation, and (ii) disaggregated indicators of economic performance for Italy remain scarce during this period. We thus construct a dataset that links the administrative records of Italian emigrants who arrived at Ellis Island between 1892 and 1930 to their district of origin, we match a subset of these records to the US full-count population census, and we complement it with newly digitized detailed data from industrial and population censuses.

The empirical analysis proceeds in three steps. First, we establish that the Quota Acts had a tangible impact on Italian districts. Theoretically, it is possible that those who would have migrated to the United States in the absence of the policy shift could move to a different country. Looking at aggregate emigration numbers (Figure 1), this seems implausible. Emigration to the United States completely dried up after 1921, but emigration to other countries did not. The total number of emigrants in the 1920s is roughly comparable to that in the 1880s and early 1890s before the US migration gained momentum.

We formally test this "imperfect substitution" argument through the DiD design and find that the population in districts that were conditionally more exposed to the Quota Acts increased after 1921. The effect of the Quotas on the Italian population is sizable in magnitude. A 1% increase in the number of US emigrants leads to a 0.05% increase in population. Equivalently, moving from the 50th to the 75th percentile of the distribution of exposure to the policy yields 8,800 additional population, relative to an average pre-Quota population of approximately 247,000. The change in population is equivalent to almost 80% of the number of US emigrants between 1892 and 1921. While this figure should be interpreted with caution, for it includes foregone migrants as well as their children who would not have been born if they had migrated, it nonetheless suggests that a substantial share—possibly over 50%—of those who would have migrated remained in Italy after the United States closed the border. This finding is consistent with previous studies documenting the spatial persistence of Italian emigration flows (Gould, 1980b; Brum, 2019; Spitzer *et al.*, 2020). More qualitative cross-country evidence confirms that emigration outflows toward countries that did not promulgate IRPs did not increase. Hence, districts supplying relatively more U.S.-bound emigrants ended up having more "missing" migrants, i.e., people who would have migrated in the absence of the Quota Acts. This mechanism generates a spatially segmented positive labor supply shock.

Our second finding is that manufacturing firms in provinces more exposed to the Quota Acts substantially decreased investment in capital goods. We consistently estimate adverse effects across several measures of capital goods, ranging from firms with at least one engine to the number of installed engines and the power they generate. For instance, a 1% increase in US emigration yields a 0.15% decrease in the horsepower generated by mechanical engines and a 0.26% drop in the energy generated by electrical ones. All sectors within manufacturing exhibit similar decreases in capital investment. How can we reconcile the increasing population with the decreasing adoption of productivity-enhancing production technologies? We propose interpreting these findings through the lens of directed technological change theory (Acemoglu, 2002, 2007). As labor becomes a more abundant production input, firms are incentivized to forego investment in capital goods and employ more labor. In Online Appendix D, we develop a simple theoretical framework in the spirit of Zeira (1998) and San (2023) to show why investment in labor-saving technologies would decrease in response to a positive labor supply shock.

To test this interpretation, we study how employment across sectors reacted to the Quota Acts. First, we explore how agriculture and manufacturing employment responded to the policy shock at the district level. We find that manufacturing employment increases in districts that are more exposed to the IRP shock. Quantitatively, a 1% increase in instrumented US emigration yields a 0.084% increase in manufacturing employment while the effect on agriculture employment—a 0.016% decrease—is not statistically significant. Historical evidence suggests that Italian agriculture in the 1920s was organized as a heavily labor-intensive sector (Cohen and Federico, 2001). It thus seems plausible that manufacturing could be the primary beneficiary of the substantial labor supply shock. Second, we look at how different sectors within manufacturing absorbed the population increase. Analogous to the capital results, we estimate comparable increases in manufacturing employment across industries. Thus, the aggregate and sector-level employment and capital responses to the IRP shock are consistent with the predictions of the directed technical change conceptual framework.

Overall, this paper documents that policies enacted by immigration countries to curtail migrant inflows bear important consequences on countries sending migrants. Foregone emigration, in fact, generates a positive labor supply, which, in turn, dampens technology adoption, thus potentially hampering their long-run prospects of economic growth. Despite the historical setting, the Italian emigration to the United States shares several similarities with contemporary migration flows between developing and developed countries. We thus view our results as informative for the current migration policy debate and for a more comprehensive evaluation of the consequences of immigration restriction policies.

RELATED LITERATURE. This paper is related to three streams of literature. First, we speak to the several contributions investigating the impact of emigration on sending countries, as opposed to the larger literature studying the economic and social effects of immigration (Clemens, 2011). Emigration has been shown to impact wages (e.g., Dustmann *et al.*, 2015), attitudes towards democracy and voting (Spilimbergo, 2009; Batista and Vicente, 2011; Ottinger and Rosenberger, 2023) and political change (Chauvet and Mercier, 2014; Kapur, 2014; Karadja and Prawitz, 2019), the diffusion of novel knowledge (Coluccia and Dossi, 2023), entrepreneurship (Anelli *et al.*, 2023), and social norms (Beine *et al.*, 2013; Bertoli and Marchetta, 2015; Tuccio and Wahba, 2018). We inform this literature by showing that emigration fosters the adoption of labor-saving technologies. We emphasize that this channel operates plausibly independently from human capital accumulation. In addition, we document that this mechanism arises in response to restrictive immigration policies enacted by receiving countries.

Second, we contribute to the literature that studies the relationship between technology adoption and the supply of production inputs. Following the seminal contributions by Hicks (1932 [1963]) and Habakkuk (1962), Hornbeck and Naidu (2014), Clemens *et al.* (2018), and Hanlon (2015) all study historical settings where changes in the availability of labor and other factors of production altered the direction of innovation activity. Lewis (2011) offers similar evidence in a modern setting. Our paper is closest in spirit to Andersson *et al.* (2022), who show that labor-saving innovation emerged in response to migration-induced labor shortages in 19th-century Sweden. We present several novel findings relative to their paper. First, we show that immigration restriction policies generate a positive labor supply shock in emigration countries because potential emigrants do not fully substitute the restricted destination with other countries. Second, we document that a *positive* labor supply shock depresses investment in capital technology. In principle, since a positive labor supply shock may sustain aggregate demand, its impact on investment is ambiguous. Our results, therefore, highlight the centrality of directed technical adoption incentives for firms' decision-making. Third, instead of innovation, we focus on technology adoption, which is the core driver of economic growth and modernization in developing countries (Suri, 2011; Bryan *et al.*, 2014; Juhász *et al.*, 2020). Finally, the episode we study allows us to implement a difference-in-differences estimation strategy whose identification assumption can be evaluated transparently.

Third, by its setting, this paper adds to the literature that studies technical change and the diffusion of novel technologies during the Age of Mass Migration. A growing number of papers examines the short-run (Arkolakis *et al.*, 2020; Moser and San, 2020; Diodato *et al.*, 2022) as well as the long-run (Akcigit *et al.*, 2017; Burchardi *et al.*, 2020; Sequeira *et al.*, 2020) implications of immigration on US innovation. Moreover, we contribute to studies examining the archetypal Italian case of mass migration. Among those, Hatton and Williamson (1998) study the aggregate determinants of Italian emigration. Spitzer *et al.* (2020) validate the Gould (1980) theory, whereby social networks exerted substantial influence on Italian emigration dynamics. Pérez (2021) compares the assimilation dynamics of Italian emigrants to the United States with those who moved to Argentina. Our contribution to this literature is twofold. Methodologically, we present newly digitized district-level data from Italian population and industrial censuses. In terms of new findings, we show that the mass migration was unlikely to have hampered the structural shift toward manufacturing. Our results suggest that the opposite impact prevailed: immigration *restriction* likely hampered economic modernization in Italy.

OUTLINE OF THE PAPER. We structure the paper as follows. Section II describes the Italian mass migration, the policies that shaped it, and the fundamental economic characteristics of early 20$^{\text{th}}$-century Italy. Section III discusses our data-collection contribution and the sources. Section IV describes the difference-in-differences and instrumental variable strategies. In Section V, we present our three sets of results. Section VI concludes.

II Historical Background

II.1 The Italian Mass Migration

The Italian mass migration (1870–1925) was the largest episode of voluntary migration in recorded history (Choate, 2008). Between 1880 and 1913, 17 million —corresponding to 65% of the Italian population in 1900—emigrated; most headed toward continental Europe and the Americas. Along with Ireland, Italy had the highest per capita emigration rate (Taylor and Williamson, 1997). Even though Bandiera *et al.* (2013) document that returns rates were equally among the highest in Europe, the Italian mass emigration has long been recognized as a focal feature of the country's development process (Hatton and Williamson, 1998).

II.1.1 A Short History of the Italian Mass Migration

Italy was a latecomer to large-scale mass migration. Northern European countries had been experiencing substantial population outflows since the 1840s. By contrast, Italy and other Southern and Eastern European countries didn't start experiencing mass emigration until the 1880s. The country's migration patterns over 1870–1925 display substantial time variation. Until the 1880s, its emigration rate remained relatively modest, and most migrants hailed from Northern regions. Prohibitively high transportation costs and prevailing poverty in rural Southern areas largely inhibited migration from the *Mezzogiorno*. During the 1880s, Northerners chiefly moved to neighboring countries on a temporary, seasonal basis (Sori, 1979). The widespread adoption of steamships and an agrarian crisis kicked off the Southern mass emigration (Keeling, 1999). A decade later, the script had flipped: most migrants were now coming from Southern regions. Though the share of migrants from Northern regions declined as the share from Southern regions grew, emigration rates from *both areas* rose steadily from 1870 to 1913 (Hatton and Williamson, 1998). By the 1890s, Italy had become the global leader in sheer numbers of emigrants and in emigration rate, which grew from 5‰ in 1880 to a peak of 25‰ in 1913.

Italian emigration collapsed during World War 1 (WW1) but quickly regained momentum in the years immediately following the war. The epoch ended in the early 1920s when the U.S. Congress enacted restrictive immigration policies that effectively halted mass emigration to the United States. Emigration toward other transoceanic and European destinations nonetheless endured until the outbreak of WW2. In Online Appendix Table B.2, we tabulate data from official statistics on regional out-migration to the United States and other international destinations to gauge the geographical evolution of the Italian mass migration over time.

Internal migration is one last, largely overlooked component of labor mobility in Italy during the Age of Mass

Migration. Current data limitations hinder a quantitative study of internal migration from 1870 to 1925. In the rest of this study, we abstract from explicitly accounting for internal migrations for three reasons (beyond data availability). First, Gallo (2012) shows that internal migrants were easily outnumbered by international migration flows, particularly during the Age of Mass Migration. We provide a quantitative assessment of this claim in Online Appendix Table B.3, which confirms that the number of international emigrants is one order of magnitude larger than that of internal migrants. Second, internal mobility was largely temporary and seasonal, inherently different from transoceanic migration (Gallo, 2012). Third, internal migrations reflected historically deep-rooted, persistent economic relationships between regions unlikely to influence our results on economic modernization in the 1930s.

II.1.2 Composition and Determinants of the Migratory Movements

In the 1880s, Italy was a young nation rife with regional disparities spanning cultural and economic dimensions (Smith, 1997). The resulting geographically segmented migratory patterns largely reflected this substantial heterogeneity and provided our empirical strategy's backbone. Until the early 1880s, most migrants from Northern regions moved to European countries. Most of the rest steamed across the Atlantic to Argentina and Brazil. This pattern is completely reversed for Southern migrants, whose primary destination was the United States.

To explain why destinations with low relative wage gaps, such as Argentina and Brazil, received sizeable migration inflows, Gould (1980b) hypothesizes that local emigration dynamics were driven by information diffusion. Information about emigration opportunities required time to spread across the country, and this diffusion accelerated as the volume of emigration increased. Gould (1980b) provides convincing evidence suggesting that declining regional emigration-rate inequality is consistent with this mechanism. An indirect consequence of the Gould hypothesis is that local emigration rates displayed relatively little sensitivity to economic and demographic conditions, instead featuring high persistence (Hatton and Williamson, 1998). Spitzer and Zimran (2023) further provide evidence consistent with Gould's diffusion hypothesis. They show that emigration began in a few districts in the 1870s and 1880s and subsequently spread to nearby districts over time through immigrants' social networks. In Online Appendix A.III.3, we present some evidence that points in the same direction.

II.2 Migration Policy in Italy and the United States

An appealing feature of this context is that migratory flows from Europe to the United States remained essentially unregulated until 1921 (Abramitzky and Boustan, 2017). This section describes the key features of the historical Italian and U.S. systems of migration laws.

II.2.1 Italian Emigration Policy

Newly unified Italy had virtually no emigration policy until 1873. Occasional, largely ineffective provisions were enacted between 1873 and 1887 that reflected the perceived need to deal with labor agents and recruiters, the so-called *padroni*, but did not form a corpus of migration law (Gabaccia, 2013). The first such attempt was the 1888 Crispi-De Zerbi law, which introduced and regulated the emigration contract between the migrant and the migration agency. However, the law was manifestly inadequate to deal with the waves of migration that unfolded starting in the 1890s, and it effectively failed (Foerster, 1919).

