The Economic Effects of Immigration Restriction Policies

Evidence from the Italian Mass Migration to the US*

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Abstract

This article studies the impact of immigration restriction policies on technology adoption in countries sending migrants. Between 1920 and 1921, the number of Italian immigrants 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 immigrants 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 employment in manufacturing. We provide evidence that "missing migrants," whose migration was inhibited by the Act, drive this result.

Keywords: Age of Mass Migration, Emigration, Economic Development, Immigration Barriers, Technology Adoption.

JEL Classification: N14, N34, O15, O33.

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1 Introduction

In recent years, attitudes towards immigration in developed countries have considerably deteriorated (*e.g.*, Guriev & Papaioannou, 2022). Immigration restriction policies (henceforth, IRPs) are becoming increasingly common, reinforcing an upward trend that has been documented since the 1970s.[1] A large literature in economics studies the potential effects of these pieces of legislation in countries receiving migrants. Evidence on emigration countries remains, however, comparatively scant.[2]

In this paper, we focus on one crucial dimension of economic growth: technology adoption. Since developing and emigration countries typically operate far from the technology frontier, the adoption of new technologies represents a major source of productivity gains.[3] This notwithstanding, the effects of out-migration – and policies attempting to restrict it – on technology adoption are *ex-ante* ambiguous and potentially conflicting. On the one hand, emigration entails a loss of human capital – the so-called brain drain – that may hamper the ability of countries to adopt new technologies (Kwok & Leland, 1982; Gibson & Mckenzie, 2011; Docquier & Rapoport , 2012).[4] On the other, however, emigration may incentivize the adoption of labor-saving technologies because it increases the relative cost of labor (among others, see Hicks, 1932; Habakkuk, 1962; Acemoglu, 2002). The former interpretation implies that IRPs would bolster technology adoption and prove beneficial for long-run growth. The latter theory, however, predicts that IRP-induced labor supply shocks would dampen the incentive to adopt labor-saving technologies, thus hampering economic development. In this paper, we offer a causal quantification of the effects of restrictive immigration policies on technology adoption in emigration countries.

We investigate this question in the context of the Age of Mass Migration, the largest episode of voluntary migrations in recorded history (Choate, 2008). Specifically, we focus on Italy, the archetypal sending country during this period. From 1876 to 1925, approximately 17 million emigrants left Italy (nearly 70% of the average Italian population in 1900); about half of them never returned. Italy had one of the highest emigration rates and, since the 1890s, it was the leader in sheer emigration numbers

[1]Data from (de Haas *et al.*, 2015) show that immigration restriction policies make up for approximately 40% of the entire corpus of migration laws. This share has been steadily increasing since the beginning of the 1970s.

[2]Clemens (2011) notes that in the RePEc archive, papers on emigration account for 25% of the overall migration literature.

[3]Several papers highlight the centrality of technology adoption for economic growth, especially in countries farther from the technology frontier, both theoretically (*e.g.* Parente & Prescott, 1994; Foster & Rosenzweig, 1995; Eaton & Kortoum, 1999) as well as empirically (*e.g.* Suri, 2011; Bryan *et al.*, 2014; Juhász *et al.*, 2022). Historically, Gerschenkron (1962) famously discusses the technological catch-up of countries at the periphery of the industrial world in the XIX century.

[4]Emigration has been shown to influence, among others, human-capital accumulation through remittances (Fernández-Sánchez, 2020), return migration (Dustmann *et al.*, 2011), and increased returns to schooling (Beine *et al.*, 2008). In this paper, however, we focus on technology adoption as one major determinant of long-run growth.

(Hatton & Williamson, 1998). On average, 40% emigrants headed toward the United States, and the remaining 60% were split between South America and Europe. The United States was therefore the single most absorbent emigration destination. Italian mass migration to the United States, however, abruptly ended in 1921, when Congress passed the first of a series of restrictive IRPs that we refer to collectively as the "Quota Acts." The Quota Acts defined numerical quotas for European countries that were based on how many citizens from each country were recorded living in the United States at a given point in time.[5]

We leverage the differential exposure to this shock across Italian districts to estimate the economic effects of emigration on industrialization and technology adoption. Comparable empirical exercises face three major limitations. First, emigration seldom flows into only a few destinations; hence, it is difficult to observe large restrictive policy shifts. 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 & Boustan, 2017). Third, it is often difficult to retrieve information on emigrants in their home country (Dustmann *et al.*, 2015).[6] Our unique historical setting allows overcoming these difficulties.