As emigration to the United States gained momentum, a new law was passed in 1901 under the renewed understanding that emigration was no artificial phenomenon and could positively affect Italy (Foerster, 1919). As such, the law sought to protect migrants from exploitation rather than restricting their movement. The law established a Commissioner-General of Emigration to oversee the protective institutions and collect migrant data. Only companies licensed by the Commissioner-General could sell tickets, whose rates were reset every three months. Comparatively minor subsequent legislation further protected remittances (1901), strengthened the authority of the Commissioner-General (1910), and regulated citizenship (1913) (Rosoli, 1998).

II.2.2 American Immigration Policy

The United States, for its part, maintained an open border between 1775 and the early 1920s, interrupted only by isolated outbreaks of anti-immigration policy interventions. During the Age of Mass Migration, some 30 million migrants entered the United States. By 1910, 22% of the labor force was foreign-born, the highest share ever since (Abramitzky and Boustan, 2017). In 1907, the United States Congressional Joint Immigration Commission, also known as the Dillingham Commission after its chairman, was formed to study, among other things, immigrants' economic and social conditions. The Commission's 41-volume report favored "old" immigration countries such as England and Germany over "new" ones, mainly Southern and Eastern Europe.

When immigration ramped up again after WWI, nativist demands for restrictions surged, and the Emergency Quota Act was passed in 1921. It was modified by the 1924 Immigration Act, which further tightened immigration restrictions on second-wave countries. The 1921 Emergency Quota Act envisaged a (temporary) annual quota of 360,000 immigrants from Europe.[5] Importantly, for our identification, entry quotas were assigned to each country as 3% of that country's nationals living in the United States in 1910, as recorded in that year's census. The 1924 Immigration Act made the quota system permanent, lowered the inflow from 3% to 2%, and shifted the census baseline year to 1890. The last provision, in particular, targets Southern and Eastern European countries that took part in the Mass Migration starting in the late 1890s, as advised by the report of

[5]U.S. immigration peaked in 1907 at 1,285,349 entrants. The number of entrants during the 1910s averaged around 800,000.

the Dillingham Commission. Abramitzky *et al.* (2023) note that the impact of the 1924 Immigration Act on immigration was highly heterogeneous across sending countries. Flows from Southern and Eastern Europe were heavily curtailed because the share of foreign-born individuals from those countries who lived in the United States in 1890 was very modest. Since the 1890s, the United States had been absorbing 30% to 40% of all Italian emigration, so the Quota Acts represented a significant policy shock for Italy.

II.3 Technology Adoption and Economic Growth in Italy

Italy entered the Age of Mass Migration in the 1880s. The country was amid an agrarian crisis that followed two decades of stagnation (Toniolo, 2014). The period from 1895 to 1913 was the only time until the 1950s "economic miracle" in which Italy outperformed and narrowed the income gap with the leading industrial nations. Still in the 1920s and 1930s, however, during the Fascist period, Italy remained a mainly agricultural country, featuring low income per capita and languishing productivity growth (Cohen and Federico, 2001). During the first half of the Fascist *Ventennio*, economic policy was aimed primarily at fiscal and monetary consolidation. Agricultural policy—which formed an integral part of the Fascist propaganda—centered on boosting agricultural productivity, which had been stagnating since WW1, and draining marshlands. However, sheer numbers attest that agrarian policies resulted in neither substantial intervention nor sizeable progress (Zamagni, 1993). Growth slowed after 1925, and regional disparities widened (Cohen and Federico, 2001). Historical evidence is thus consistent with our finding that following the 1921–1924 U.S. emigration restrictions, Italy underwent a period of economic distress and rising North-South economic inequality.

We relate the migration shock to diminished investment in capital goods, especially technologically advanced ones, and a shift to labor-intensive production routines. Italy operated far from the technological frontier throughout the period, and skill premia *declined* from the 1890s onward (Vasta, 1999). Similarly to contemporary developing countries, Italy lagged behind advanced industrial nations in research-and-development expenditures, and it imported substantial amounts of foreign technology, both patents and machinery. Italian firms bundled different vintages of capital whenever possible, adding new machines to existing ones instead of renovating the whole stock (Cohen and Federico, 2001). The large pool of unskilled workers made it more profitable for Italian entrepreneurs to adopt labor-intensive technologies relative to the highly capital-intensive German and British ones. Consistent with this narrative, the migration policy shock increased the stock of unskilled workers in regions with high emigration. There, firms opted out of investment in capital goods and became more labor-intensive, thus hampering the modernization process they had been undergoing before the Quota Acts.

III Data

Our analysis spans the years 1881 to 1936. We collect data from several sources; data are organized by census years and analyzed at the *circondario* (henceforth, "district") level if available. Otherwise, the units of observation are provinces.[6] In 1921, there were 216 districts and 69 provinces, each consisting of a variable number of municipalities (see Online Appendix section A.I for a complete description of the data). Because districts were abolished in 1927, all subsequent district-level data are collected at the municipality level and aggregated at the 1921-district boundaries. We adopt the geographical cross-walk procedure described by Eckert *et al.* (2020) to ensure that we work with consistent 1921-border district and province geographies. Table 1 reports summary statistics for the variables in our final dataset. Online Appendix Table A.1 lists the source and coverage of each variable in the final dataset.

III.1 Emigration Data

Italian official emigration statistics provide insufficient information because out-migration flows by destination were recorded at the province level (Hatton and Williamson, 1998). This poses a major challenge because we would rather work with more granular geographical units than provinces whenever possible. This limitation precludes the use of official statistics for a spatially-detailed econometric exercise. In addition, official statistics typically relied on issued passports, which were not required to emigrate to the US. We nonetheless digitize province-level emigration outflows from official statistics and use them to construct overall emigration rates and validate the series we derive from the individual dataset we assemble.[7]

To overcome these issues, we collect administrative records of Italians who entered the country between 1890 and 1930 through the Ellis Island immigration station.[8] This was by far the largest, though not the only, immigration gateway during this period.[9] Administrative records report, for most migrants, name and surname,

[6] Population censuses were taken in 1881, 1901, 1911, 1921, 1931, and 1936. Manufacturing censuses were taken in 1911, 1927, and 1936. Districts were instituted in 1859 as the middle administrative unit between municipalities and provinces. They had mainly statistical and judiciary purposes and were granted little administrative autonomy. Districts were organized in provinces, which encompassed one to five districts. In the Online Appendix section A.I, we discuss the sources we digitized in more detail and present a visual summary of all the variables we analyze. In Online Appendix A.II, we describe how the final estimation samples are constructed.

[7] We construct district-level emigration rates from official statistics. To compute a district's emigration rate, we multiply the province-level number of emigrants by the share of that district's population. In other words, we assume that the emigration rate is constant among districts in the same province.

[8] These records are freely available at `heritage.statueofliberty.org`. We run queries over a comprehensive pool of 20,000 Italian surnames over the 1890–1930 period. In Online Appendix section A.III.2, we document that our constructed series correlates well with existing—albeit less granular—emigration data from official statistics.

[9] According to official U.S. statistics, between 1892 and 1924, a total of 14,277,144 migrants entered the country through Ellis Island, out of a total immigration inflow of 20,003,041 (Unrau, 1984, p. 185). Thus, Ellis Island alone accounted for 71.4% of the total immigrant

year of arrival, age, municipality of origin, and sailing ship. This study concentrates on the migration year and the municipality of origin. Ultimately, we collected approximately 2.7 million individual observations from 1890 to 1930.

The municipality was recorded for the vast majority of the records—more than 80% during the peak immigration years, as shown in Appendix Figure A.1—but it displays frequent coding errors. We perform manual and automated trimming of the raw data and geo-code each municipality to precise geographical coordinates. We can match 1.6 million migrants to their municipality of origin. Among those, 800,000 municipalities are coded with no error. We then map each municipality to the district it belonged to in 1921 through historical GIS boundary files and compute the resulting aggregate U.S. emigration.[10] In Appendix Figure A.2, we compare the resulting dataset with more aggregate province-level figures tabulated from official statistics and find that the two are remarkably consistent. The positive correlation remains stable and highly statistically significant throughout the sample period (Appendix Figure A.3)[11] To the best of our knowledge, the Ellis Island repository is the most comprehensive source spanning the whole Age of Mass Migration for Italy at this level of aggregation.[12] Similar data have been assembled by Gray *et al.* (2019) and Spitzer and Zimran (2023).[13] Spitzer and Zimran (2018) relied on a small subset of the overall Ellis Island data in their seminal contribution.

To conduct a Bartik-type instrumental variable analysis, we link the individual-level Ellis Island immigration records for 1895–1900 with the full-count US population census (Ruggles *et al.*, 2021). To our knowledge, ours represents the first attempt at linking census records with the Ellis Island administrative data. Concretely, we link individuals with records of Italians who appear in the 1900 census on their name, surname, and immigration year. We detail the procedure in Online Appendix section A.III.4. Using this linked sample, we attach a municipality of origin to Italian migrants, allowing us to compute migration flows from Italian districts to US counties.

Figure 1 plots the overall country-level yearly inflow of immigrants who landed in Ellis Island from 1892 to 1930. Emigration took off in the mid-1890s and peaked between 1905 and 1913. It collapsed during World War 1 (WW1), quickly regained momentum in 1920, and was definitively shut down by the Quota Acts in 1921 and 1924. Our data are consistent with comprehensive U.S. immigration data and overall Italian migration patterns (Brum, 2019; Sequeira *et al.*, 2020). In Figure 2, we plot the geographical distribution of migrants

inflow. Some 95% of all Italian immigrants passed through Ellis Island.

[10] Appendix A.III.1 discusses the methodological details, including the frequency of missing data on immigrants' municipality of origin.

[11] Online Appendix A.III.2 provides a more detailed description of the validation exercises.

[12] Brum (2019) produced a similar dataset for the pre-1900 period.

[13] Compared to Gray *et al.* (2019), who list 4.8 million observations, we recover a smaller proportion of migrants because we do not allow for fuzzy matches. This choice is motivated by the fact that the fuzzy match tool provided by the database search engine returns very distant and only vaguely similar results for the given queried surname. By only including exact matches, we ensure that our database does not comprise these (potentially) spurious observations.

across districts. Panel 2a displays the number of international emigrants across provinces. Panel 2b reports the volume of US out-migration across districts. Out-migration flows exhibit substantial dispersion across provinces and are not clustered in any specific area of the country. Emigration to the United States appears to be more clearly concentrated in Southern districts. Online Appendix A.III.3 presents further stylized facts that the data allows documenting.

III.2 Population and Manufacture Censuses

The Italian Statistical Office (ISTAT) compiled the population of each municipality from the historical population censuses. We aggregate these tabulations by district and province for each census between 1881 and 1936.[14] We compute the k-urbanization rates as (i) the share of the population residing in cities with at least k-thousand inhabitants and (ii) the share of cities with at least k-thousand inhabitants. These tables also contain the altitude and area of each municipality and an indicator variable returning value one for towns near the sea. We collapse the first two at the district and province levels, weighting them by municipality population and tagging districts and provinces with access to the sea.

We construct manufacturing and agriculture employment series from disaggregated data listed in census records. These are available at the district level before 1921 and the municipality level afterward. For consistency, we recast them at the district level throughout the sample period. Employment in agriculture was not reported in the 1931 population census. Until 1921, population censuses contain sector-level employment data for manufacturing firms at the district level. After that, the same variables are reported in the manufacturing census at the province level. Thus, we can observe sector-level manufacturing employment at the province level throughout the sample period.

Manufacturing censuses contain detailed information on the quantity and quality of capital employed in each province by manufacturing firms in 1911, 1927, and 1937. We collect within-manufacturing sector-level data on (i) the number of operating firms, (ii) the number of operating firms employing inanimate horsepower, (iii) the number of mechanical engines, (iv) the number of electrical engines, (v) the amount of horsepower generated by mechanical engines, and (vi) the amount of horsepower generated by electrical engines. We distinguish between electrical and mechanical engines because the former were at the forefront of technological progress (David, 1990). This allows us to disentangle the possibly differential impact of the labor supply shock induced by the migration shock on different technology vintages.

[14] Popoulation censuses were taken in 1881, 1901, 1911, 1921, 1927, and 1936. Manufacture censuses were taken in 1911, 1927, and 1936. Data in manufacturing censuses are only available at the province level.

III.3 Other Data

Italy participated in WWI between 1915 and 1918. Because the war took place between two census years and ended just three years before the Emergency Quota Act, it can potentially confound our estimates. We, therefore, collect WW1 death records to measure the geographical variation in the cost imposed by the war across districts.[15] The dataset provides rich information on Italian military personnel who died during WWI. Importantly for our analysis, it includes the municipality of origin of each soldier. Because we conduct the analysis at the district level, we collapse the dataset from municipalities to 1921 districts, and we measure the war's severity in a given district as the ratio between deaths and population in 1910.

In robustness checks, we use aggregate information on US GDP and industrial production as further controls. These are constructed by Maddison (2007) and Davis (2004), respectively.

To account for changes in market access, we digitize the entire Italian railway network over the 1839–1926 period.[16] We know all the stations it is connected to for each railway section. Stations are generally labeled according to the municipality in which they are located. Further details are included for stations located in municipalities with more than one station. We also know the exact date when each trunk was built and opened to public use, the distance it covered, and the train's traction system. We use these data to construct the Italian railway network. To capture its evolution over time, we take snapshots of the network at decade frequency.

IV Empirical Strategy

This section presents the empirical strategy we adopt to identify the effects of the Quota Acts and provides evidence supporting the research design's validity.

IV.1 Measuring Exposure to the Quota Acts

The empirical analysis hinges on the observation that areas whose emigrants were more likely to settle in the United States before 1921 would be more exposed to the Quota Acts. This implicitly assumes that emigrants do not perfectly substitute the United States with other—internal or international—destinations. The first step

[15] Death records were compiled by the Fascist regime for propaganda purposes. They are available at cadutigrandeguerra.it. This dataset is maintained by the *Istituto per la storia della Resistenza e della società contemporanea*. Acemoglu *et al.* (2022) were among the first to use it in the economics literature.

[16] The data come from the volume *Sviluppo delle ferrovie italiane dal 1839 al 31 dicembre 1926*, edited by the Italian Statistical Office (*Ufficio Centrale di Statistica*) in 1927. To our knowledge, this is the first paper to use these data. Compared to Ciccarelli *et al.* (2021), we do not have access to the geography of historical railway routes as we only digitize the list of stations and the year when each trunk was opened. The advantage of our data is that we can reconstruct the network until 1924, which marks the end of the transatlantic emigration, while previous studies focus on the period until 1913.

of the analysis validates this assumption.

In principle, districts with more emigrants headed toward the United States before 1921 would be relatively more exposed to the Quota Acts shock. However, if we were to leverage unconditional variation in the number of US emigrants, we would compare districts with very different pre-treatment emigration intensities. If the decision to emigrate were correlated with other—possibly unobserved—characteristics, then the resulting estimates would conflate this underlying spurious association.

Our identification strategy tackles this issue explicitly. To estimate the impact of the Quota Acts on Italian economic development, we compare districts located in provinces with similar volumes of international emigrants and leverage variation in the volume of emigration flows towards the United States. Both variables are computed as the cumulative number of people who emigrated between 1892 and 1921. Before 1892, the officials at Ellis Island did not record the municipality of origin. We set 1921 as the final year because, in 1921, the United States enacted the first restrictive Quota Act.[17] Formally, we thus leverage variation in the number of emigrants towards the United States, conditional on the overall intensity of international emigration outflows.

IV.2 Baseline Difference-in-Differences Model

Throughout the paper, we estimate variations of the following double-differences (DiD) Poisson quasi-maximum likelihood model (PQML):[18]

$$\begin{aligned} \ln \mathbb{E}\left(y_{it} \mid \mathbf{X}_{it}\right) = \alpha_i + \beta_t + \sum_{\tau \neq 1911} \gamma_\tau \times \text{Emigrants}_{p(i)} \times \mathrm{I}(t=\tau) + \\ + \sum_{\tau \neq 1911} \delta_\tau \times \text{US Emigrants}_i \times \mathrm{I}(t=\tau), \end{aligned} \tag{1}$$

where $\mathbb{E}(\cdot)$ denotes the expected value of a generic outcome y conditional on a set of baseline controls $\mathbf{X}$; terms α_i and β_t capture, respectively, unit and time fixed effects, and $\mathrm{I}(\cdot)$ are indicators for each period since the Quota shock. The term (US Emigrants) is the log number of migrants who left district i and moved to the United States between 1892 and 1921. The baseline period is the year of the last census before the Quotas (1911). Depending on the outcome y, the included periods are either the population censuses (1881, 1901, 1911, 1921, 1931, 1936) or the manufacturing censuses (1911, 1927, 1937).

In (1), we employ the PQML estimator to account for the non-normality of the outcome and the presence of zeros, particularly when looking at capital investment outcomes. Importantly, equation (1) controls for an

[17] In several robustness checks, we further restrict the sample period to exclude the years 1892–1900, when municipalities in the Ellis Island dataset are recorded less frequently, the war years (1915–1918), and the post-war period (1915–1924). The results remain quantitatively unchanged through these different sample restrictions.

[18] The key advantage of the PQML estimator is that it remains consistent when dealing with non-negative dependent variables in the presence of fixed effects without requiring to model the underlying distribution explicitly (Correia *et al.*, 2020).

interaction term between the cumulative log number of emigrants over the period 1892–1921 ($\text{Emigrants}_{p(i)}$) and a set of indicator variables coding the years since the Quotas are enacted.[19] This ensures that the DiD estimators $\{\delta_\tau\}$ compare districts located in areas with comparable emigration intensities.[20] In the baseline analysis, $\mathbf{X}$ contains an indicator variable for southern regions interacted with a post-Quota indicator to account for potential diverging North-South trends after the Quotas. In several robustness checks, however, we enrich the included controls. Since districts are quite heterogeneous in population, we weigh them by their 1881 population in all regressions to ensure that very small areas do not drive the results. Standard errors are clustered at the level of the treatment relevant to each regression: districts for district-level regressions and provinces for province-level regressions.

The identification assumption that model (1) requires can be stated in terms of conventional parallel trends. Absent the Quotas, units with a conditionally higher number of US emigrants would not have displayed different patterns in y compared with districts with fewer relative US emigrants. While this assumption is not testable, we estimate pre-treatment coefficients that are never statistically different from zero for population (Figure 4) and manufacturing and agriculture employment (Figure 8). These exercises support the parallel trends assumption. We defer a more detailed discussion on the plausibility of the assumption of parallel trends to the following section.

In Table 2, however, we provide one additional exercise to gauge the validity of the research design. In each line, we report the correlation between district-level variables from population censuses (Panel A), province-level manufacturing employment by sector (Panel B), province-level capital variables (Panel C), the number of US emigrants (in columns 1–6), and the instrumental variable defined in (2). In columns (1–2) and (7–8), the variables are measured in 1901; in columns (3–4) and (9–10), they are measured in 1911. Importantly, identification in a difference-in-differences setting requires that the treatment does not correlate with *changes* in pre-determined characteristics, which may impact the outcomes of interest. In columns (5–6) and (11–12), we thus compute the correlation between the growth rate of each variable between 1901 and 1911 and the two treatment indicators. These indicate that observation units appear comparable along a majority of all observed variables in growth rates despite substantial level differences. We cannot repeat this exercise for the capital variables since we do not observe two pre-treatment values. In Panel C, we thus report the correlation between the treatment and their levels in 1911 for completeness.

To avoid the pitfalls of the DiD estimator with continuous treatments reported by Callaway *et al.* (2021), we

[19]Unfortunately, district-level data on the number of international emigrants, to our knowledge, do not exist. However, controlling for emigration at the province level allows us to mitigate the concern that transatlantic emigration from a given district could be compensated by internal mobility from the other districts in the same province.

[20]All results remain qualitatively unchanged if we estimate the baseline specification (1) as a log-log linear regression instead of a PQML model, and upon normalizing the volume of emigrants by 1881 district population.

always report estimates of model (1) where the intensive margin is coded as a binary variable returning value one for districts above the median value, and zero for districts below the median.

IV.3 Instrumental Variable for Quota Exposure

To provide more solid evidence in favor of a causal interpretation of our estimates, we construct a shift-share instrument to predict US emigration across Italian districts. Our approach mirrors a widely employed methodology to estimate the causal impact of immigration (Card, 2001; Tabellini, 2020). The instrument predicts district-level out-migration flows to the United States by interacting the 1895–1900 migration ties between Italian districts and US counties with subsequent county-level immigrant inflows from countries other than Italy. Formally, the actual US emigration is instrumented with

$$\text{US }\widehat{\text{Emigrants}}_{it} \equiv \sum_{j} \omega_{ij} \times \text{Immigrants}_{jt}^{-\text{Italy}}, \tag{2}$$

where ω_{ij} denotes the number of immigrants from district i into county j between 1895 and 1900, while the variable ($\text{Immigrants}_{jt}^{-\text{Italy}}$) is the number of non-Italian immigrants who settle in county j in year t, expressed as a share of the overall number of non-Italian immigrants who enter the US in year t. To compute the exposure share terms (ω_{ij}), we rely on our novel dataset that links Ellis Island immigrant records with the full-count US population census (Ruggles *et al.*, 2021). We construct bilateral flows between districts and counties by attaching a municipality of origin to Italian immigrants recorded in the US census. In regression (1), we substitute the baseline treatment with the instrumented number of US emigrants defined in (2) and control for the number of overall international emigrants, as in the baseline case.

Our key identification assumption when using the instrumental variable in the double differences setup is that the initial sorting of emigrants across US counties, weighted by the pull shocks represented by non-Italian immigration inflows, does not correlate with unobserved factors that may impact the *changes* in the outcome variables in the same periods. In contrast to a standard instrumental-variable estimation setting, we do not require that the instrument does not correlate with the *levels* of such variables, as in Anelli *et al.* (2023). While this assumption cannot be tested, in Table 2, we show that the instrument does not systematically correlate with changes in the outcome variables before the Quotas. This exercise supports the validity of this research design.

In Figure 3, we check that actual and predicted US emigration flows are positively correlated. We compute the residualized values of observed and instrumented number of US emigrants by regressing each variable against province-fixed effects, which implicitly control for the province-level emigration rate. We then plot the residuals and highlight the binned values in blue. This exercise reveals a strong, positive, and significant correlation between the instrument and observed US emigration. We provide more detailed tabular evidence in Online Appendix Table B.1.

V RESULTS

We organize the results in three logically distinct sections. First, we show that the immigration restriction policy generated "missing migrants" who remained in Italy. Second, we explore its effect on technology adoption and investment in physical capital. Finally, we show that immigration restriction policies generate a positive labor supply shock in manufacturing, dampening firms' incentive to invest in labor-saving technologies. We conclude by discussing the limitations of the analysis and possible avenues for future research.

V.1 POPULATION INCREASES IN DISTRICTS MORE EXPOSED TO THE QUOTA ACTS

The first step of our argument maintains that areas more exposed to the US Quota Acts experienced an unexpected population increase. Implicitly, this requires that not all those who would have migrated to the United States had the Quotas not been promulgated would settle in a different country. This "imperfect substitution" argument can be tested and quantified empirically. If the US and other countries were perfectly substitutable, we would expect to find no effect of exposure to the Quotas on population.

To assess the validity of the imperfect substitution hypothesis, we estimate model (1) using population as the outcome variable. Table 3 reports the results. We find that districts more exposed to the IRP shock display a larger population after 1921, using the observed exposure to the US quotas (columns 1–2) or the instrument (columns 3–6). Importantly, in columns (2) and (4), we include region-by-time fixed effects to account for time-varying unobserved heterogeneity at the district level. In column (5), we repeat the estimation using a binary treatment for districts below and above the median US emigration rate to avoid issues related to continuous treatment DiD designs. The effect remains quantitatively unchanged if we focus on the sub-sample of Southern districts, where exposure to the Quotas was generally higher (column 6).[21] All these specifications yield quantitatively consistent results. For instance, as shown in column (3), a 1% increase in the number of instrumented US emigration leads to a 0.05% increase in population. Equivalently, moving from the 50th to the 75th percentile of the distribution of predicted US emigration yields 8,860 additional population, compared to a pre-treatment average equal to 247,000.[22] The increased population due to the Quotas amounts to almost 80% of the number of US emigrants between 1892 and 1921. This share should be interpreted with caution, for it includes foregone migrants *and* their children who would not have been born in Italy had they not migrated.

[21] By "Southern" districts, we mean areas in the NUTS-2 ITF and ITG regions, which correspond to the former Kingdom of the Two Sicilies.

[22] We employ a Poisson quasi-maximum likelihood estimator. Since the main treatment variable is the log of the number of US emigrants, the associated $\hat{\delta}$ coefficient quantifies the log change in the outcome given a one log-unit change in the treatment. Thus, considering column (3), a 1% change in the predicted Quota Exposure approximately yields a 0.05% increase in population. The absolute population change is then $Y_{\text{post}} - \exp(\hat{\delta} \times (\log \text{US } \widehat{\text{Emigrants}}_{75} - \log \text{US } \widehat{\text{Emigrants}}_{50})) \times Y_{\text{pre}}$, where US $\widehat{\text{Emigrants}}_k$ is the value of US emigration at the k-th percentile, and Y_{pre} and Y_{post} are the average pre- and post-treatment outcomes.

Overall, however, we find that the share of those who would have migrated to the United States and remained in Italy was substantially, possibly over 50%.

We also report the coefficient of an interaction between the number of international emigrants over the period 1890–1921 and a post-Quota indicator. While this term cannot be interpreted causally, it remains informative about the validity of the research study. The core observation is that emigration toward countries other than the United States was not restricted. We thus expect that the population in districts with relatively more intense out-migration flows would decrease following the Quotas. The empirical evidence confirms this claim. The coefficient of the interaction term between out-migration and the post-treatment indicator is negative and generally significant, indicating that emigration towards non-US destinations did not generate "missing migrants" in the years following the Quotas.

The validity of the DiD design hinges on a standard assumption of parallel trends. This requires that if the US had not promulgated the Quota Acts, the population would have evolved similarly in districts with relatively more or less intense US emigration. To gauge the sensibility of this assumption, we estimate model (1) as a generalized DiD using the instrument interacted with time dummies. Figure 4 reports the estimated dynamic treatment effects. We do not find any statistically significant correlation between predicted US emigration and population before the Quotas were enacted. This pattern supports the parallel trends assumption. We estimate a positive effect of the Quotas already in 1921, which persisted and grew in magnitude over time. The immediacy of the impact of the Quotas is not surprising and is motivated by two factors. First, the Quotas were enforced immediately, and immigration after 1921 swiftly decreased, as shown in Figure 1. More importantly, however, it reflects that during World War 1, international mobility collapsed. Areas that would be more exposed to the Quotas were also more exposed to the sharp drop in cross-country mobility due to the War. The 1921 population increase is thus plausibly a consequence of the War rather than the Quotas *per se*. The effect of the Quotas would, in turn, compound over time because every year, additional foregone migrants would remain in Italy and contribute to increased population. The pattern of the dynamic response of the population to the Quotas, which increases over time throughout the sample period, is entirely consistent with this prediction.

As we already mentioned, Gould (1980) and, more recently, Spitzer and Zimran (2023) document an S-shaped pattern in the evolution of US emigration at the municipality level. US emigration takes off slowly and rapidly increases until a saturation point, after which it remains relatively flat.[23] This pattern suggests that the district-level exposure to the Quota Act would depend on when they reached this inflection point. Areas that entered the transatlantic mass migration earlier would be less exposed to the Quotas than districts with more recent out-migration flows. In Figure 5, we evaluate the empirical standing of this prediction. In Panel 5a, we estimate the baseline difference-in-differences regression and include an interaction term between the Quota exposure

[23] Our data corroborate these empirical findings, as shown in Appendix A.4.

treatment and a set of indicators coding the quintiles of US out-migration during the first half of the sample period, between 1892 and 1906. In Panel 5b, we perform the same exercise, but the quantiles are computed over the distribution of US emigrants in the second half of the sample period (1907–1921).[24] Areas with relatively larger US out-migration flows in the early years of the sample period do not display an increased population following the Quotas. By contrast, in areas where US emigration was more intense in the years that more immediately preceded the Quotas, we estimate a larger population response. These patterns are highly consistent with the findings of Spitzer and Zimran (2023). The impact of the Quotas was larger in areas where US emigration was more intense and temporally closer to the enactment of the Act.

V.2 Capital Investments Decrease in Districts More Exposed to the Quota Acts

How did the increased population interact with technology adoption and investment? This section shows that investment in labor-saving and possibly productivity-enhancing technologies decreased in areas more exposed to the shock. To do so, we estimate model (1) using several proxies of capital investment as outcome variables. We distinguish between the overall number of firms, the number of firms with at least one installed engine, mechanical and electrical engines, and the mechanical and electrical horsepower generated by the respective installed engines. The data cover manufacturing firms only.

Table 4 reports the estimated effect of exposure to the Quotas on capital investment. Panel A refers to the observed US emigration, whereas in Panel B, the treatment is the predicted values from (2). We find that investment in physical capital decreased in areas more exposed to the Quotas. This finding is confirmed across all the imperfect proxies available. In particular, we estimate larger treatment effects for electrical engines and horsepower. This is particularly striking, as David (1990), Mokyr (1998), and Reichardt (2024) note that electrical engines stood as a major defining technology of the Second Industrial Revolution, which could-yield sizable productivity advantages. Gaggl *et al.* (2021), moreover, show that electrification in the US was a catalyst for urbanization and structural transformation. In terms of magnitude, the estimated effects are sizable. Moving from the 50th to the 75th percentile of the distribution of predicted US emigration yields (in parentheses, we indicate the corresponding pre-treatment average value): 420 fewer firms (4,707), 135 fewer firms with engines (937), 36 fewer mechanical engines (702), 435 fewer electrical engines (1,511), 2,392 less mechanical horsepower (16,115), and 2,999 less electrical horsepower (12,326).

[24] Formally, denote with ξ_i the quantile of US emigrants over either sample period in district i. The empirical specification is:

$$\ln \mathbb{E}(y_{it}\,|\,\mathbf{X}_{it}) = \alpha_i + \beta_t + \gamma \times \text{Emigrants}_{p(i)} \times \text{I}(t \geq 1921) + \delta_1 \times \text{US Emigrants}_i \times \text{I}(t \geq 1921) + \\ + \sum_{k=2}^{5} \delta_2^k \times \text{US Emigrants}_i \times \text{I}(t \geq 1921) \times \text{I}(\xi_i = k).$$

We estimate the regression twice. In the first case, the quantiles $\{\xi_i\}$ are computed on the distribution of US emigrants between 1892 and 1906. In the second, they refer to US emigration between 1907 and 1921. The Figure displays the coefficients $\{\delta_k^2\}_{k=2}^5$ for the two specifications, and the first quantile serves as the baseline category.

Figure 6 displays the associated dynamic DiD coefficients. Unfortunately, the data are available for three points in time only. Of these, 1911 is the only pre-treatment observation. We thus cannot estimate pre-treatment coefficients. Instead, in Table 2, we show that treated and control provinces were similar along all outcome variables except mechanical engines in 1911. All variables decreased in 1927: the effect remains persistent for electrical engines, while it appears to be shorter-lived for mechanical ones.

Thus far, we grouped all manufacturing sectors. Nonetheless, the data allows us to undertake a more disaggregated analysis. We report the results in Figure 7. Each "Capital" panel reports the estimated effect of the Quotas on each capital indicator for a given manufacturing sector. The reaction to the population shock does not exhibit sizable heterogeneity across industries. Capital investment decreases in all sectors except for chemical manufacturing.

V.3 The Quota Acts as Passive Labor Market Policies

Why did investments in capital and technology decrease in areas more exposed to the Quota Acts? We advance and validate the hypothesis that the immigration restriction shock triggered directed technical adoption mechanics *à la* Zeira (1998) and Acemoglu (2002). Under the imperfect substitution hypothesis, which we documented in section V.1, areas that had been sending relatively more migrants to the United States are, in fact, subject to a larger labor supply shock because a relatively larger fraction of people who would have emigrated are prevented from doing so. This positive labor supply shock decreases wages, which triggers firms' incentive to substitute capital with labor. We formalize this argument in Online Appendix section D.

To test this mechanism more formally, we study how manufacturing and agriculture employment reacted to the shock. We look at manufacturing and agriculture because they account for more than 80% of overall employment. In section II, we noted that Italian agriculture in this period was labor-intensive and not mechanized (Cohen and Federico, 2001). The directed technical adoption narrative would thus predict that the labor supply shock generated by the Quota Acts would primarily flow into increased manufacturing employment.

In Table 5, we explore the effect of the Quota Acts on manufacturing employment, while in Table 6, we focus on agriculture employment. We find that manufacturing employment increases in areas more exposed to the immigration restrictions shock, while employment in agriculture does not exhibit similarly detectable changes. In both tables, in columns (1–2), we report the results using observed US emigration, while in columns (3–6), we employ the instrumented treatment. Column (5) refers to a binary treatment that codes as treated all districts with above-median US emigration, whereas in the other columns, we employ the continuous measure. For manufacturing employment, the OLS estimates are not statistically significant because, as discussed below, we find a statistically significant correlation between the treatment and the outcome before the treatment period; by contrast, the IV-DD estimates are highly significant with no evidence of similar pre-trends. Importantly, the results remain unchanged when the sample is restricted to the sub-sample of Southern districts (column 6).

Since manufacturing employment increases while agriculture employment does not, the labor supply shock generated by the Quotas prompted a reallocation of labor from agriculture, a largely labor-intensive activity, to manufacturing. Overall, the evidence presented in the table provides solid evidence that manufacturing employment increased in areas more exposed to the Quotas. From a quantitative perspective, moving from the 50th to the 75th percentile of the distribution of predicted US emigration yields 2,332 more workers employed in manufacturing (compared to a pre-treatment average equal to 35,121) and 652 fewer agriculture workers (compared to a pre-treatment average of 53,599), although the latter effect is not statistically significant. Crucially, the positive labor supply shock generated by the policy shift did not, however, foster structural change. Employment in agriculture in more exposed areas hardly reacts to the shock. Therefore, the increase in manufacturing employment plausibly reflects the fact that "missing migrants" take jobs in manufacturing rather than a reallocation of jobs from agriculture into manufacturing. It thus appears unlikely that the Quota Acts could prompt the modernization of Italian rural areas.

The historical literature views Italian emigration as a response to over-population in rural areas (Choate, 2008). We evaluate these claims by reporting the coefficient of the interaction between out-migration before the Quotas and a post-treatment indicator. While this term has no causal interpretation, it reveals important patterns related to the motives underlying the migration choice. Employment in manufacturing does not change in districts with relatively more emigrants after the Quota shock. By contrast, agriculture employment decreases. Since out-migration towards countries other than the United States was not curtailed, these patterns appear consistent with the historical literature. Out-migration was prevalent in rural agricultural areas, and thus, agricultural employment decreased in regions with more intense (non-US) out-migration flows after the Quotas.

We report the flexible difference-in-differences estimates in Figure 8. We do not estimate statistically significant pre-treatment coefficients for manufacturing employment, thus providing further solid support for the assumption of parallel trends. Manufacturing employment increased after the Quotas. The effect is relatively persistent as it lasted at least until 1931, although it peaks immediately after the immigration restriction shock. It is worth noting that this pattern is not inconsistent with the increasing effect of the Quotas on population over time documented in Figure 4. Since most emigrants were working-age men, it is plausible that the response of manufacturing employment to the Quotas would be more immediate than that of the overall population, which comprises the children of those who would have migrated and were born after 1921. We do not find any statistically significant response of agricultural employment, and we thus omit the associated event study.

We can break up manufacturing employment by sector, even though only at the coarser province level of aggregation. As in section V.2, we do not uncover substantial heterogeneity across sectors. Employment increases in all industries except mining, where geographical constraints may constrain the employment response. These findings confirm that a positive labor supply shock is associated with decreased capital investment in all manufacturing sectors.

V.4 Robustness Checks

This section summarizes the robustness checks in Appendix C. We provide evidence that the baseline estimates are robust to alternative definitions of US emigration, to the inclusion of several covariates controlling for both push and pull factors, to the exclusion of specific parts of the sample, and to using different methods to estimate the standard error.

Since observed US emigration may be subject to mismeasurement, in Tables C.1, C.2, C.3, and C.4 we show that the results are robust to alternative definitions. The first concern is that our results might be driven either by remote migration patterns or by more recent migration closer to the introduction of the quotas. We address these concerns by constructing two measures of US emigration that assign increasing or decreasing weights to more recent out-migration flows to the US. We consider alternative definitions of the stock of US emigrants depending on the year when they migrated. First, we include all emigrants until 1924, i.e., until the inflow of Italians in the US completely dries up following the Johnson-Reed Act. Second, we construct the treatment using migration to the US before the first Quota Act in 1920. Last, as discussed in Section III, though emigration collapsed during WW1, it did not completely dry out. During the war, districts closer to emigration ports are disproportionately represented relative to previous shares, possibly because of their geographic position. To ensure that this pattern does not determine our results, we restrict the sample of US emigration to the years before the outbreak of WWI. In all cases, we find that all our baseline results hold.

In Tables C.5, C.6, C.7, and C.8 we show that our baseline results are robust to the inclusion of a large set of covariates measured before the Acts, interacted with a post-Quota indicator variable, as further controls. These unit-specific control variables are the literacy rate in 1901, measured as the share of people who could read relative to the district's overall population; the urbanization rate in 1901, measured as the share of people living in towns with more than 5,000 inhabitants in the district; the altitude at which the district is located; an indicator variable that returns a value of one if the district has access to the railway network before 1901;[25] the number of deaths due to World War One.[26] To control for pull factors, in some specifications, we explicitly control for an interaction term between US GDP, which serves as an indicator of the business cycle, and US emigration. All results remain quantitatively unchanged.

In the above-mentioned tables, we also document the robustness of our results in meeting these specifications

[25]For province-level analyses, we use the share of municipalities in the province that had access to the railway network before 1901.

[26]Shares are expressed relative to the population in 1911 for province-level regression, as 1911 is the first decade of observation for analyses at that level.

when using the shift-share instrument as treatment. In addition, we test that our results are robust to the exclusion of specific parts of the sample. In Figures C.2 and C.4, we show that any specific region does not drive the baseline estimates, as regression coefficients remain stable irrespective of the region that we exclude from the sample.

In Figures C.1 and C.3, we compare the baseline estimated standard errors with a battery of alternative estimators. Among others, we employ the correction suggested by Conley (1999) to allow for time and spatial autocorrelation.[27] The significance of the baseline results remains largely unaltered.

To further assess the validity of the instrumented difference-in-differences design, following Rambachan and Roth (2023), in Appendix figure C.5, we estimate bounds on the relative magnitude of deviations from parallel trends. This exercise addresses the possibility that there may be unobserved shocks prior to the Quotas that affected with differential intensity the districts that would be more exposed by the emigration restriction shock. We report the confidence intervals for different values of the $\bar{M}$ threshold for population (Panel C.5a) and manufacturing employment (C.5b). In both cases, the "breakdown" value for a null effect is close to 2, implying that our findings that the Quotas had a significant effect on population and employment are valid inasmuch as we are willing to assume that the post-treatment violations of parallel trends is less than twice as large as the largest pre-treatment violation. We interpret this result as corroborating the robustness of our findings.

V.5 Discussion and Limitations of the Analysis

In this section, we discuss some alternative mechanisms that could be compatible with our findings, and we touch on how data limitations might preclude some additional and potentially relevant analysis. We then briefly elaborate on the external validity of our results.

Human capital spillovers ignited by out-migration have traditionally received sizable attention in the literature. Evidence by Spitzer and Zimran (2018) suggests that Italian emigrants to the United States were positively selected within Southern regions, implying that emigration was exerting a "brain drain" effect on Southern Italy. Under this interpretation, our estimated effects of the Quota Acts would be partially confounded by human-capital dynamics triggered by the IRP shock. More specifically, the drop in capital investment and technology adoption might be driven by substitutability between capital goods and the upper tail of the skill distribution of workers rather than by directed technical adoption. Even though this mechanism does not necessarily conflict with the one we propose, we view this as second-order in our setting for two reasons. First, we estimate employment gains and capital investment losses in the First Industrial Revolution, traditionally low-skilled and labor-intensive sectors. Hence, it is unlikely that high-skilled workers would be comparatively more productive

[27] Unfortunately, Conley standard errors are not implemented in any available package for PQML estimation. We thus estimate these regressions through OLS on the logged value of each outcome.

there. Second, we run a battery of robustness checks—see Online Appendix Tables C.5, C.6, C.7 and C.8. Results hold when we include the literacy rate as a proxy for average human capital in our regressions.

Besides the brain-drain effect, remittances are a traditionally major research topic within the emigration literature. Despite sizable global flows, Clemens (2011) argues that remittances can have, at best, a second or third-order effect on economic growth in sending countries when compared to the welfare effects of immigration restriction barriers. Building on this insight and given data limitations, we abstracted from including remittances in our analysis. Remittances nonetheless represent a competing mechanism. More exposed districts were receiving more remittances before the Quota Acts. Hence, they suffered the most from the border closure. Since within-household cash transfers result in aggregate savings, remittances may accrue to overall investment dynamics (Rapoport and Docquier, 2006). A large literature has nonetheless documented that remittances are largely spent on consumption and invested in human—rather than physical—capital (for a review, see Yang, 2011). A more sensible interpretation could be that remittances fostered literacy (e.g., Fernández, 2022). Exposed districts would have thus suffered from a relative drop in skilled workers following the Acts, and the labor force would have reshuffled toward unskilled sectors. This pattern would thus move in the opposite direction of the reverse-brain-drain effect. Under this interpretation, this channel does not conflict with the one we propose. If anything, it augments the relevance of exposure to the Quota Acts in generating an excess supply of workers, which triggered the directed technical incentive to abandon investment in physical capital.

One reason precluding a causal interpretation of our estimates would be that—even when conditioning on the decision to emigrate—the choice of *where* to emigrate was systematically correlated with factors inducing an underlying correlation with local economic development. We provide and discuss evidence against this interpretation throughout this paper. Historical scholarship, however, notes that assimilation patterns of Italian immigrants in the United States and Argentina during this period substantially differed (Klein, 1983).[28] If this was caused by pre-migration differences in characteristics, then our identification scheme may fail. Using detailed data from censuses and passenger lists, Pérez (2021) nonetheless documents that the "success" of Italians in Argentina compared to Italians in the United States was unlikely to be caused by pre-migration differences in observable characteristics between the two groups. Emigrants to Argentina and the United States were essentially indistinguishable in terms of occupation and literacy rate, the only difference being that the former chiefly originated from Northern regions. In contrast, the latter mostly came from Southern areas. Selection patterns across the two groups do not display sizable differences, providing solid evidence in favor of our identification assumption.

Data limitations prevent us from studying two additional, potentially interesting variables: wages and output

[28] Argentina and the United States were the two leading destinations for Italian emigrants in this period. Klein (1983), among others, noted that Italian immigrants in Argentina had higher home-ownership rates and were more likely to be employed in skilled occupations compared to Italians in the United States.

(productivity). Studying wages would be informative because directed technical adoption hinges on relatively more abundant labor becoming relatively cheaper. An analysis of wages could reveal this pattern, which we currently implicitly assume. Unfortunately, geographically disaggregated data on wages do not exist. Productivity would, in turn, be key to investigating the welfare effects of the Quota Acts. However, disaggregated data on output were not recorded until 1936.

It is not obvious that our results lend themselves to further generalization. Some similarities with contemporary settings nonetheless emerge. In terms of emigrant selection, the average Italian emigrant to the United States was slightly positively selected, left a rural area, and took on unskilled industrial jobs once in the United States (Sequeira *et al.*, 2020). This description is remarkably similar to contemporary emigration from poor countries, whereas it is starkly different from emigration from rich countries (e.g., Gibson *et al.*, 2011). While we do not claim that all our findings generalize to contemporary migration relationships, the evidence presented in this paper indicates that IRPs should be evaluated in terms of their joint effects on sending and receiving countries beyond remittances and human capital deprivation, as is standard in the existing literature.

VI CONCLUSIONS

The adoption of foreign technology is a crucial driver of economic growth for developing countries, which typically operate far from the technology frontier (Eaton and Kortum, 1999; Suri, 2011). This paper explores the relationship between technology adoption and out-migration, a common feature of the development process.

We study the Italian mass migration to the United States between 1892 and 1936 using individual-level data on Italian immigrants and newly digitized census data. Between 1921 and 1924, the U.S. promulgated two immigration restriction policies—the “Quota Acts”—which completely halted the inflow of Italian immigrants. Comparing districts with similar emigration rates but different destinations, we leverage identifying variation in exposure to the Quota Acts to estimate the impact of immigration restriction laws in a difference-in-differences framework. Moreover, we produce a novel sample of Ellis Island immigrants linked to the US full-count population census to construct a shift-share instrumental variable, which confirms the baseline results.

We document three facts. First, we find that the population increased in areas that were comparatively more exposed to the Quota Acts. This finding supports an “imperfect substitution” narrative whereby immigration restriction policies generate foregone emigration because those who would have migrated do not—or cannot—substitute the restricted location with alternative destination countries. We thus interpret immigration restriction policies as “passive” labor market policies that increase the supply of labor in the countries they target. Second, we show that firms in treated locations decreased capital investment and technology adoption, particularly in relatively more advanced technology vintages. How can we reconcile a positive labor supply shock with depressed investment in productivity-enhancing capital? We argue that the immigration restriction policy triggered directed technical change incentives (Zeira, 1998; Acemoglu, 2002). Firms face an incentive to substitute capital with more abundant, hence cheaper labor. To validate this hypothesis, we show that manufacturing employment increased in the districts more exposed to the Quotas. These dynamics operated in each manufacturing sector.

This paper presents novel evidence that out-migration can act as a driver of technology adoption, thus potentially empowering long-run economic growth. We emphasize that this channel operates plausibly independently on "brain drain" effects. It also suggests that immigration restriction policies enacted by immigration—typically developed—countries may hamper the modernization of emigration—typically developing—ones. Our findings thus inform policymakers about the potential long-term consequences of such policies.

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TABLES

TABLE 1. DESCRIPTIVE STATISTICS

	Mean (1)	Std. Dev. (2)	Median (3)	Min. (4)	Max. (5)	Units (6)	Observations (7)
Panel A. District-Level Variables from Population Censuses (in 10,000 units unless otherwise specified)							
Population	1.704	1.544	1.278	0.250	15.041	211	1,235
US Emigrants	0.077	0.068	0.055	0.006	0.250	211	1,235
Emigrants	2.036	1.478	1.767	0.126	10.182	211	1,235
Manufacturing Employment Share (%)	10.810	6.191	9.170	0.278	38.287	211	1,235
Agriculture Employment Share (%)	27.209	8.786	26.803	0.953	77.549	211	1,024
Altitude	3.402	2.143	3.302	0.013	9.669	211	1,235
Area	1.335	0.831	1.097	0.105	4.982	211	1,235
Share of Cities Above 20,000 (%)	5.057	11.287	1.538	0.000	100.000	211	1,235
Share of Population in Cities Above 20,000 (%)	20.771	23.277	16.499	0.000	100.000	211	1,235
Panel B. Province-Level Manufacturing Employment by Sector (in 10,000 units)							
Agriculture	1.491	1.546	1.103	0.150	11.856	69	343
Chemicals	0.166	0.466	0.046	0.000	5.046	69	343
Construction	1.296	1.176	0.944	0.162	8.077	69	343
Metalworking	0.868	1.651	0.401	0.065	16.876	69	343
Mining	0.145	0.259	0.068	0.000	2.137	69	343
Textiles	2.030	2.736	1.151	0.070	19.988	69	343
Panel C. Province-Level Capital Variables (in 1,000 units)							
N. of Firms	7.665	6.804	5.894	0.000	54.116	71	207
N. of Firms with Engine	1.417	1.880	0.887	0.000	17.404	71	207
N. of Electrical Engines	4.841	13.869	1.410	0.000	158.155	71	207
Electrical Horsepower	35.142	83.486	9.540	0.000	841.094	71	207
N. of Mechanical Engines	0.551	0.395	0.428	0.000	2.079	71	207
Mechanical Horsepower	14.629	16.654	7.875	0.000	84.585	71	207

Notes. This Table reports summary statistics for the main variables used in the paper. Data in Panel A are tabulated from population censuses; data in Panel B and C are digitized from manufacturing censuses. Variables in Panels A and B are reported in 10,000 units unless otherwise specified except population, which is expressed in 100,000 terms; variables in Panel C are reported in 1,000 units. All variables are cross-walked to consistent 1921 district (Panel A) and province (Panels B and C) borders. Referenced on page 9.

Table 2. Correlation between Treatment and Pre-period Observable Variables

	Observed Quota Exposure						Shift-Share Instrumental Variable					
	1901		1911		Growth Rate		1901		1911		Growth Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. District-Level Variables from Population Census												
Population	0.900***	(0.136)	1.040***	(0.164)	0.264**	(0.114)	0.424***	(0.093)	0.486***	(0.112)	-0.007	(0.076)
Manufacturing Employment Share	0.504***	(0.098)	1.056***	(0.227)	0.066	(0.088)	0.252***	(0.066)	0.495***	(0.153)	-0.048	(0.057)
Agriculture Employment Share	0.556***	(0.078)	0.788***	(0.113)	0.005	(0.075)	0.165***	(0.055)	0.255***	(0.079)	0.005	(0.049)
Share of Cities Above 20,000 (%)	-0.056	(0.117)	-0.064	(0.119)	0.161	(0.203)	-0.216***	(0.062)	-0.245***	(0.063)	-0.153	(0.106)
Share of Population in Cities Above 20,000 (%)	0.516***	(0.123)	0.507***	(0.126)	0.170	(0.203)	0.191**	(0.081)	0.167**	(0.083)	-0.151	(0.105)
Panel B. Province-Level Manufacturing Employment by Sector												
Agriculture	0.054	(0.310)	0.081	(0.469)	-0.260	(0.319)	0.134	(0.144)	0.110	(0.219)	-0.270*	(0.145)
Chemicals	-0.027	(0.264)	-0.122	(0.615)	0.625*	(0.333)	0.107	(0.123)	0.399	(0.282)	0.139	(0.160)
Construction	0.031	(0.263)	0.111	(0.446)	0.366	(0.343)	0.064	(0.123)	0.233	(0.206)	0.260	(0.158)
Metalworking	0.002	(0.298)	0.107	(0.594)	0.479	(0.302)	0.212	(0.136)	0.389	(0.273)	0.250*	(0.140)
Mining	0.609	(0.426)	0.720**	(0.342)	0.165	(0.206)	0.300	(0.199)	0.405***	(0.157)	0.014	(0.098)
Textiles	0.114	(0.423)	0.125	(0.412)	0.019	(0.233)	0.159	(0.197)	0.175	(0.192)	0.146	(0.107)
Panel C. Province-Level Capital Indicators												
N. of Firms			0.244	(0.482)					0.248	(0.231)		
N. of Firms with Engine			0.361	(0.503)					0.240	(0.242)		
N. of Electrical Engines			0.556	(0.607)					0.285	(0.293)		
Electrical Horsepower			0.212	(0.545)					0.344	(0.259)		
N. of Mechanical Engines			-0.046	(0.276)					0.055	(0.133)		
Mechanical Horsepower			-0.039	(0.429)					0.360*	(0.201)		

Notes. This Table displays the correlation between the observable district- and province-level variables, observed US emigration (columns 1–6), and the instrument (columns 7–12). In columns (1–2) and (7–8), the correlation is between the treatments and the variables in 1901; in columns (3–4) and (9–10), the variables are recorded in 1911. In columns (5–6) and (11-12), we report the correlation between the two treatments and the growth rate of each variable between 1901 and 1911. The absence of any statistically significant correlation between the growth rates and the treatment variables is the crucial test in support of the validity of the research design. In Panel A, the units of observation are districts; in Panel B and C, the units of observation are provinces. Capital variables, displayed in Panel C, are observed in 1911, 1927, and 1937, so we cannot compute the growth rate between two consecutive pre-treatment periods. The outcome variable in each regression is standardized to facilitate the comparisons. Each regression controls for the number of emigrants and region-fixed effects. Units are weighed by their population in 1881. Standard errors are clustered at the district level and are displayed in parentheses. Referenced on pages 15, 16, 19.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

TABLE 3. THE RESPONSE OF POPULATION

	Dependent Variable: Population					
	Difference-in-Differences		Instrumented DiD			
	(1)	(2)	(3)	(4)	(5)	(6)
US Emigrants × Post	0.103**	0.067***				
	(0.048)	(0.026)				
US $\widehat{\text{Emigrants}}$ × Post			0.047**	0.035***		0.016**
			(0.019)	(0.013)		(0.008)
I(US $\widehat{\text{Emigrants}}$) × Post					0.115**	
					(0.048)	
Emigrants × Post	-0.088***	-0.064**	-0.047**	-0.026	-0.037*	-0.109***
	(0.034)	(0.028)	(0.022)	(0.022)	(0.021)	(0.021)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	–	Yes	–	Yes	–
Region-Decade FE	No	Yes	No	Yes	No	Yes
Regions in Sample	All	All	All	All	All	South
N. of Districts	202	201	192	191	192	85
N. of Observations	1,198	1,192	1,140	1,134	1,140	502
R^2	0.372	0.377	0.369	0.374	0.369	0.330
Mean Dep. Var.	1.734	1.723	1.788	1.776	1.788	1.599
Std. Beta Coef.	0.193	0.125	0.019	0.014	0.022	0.008

Notes. This Table reports the estimated effect of district-level exposure to the US Quota Acts on population. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. In columns (1–2), the treatment is an interaction term between US emigration and a post-1921 term. In columns (3–6), the treatment is an interaction between the instrument for US emigration (columns 3–4, 6) or an analogous above-median binary indicator (column 5) and a post-1921 term. In column (6), the sample includes only regions in the South (former Kingdom of Two Sicilies). We include regions-by-time fixed effects in columns (2), (4), and (6). Each regression includes district and time-fixed effects and controls for the emigration rate and an indicator variable for Southern regions; both interacted with a post-1921 indicator. The last row reports the coefficient of the interaction between the number of emigrants and a post-1921 indicator. Units are weighed by their population in 1881. Standard errors clustered at the district level are reported in parentheses. Referenced on page 17.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

TABLE 4. THE RESPONSE OF CAPITAL AND TECHNOLOGY ADOPTION

	Dependent Variable: Province-Level Number of... (in 1,000 units)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Firms	Firms with Engines	Mechanical Engines	Electrical Engines	Mechanical Horsepower	Electrical Horsepower
Panel A. Difference-in-Differences Estimates						
US Emigrants $\times$ Post	-0.186**	-0.070	-0.220***	-0.339	-0.243**	-0.155
	(0.073)	(0.166)	(0.050)	(0.383)	(0.099)	(0.299)
Emigrants $\times$ Post	0.103	-0.126	0.043	0.435	0.042	0.007
	(0.118)	(0.189)	(0.063)	(0.488)	(0.102)	(0.364)
Panel B. Instrumented Difference-in-Differences Estimates						
US $\widehat{\text{Emigrants}}$ $\times$ Post	-0.088*	-0.147***	-0.050	-0.319***	-0.151**	-0.262***
	(0.051)	(0.042)	(0.035)	(0.115)	(0.061)	(0.082)
Emigrants $\times$ Post	0.035	-0.013	-0.087	0.552	0.012	0.217
	(0.112)	(0.116)	(0.070)	(0.344)	(0.101)	(0.259)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
N. of Provinces	68	68	68	68	68	68
N. of Observations	204	204	204	204	204	204
R^2	0.435	0.284	0.118	0.672	0.685	0.854

Notes. This Table reports the estimated effect of exposure to the US Quota Acts on capital investment. The unit of observation is a province observed at a census-decade frequency between 1911 and 1936. The dependent variables are the number of firms (column 1), the number of firms with at least one engine (column 2), the number of mechanical engines (column 3), the number of electrical engines (column 4), the horsepower generated by mechanical (column 5) and electrical (column 6) engines. In Panel A, the treatment is an interaction between a post-Quota (1921) indicator variable and the number of US emigrants. In Panel B, the treatment substitutes measured US emigration with the shift-share instrument. Each regression controls for the number of emigrants and an indicator variable for Southern regions, both interacted with a post-1921 indicator. Regressions include province and decade-fixed effects. Units are weighed by their population in 1881. Standard errors clustered at the province level are reported in parentheses. Referenced on page 19.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

TABLE 5. THE RESPONSE OF MANUFACTURING EMPLOYMENT

	Dependent Variable: Manufacturing Employment					
	Difference-in-Differences		Instrumented DiD			
	(1)	(2)	(3)	(4)	(5)	(6)
US Emigrants × Post	0.128**	0.119**				
	(0.063)	(0.053)				
US $\widehat{\text{Emigrants}}$ × Post			0.084**	0.044		0.066***
			(0.036)	(0.035)		(0.018)
I(US $\widehat{\text{Emigrants}}$) × Post					0.230***	
					(0.073)	
Emigrants × Post	-0.058	-0.077	-0.027	0.008	-0.021	-0.283***
	(0.082)	(0.066)	(0.062)	(0.047)	(0.050)	(0.059)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	–	Yes	–	Yes	–
Region-Decade FE	No	Yes	No	Yes	No	Yes
Regions in Sample	All	All	All	All	All	South
N. of Districts	202	201	192	191	192	85
N. of Observations	1,198	1,192	1,140	1,134	1,140	502
R^2	0.597	0.614	0.597	0.613	0.597	0.519
Mean Dep. Var.	2.114	2.098	2.185	2.169	2.185	1.526
Std. Beta Coef.	0.110	0.103	0.015	0.008	0.020	0.019

Notes. This Table reports the estimated effect of district-level exposure to the US Quota Acts on manufacturing employment. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. In columns (1–2), the treatment is an interaction term between US emigration and a post-1921 term. In columns (3–6), the treatment is an interaction between the instrument for US emigration (columns 3–4, 6) or an analogous above-median binary indicator (column 5) and a post-1921 term. In column (6), the sample includes only regions in the South (former Kingdom of Two Sicilies). We include regions-by-time fixed effects in columns (2), (4), and (6). Each regression includes district and time-fixed effects and controls for the emigration rate and an indicator variable for Southern regions; both interacted with a post-1921 indicator. The last row reports the coefficient of the interaction between the number of emigrants and a post-1921 indicator. Units are weighed by their population in 1881. Standard errors clustered at the district level are reported in parentheses. Referenced on page 20.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

TABLE 6. THE RESPONSE OF AGRICULTURE EMPLOYMENT

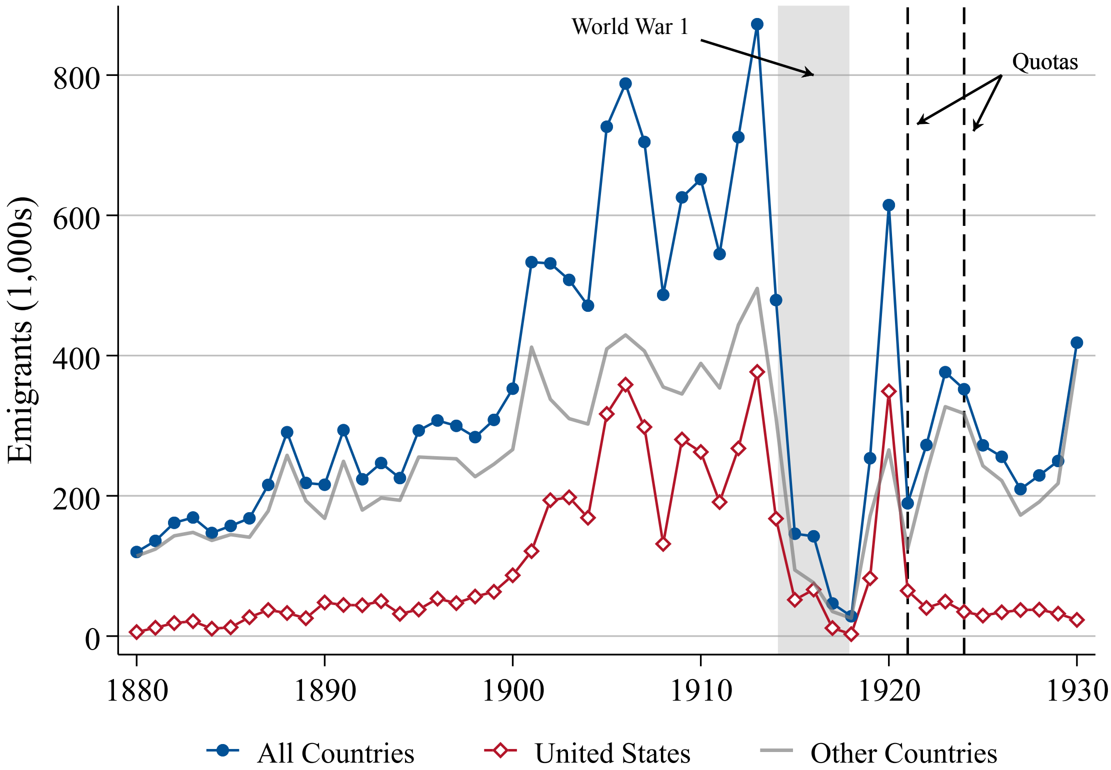
	Dependent Variable: Agriculture Employment					
	Difference-in-Differences		Instrumented DiD			
	(1)	(2)	(3)	(4)	(5)	(6)
US Emigrants × Post	-0.009	-0.011				
	(0.036)	(0.024)				
US $\widehat{\text{Emigrants}}$ × Post			-0.016	-0.032		-0.061*
			(0.021)	(0.023)		(0.034)
I(US $\widehat{\text{Emigrants}}$) × Post					-0.036	
					(0.047)	
Emigrants × Post	-0.110***	-0.120***	-0.108***	-0.117***	-0.112***	-0.165***
	(0.036)	(0.041)	(0.028)	(0.030)	(0.030)	(0.055)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	–	Yes	–	Yes	–
Region-Decade FE	No	Yes	No	Yes	No	Yes
Regions in Sample	All	All	All	All	All	South
N. of Districts	202	201	192	191	192	85
N. of Observations	996	991	948	943	948	417
R^2	0.312	0.315	0.304	0.307	0.304	0.179
Mean Dep. Var.	4.089	4.043	4.215	4.168	4.215	3.766
Std. Beta Coef.	-0.012	-0.015	-0.004	-0.009	-0.005	-0.028

Notes. This Table reports the estimated effect of district-level exposure to the US Quota Acts on agriculture employment. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. In columns (1–2), the treatment is an interaction term between US emigration and a post-1921 term. In columns (3–6), the treatment is an interaction between the instrument for US emigration (columns 3–4, 6) or an analogous above-median binary indicator (column 5) and a post-1921 term. In column (6), the sample includes only regions in the South (former Kingdom of Two Sicilies). We include regions-by-time fixed effects in columns (2), (4), and (6). Each regression includes district and time-fixed effects and controls for the number of emigrants and an indicator variable for Southern regions; both interacted with a post-1921 indicator. The last row reports the coefficient of the interaction between the number of emigrants and a post-1921 indicator. Units are weighed by their population in 1881. Standard errors clustered at the district level are reported in parentheses. Referenced on page 20.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Figures

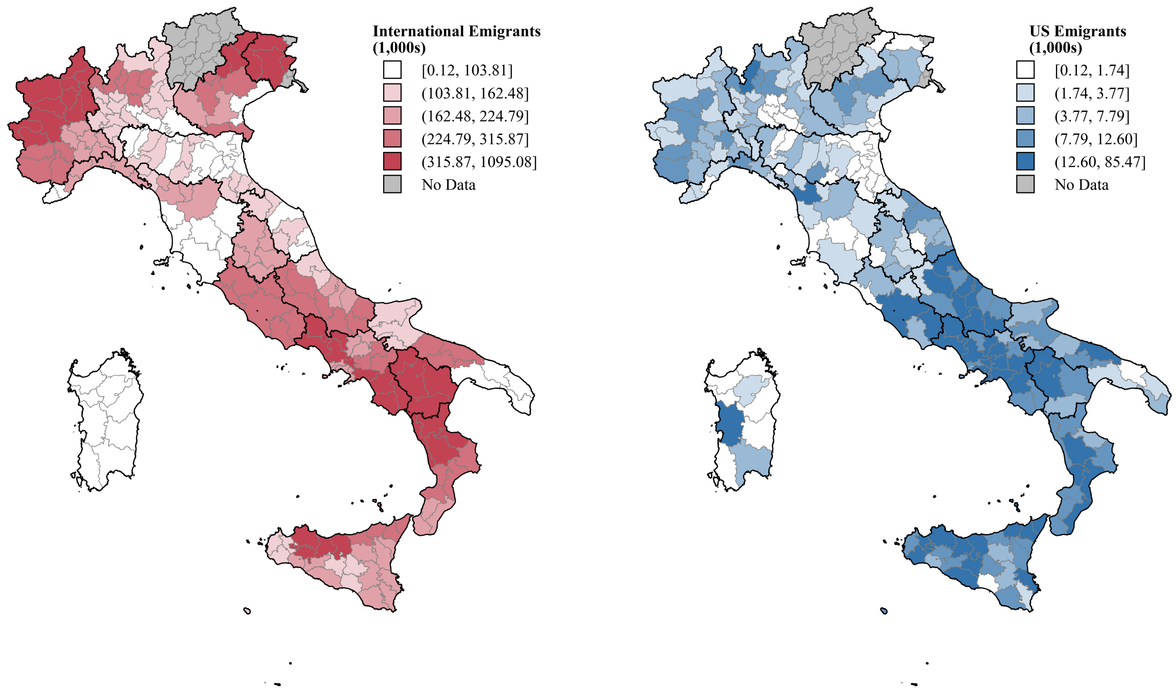
FIGURE 1. INTERNATIONAL MIGRANTS, 1880–1930



Notes. This figure reports the yearly outflow of international migrants from Italy between 1880 and 1930. Data are digitized from the *Annuario statistico dell'emigrazione italiana dal 1876 al 1925: con notizie sull'emigrazione negli anni 1869-1875* and from the *Statistica delle migrazioni da e per l'estero: anni 1928, 1929 e 1930 con confronti dal 1921 al 1927*, both edited by the Italian Statistical Office. The blue line reports the overall number of international migrants; the red line reports the number of migrants to the United States, the single most important destination country over this period; the gray line reports emigrants to every other country. The shaded gray area marks the 1914–1918 war years; the dashed vertical black lines mark the 1921 and 1924 Emergency and (Johnson-Reed) Immigration Quota Acts, respectively. Referenced on pages 1, 3, 11, 18.

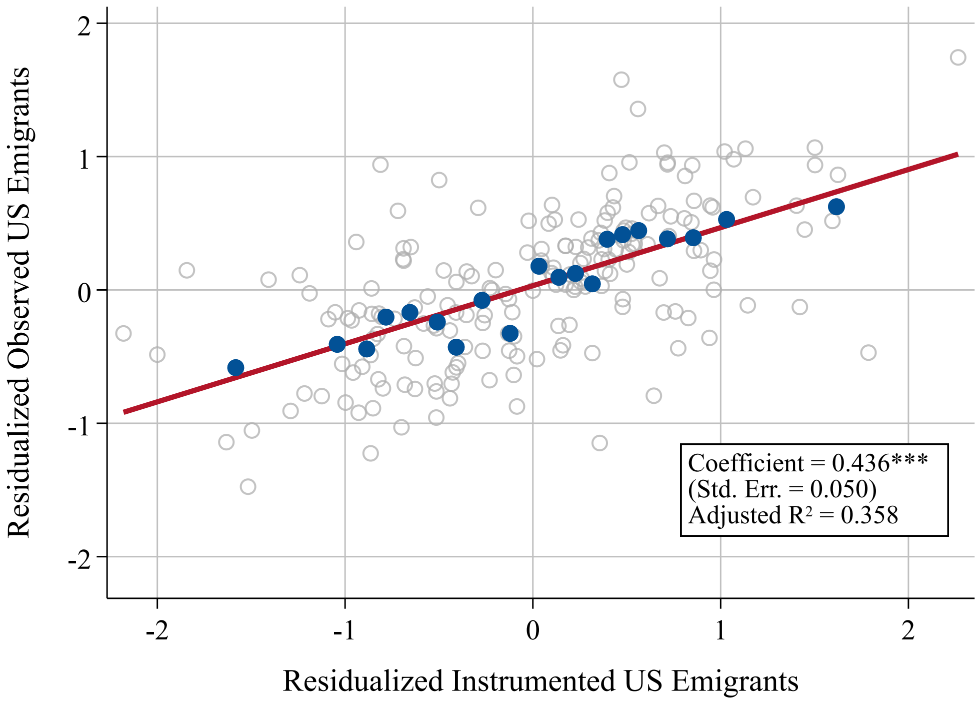
FIGURE 2. SPATIAL DISTRIBUTION OF INTERNATIONAL EMIGRATION, 1890–1921

(A) NUMBER OF INTERNATIONAL EMIGRANTS (B) NUMBER OF US EMIGRANTS



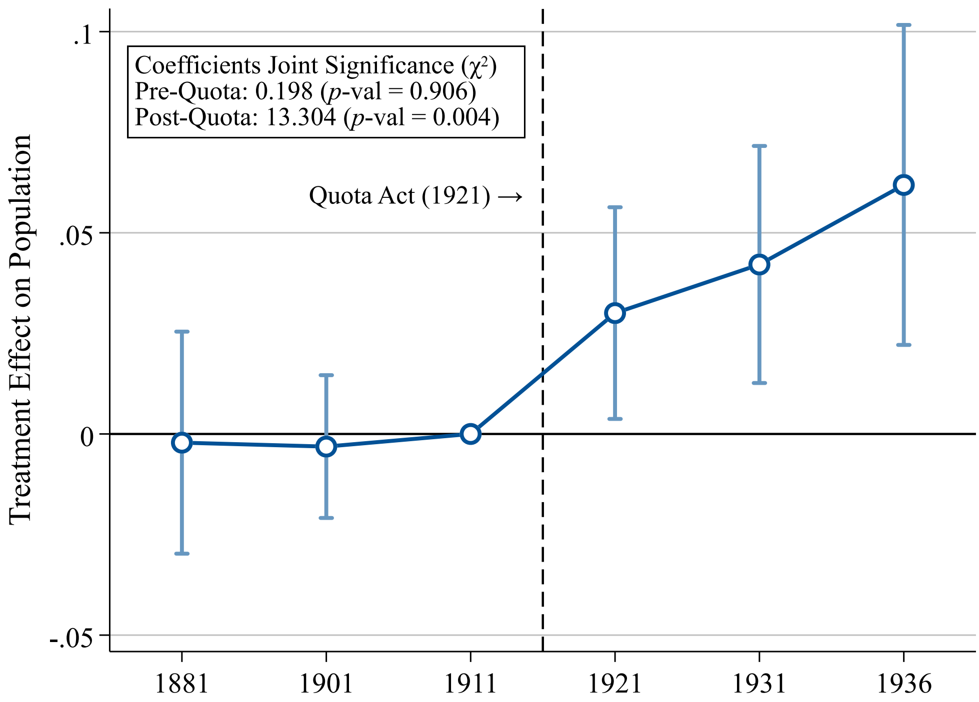
Notes. These figures report the spatial distribution of overall international emigrants (Figure 2a) and emigrants who settle in the United States (Figure 2b) across districts. In each map, the unit of observation is a district. Black lines superimpose region boundaries. The geography is at 1921 borders. The grey areas (today Trentino, Südtirol, Venezia-Giulia and Gorizia) were acquired after the Treaty of Versailles (1918) and are thus excluded from the sample. Panel 2a reports the number of emigrants who leave the Italian territory. The data is at the province level, hence the clustered rendering. Panel 2b displays the number of emigrants who settle in the US and are registered at the Ellis Island immigration station. The data are at the district level and constructed as detailed in the main text. The figures refer to 1890–1921; emigrants are expressed in thousand units. Referenced on page 11.

FIGURE 3. CORRELATION BETWEEN MEASURED AND PREDICTED US EMIGRATION



Notes. This Figure reports the "first stage" correlation between the observed Quota Exposure and the shift-share instrument. On the *y*- and *x*-axes, we report the residuals of observed US emigration and of the instrument. The residuals are obtained by regressing each variable against province fixed effects, which implicitly also control for an indicator for Southern regions and the number of emigrants, and computing the regression residuals. The unit of observation is a district. Each grey dot refers to one district; the blue dots report the binned scatterplot of the residuals. The red line corresponds to the ordinary least-squares fit. The Figure reports the regression coefficient, its standard error clustered at the district level, and the adjusted coefficient of determination. Referenced on page 16.

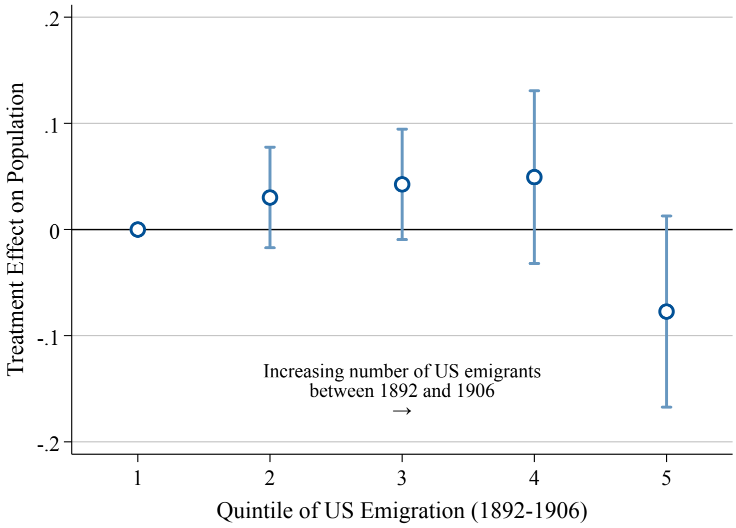
FIGURE 4. EFFECT OF EXPOSURE TO THE QUOTA ACTS ON POPULATION



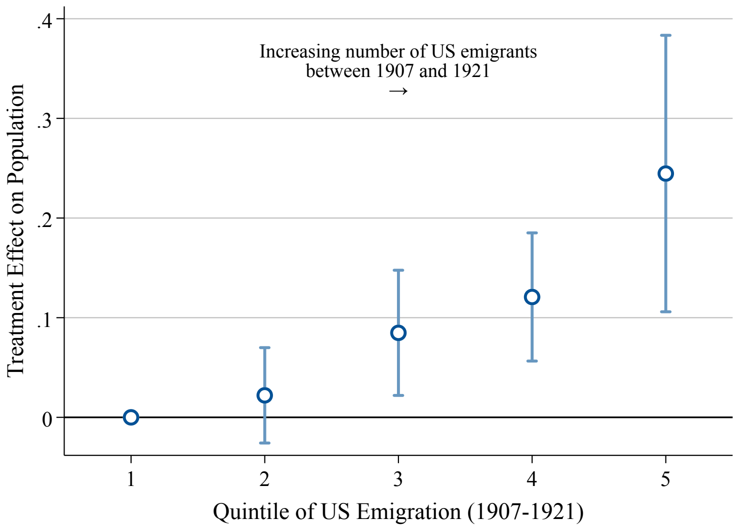
Notes. This figure reports the estimated effect of district-level exposure to the US Quota Acts on population. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. The outcome variable is population. Each dot reports the coefficient of an interaction term between period dummies and the cross-sectional instrument of US emigration. The regression includes district and decade-fixed effects and controls for the volume of international migrants and an indicator variable for Southern regions, both interacted with a post-1921 indicator. Units are weighed by their population in 1881. The bands report 95% confidence intervals from standard errors clustered at the district level. The graph reports the values of the Wald statistics of joint significance of pre-treatment and post-treatment coefficients and their respective *p*-values. Referenced on pages 15, 18, 21.

FIGURE 5. TIMING OF US EMIGRATION AND POPULATION RESPONSE TO THE QUOTA ACTS

(A) FIRST HALF OF THE SAMPLE PERIOD: 1892–1906

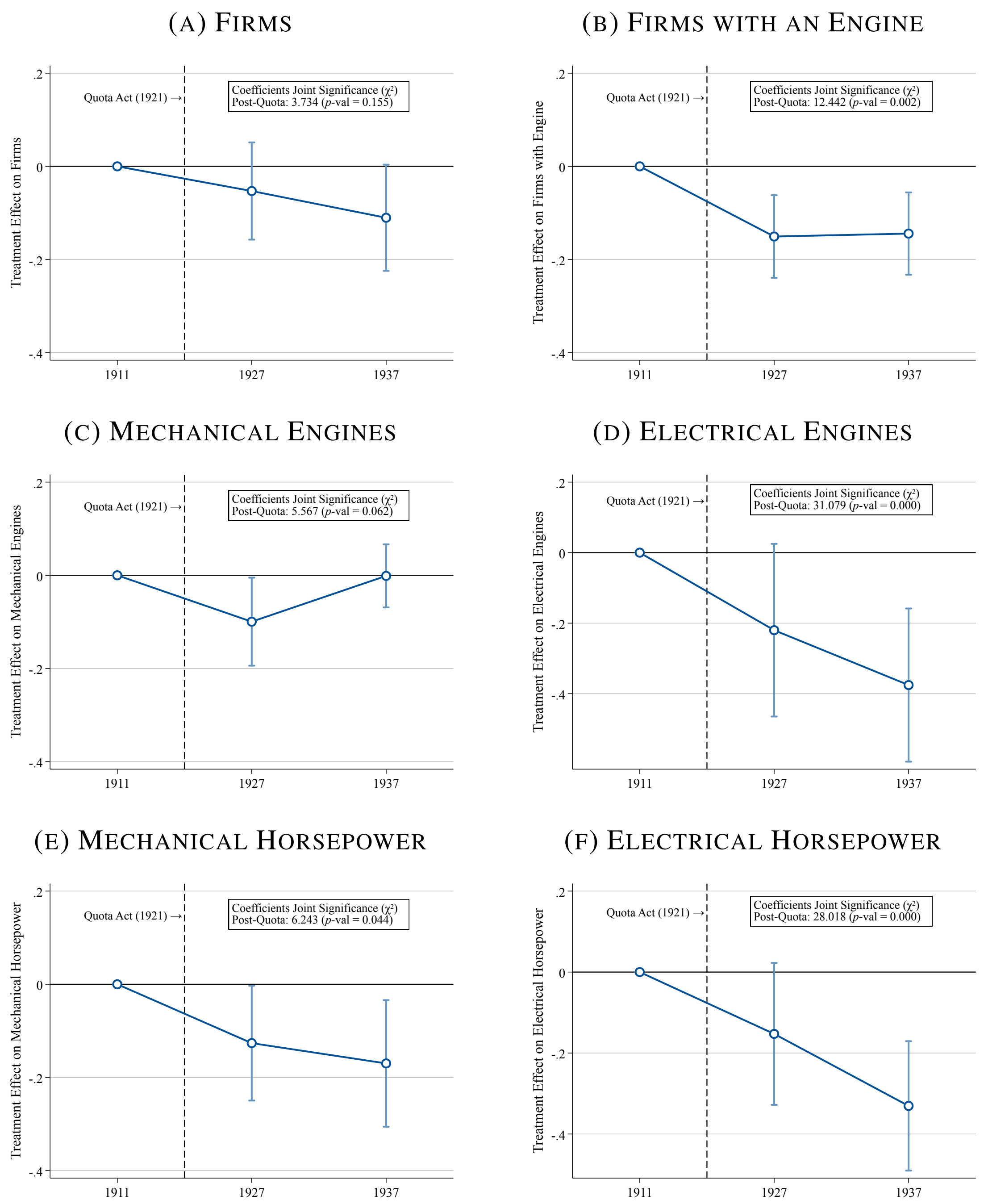


(B) SECOND HALF OF THE SAMPLE PERIOD: 1907–1921



Notes. This figure reports the estimated effect of district-level exposure to the US Quota Acts on population depending on the timing of US emigration. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. The outcome variable is population. Each dot reports the coefficient of an interaction term between the baseline Quota Exposure treatment, a post-quota indicator, and a set of indicator variables that code the quantile of US emigration over different time spans. In panel 5a, the quantiles refer to the number of US emigrants in the first half of the analysis sample, i.e., between 1892 and 1906; in panel 5b, the quantiles are computed on the distribution of US emigrants in the second half of the sample. The regression controls for the baseline treatment effect of Quota exposure. Standard errors are robust; bands report 95% confidence intervals. Referenced on page 18.

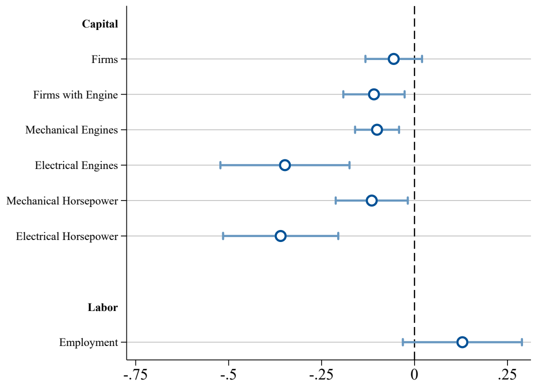
FIGURE 6. EFFECT OF EXPOSURE TO THE QUOTA ACTS ON CAPITAL INVESTMENT



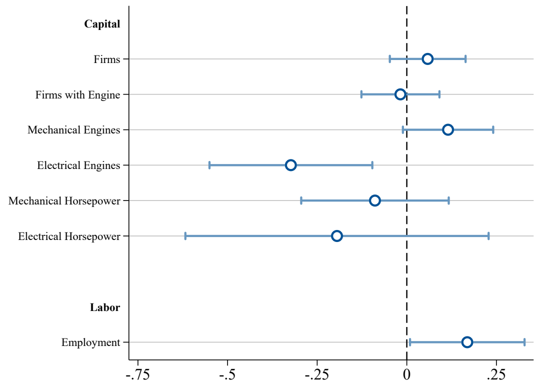
Notes. These figures report the estimated effect of exposure to the US Quota Acts on capital investment over time. The unit of observation is a province observed at a census-decade frequency between 1911 and 1936. The outcome variables are firms (panel 6a), firms with an engine (panel 6b), mechanical (panel 6c) and electrical (panel 6d) engines, mechanical (panel 6e) and electrical (panel 6f) horsepower. Each dot reports the coefficient of an interaction term between period dummies and the cross-sectional instrument of US emigration. Each regression includes province and decade-fixed effects and controls for the emigration rate and an indicator variable for Southern regions, both interacted with a post-1921 indicator. Units are weighed by their population in 1881. Bands report 95% confidence intervals from standard errors clustered at the province level. The graphs report the values of the Wald statistics of joint significance of pre-treatment and post-treatment coefficients and their respective p-values. Referenced on page 19.

FIGURE 7. EFFECT OF THE QUOTA ACTS ON CAPITAL AND EMPLOYMENT BY SECTOR

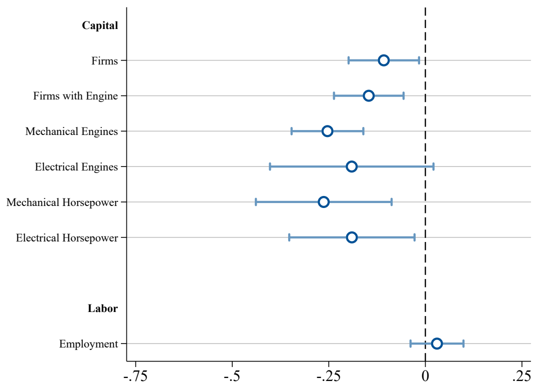
(A) AGRICULTURE



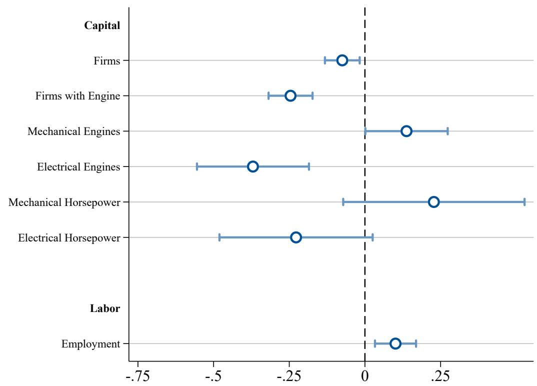
(B) CHEMICALS



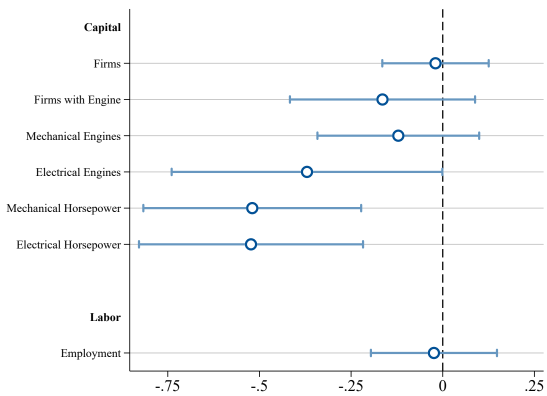
(C) CONSTRUCTION



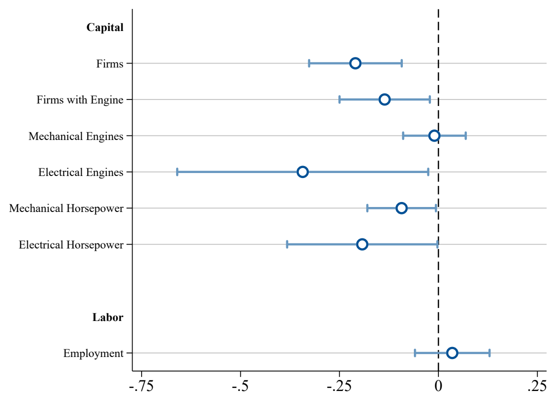
(D) METALWORKING



(E) MINING

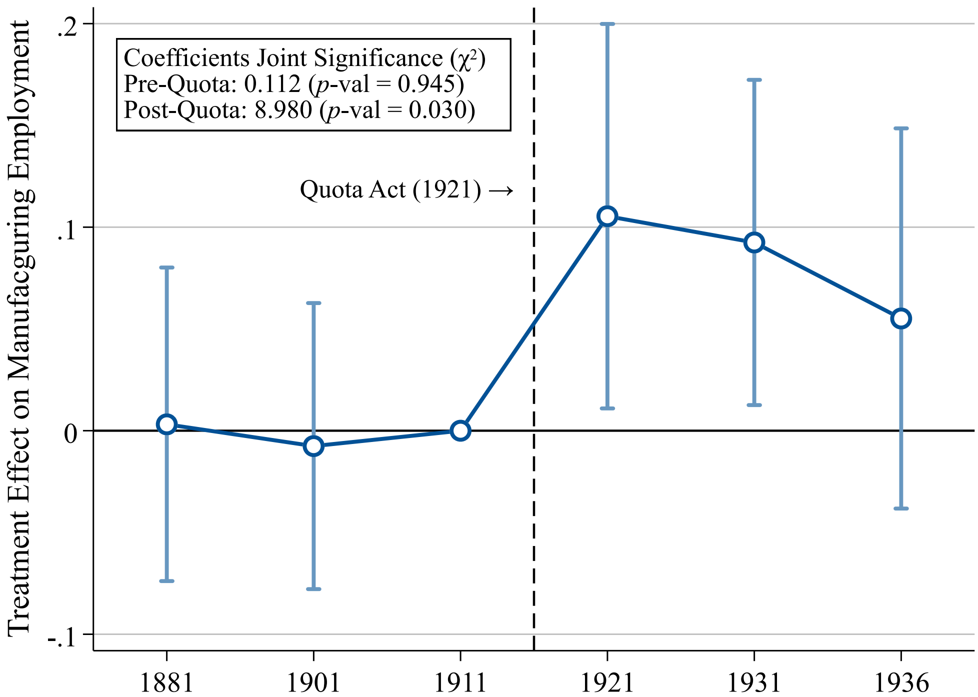


(F) TEXTILES



Notes. These figures report the estimated effect of exposure to the US Quota Acts on capital and employment by sector over time. The unit of observation is a district observed at a census-decade frequency between 1901 and 1936. Each panel reports the results for a specific manufacturing sector. The outcome variables are listed by row. Each dot reports the coefficient of an interaction between a post-1921 indicator and the instrument of US emigration. Each regression includes district and decade-fixed effects and controls for the volume of emigration and an indicator variable for Southern regions, both interacted with a post-1921 indicator. Units are weighed by their population in 1881. Bands report 95% confidence intervals from standard errors clustered at the province level. Referenced on page 20.

FIGURE 8. EFFECT OF EXPOSURE TO THE QUOTA ACTS ON MANUFACTURING EMPLOYMENT



Notes. This Figure reports the effect of exposure to the Quota Acts on manufacturing employment. The unit of observation is a district observed at a census-decade frequency between 1881 and 1936. The outcome variable is employment in manufacturing. Each dot reports the coefficient of an interaction term between period dummies and the cross-sectional instrument of US emigration. Each regression includes district and decade-fixed effects and controls for the volume of emigration and an indicator variable for Southern regions, both interacted with a post-1921 indicator. Units are weighed by their population in 1881. The bands report 95% confidence intervals from standard errors clustered at the district level. The graph reports the values of the Wald statistics of joint significance of pre-treatment and post-treatment coefficients and their respective p-values. Referenced on pages 15, 21.