Our empirical strategy relies on different exposure to the Quota Acts across Italian districts. Consider, for the sake of argument, two districts A and B, both of which had high emigration rates. However, most migrants from district A went to the United States, whereas none from district B did. Our key observation is that district A will be highly exposed to the Quota Acts, whereas district B will not. This is because emigration flows displayed substantial time and spatial persistence. Local information diffusion and social networks shaped the dynamics of Italian mass migration more than home-destination wage gaps (Gould, 1980b).[7] Formally, our identification assumption thus requires that districts with similar emigration rates but different destinations would not have undergone different development trajectories had the Quota Acts not been enacted, *i.e.* they were on parallel trends in terms of the

[5]The 1921 Emergency Quota Act restricted the annual number of immigrants admitted into the United States 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, 1955).

[6]Aydemir & Borjas (2007) and Mishra (2007) overcome this issue by studying Mexican emigration to Canada and the United States, exploiting that about 95% of Mexican emigrants go to the United States. Meanwhile, Dustmann *et al.* (2015) study this in the context of Poland. These studies all lack exogenous variation to credibly identify the causal impact of migration policy on economic development in sending countries.

[7]Recently, Spitzer & Zimran (2020) formally validated the original information-diffusion hypothesis formulated by Gould (1980b). Further, Brum (2019) argues that the location choice of pioneers was a key determinant of future emigration outflows across districts. These findings confirm the original result from Hatton & Williamson (1998), who noted that pull factors, rather than push factors, explain the bulk of variation in Italian emigration.

outcomes we consider. We provide several pieces of evidence supporting this assumption. In Figure II, we plot emigrants as a fraction of the total population, showing that Northern, as well as Southern regions, experienced varying emigration intensities. By contrast, the share of emigrants heading to the United States is prevailing in the *Mezzogiorno* (South of Italy). The figure also shows that exposure to the Quota Acts reflects these heterogeneous patterns once we control for the extensive margin of emigration.[8] It is straightforward to conceive this context in terms of a simple difference-in-differences (DiD) framework with a continuous treatment defined by some measure for quota exposure at the district level, where we control for the share of emigrants relative to the total population.

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. We thus construct a novel dataset linking administrative records of Italian emigrants who arrived at Ellis Island between 1892 and 1930 to their district of origin, and we complement it with newly digitized detailed data from industrial and population censuses. These data allow us to document three sets of results.[9]

We first show that industrial firms located in districts more exposed to the Quota Acts substantially decreased investment in capital goods. We measure investment in capital-intensive production technologies with the number of installed engines, and we further distinguish between traditional mechanical engines and cutting-edge electrical ones. The electrical engine—a defining technology of the Second Industrial Revolution—could yield sizable productivity gains (David, 1990; Mokyr, 1998). We show that in more-exposed districts, the adoption of engines slowed. This effect is particularly strong in magnitude for electrical engines, either measured in absolute number or weighted by the horsepower they generated. This is relevant for our argument because electrical engines were a decisively labor-saving technology (Gaggl *et al.*, 2021). We also show that the worker-per-engine ratio, a proxy for the labor intensity of production technologies, increased in firms located in more-exposed districts. This result is consistent with findings by Andersson *et al.* (2022), who show that emigration fosters the adoption of labor-saving technologies because it dampens labor supply, hence increasing the relative cost of labor. Since technology adoption is a key driver of long-run growth (e.g., Juhász *et al.*, 2022), our evidence suggest that the Quotas had possibly detrimental effects on Italian economic development.

To rationalize these findings, we advance and validate the hypothesis that IRPs induce a geograph-

[8] In Figures C.1, C.2, and C.3, we show that more-exposed districts were not on different development trajectories before the Quota Acts, conditional on total emigration. This is key for valid causal inference of our estimates, as we explain later.

[9] In Section D.1, we develop a simple theoretical framework to explain our results in the context of labor-saving directed technical adoption, in the spirit of Zeira (1998) and San (2021).

ically segmented labor supply shock.[10] This is because, following an IRP, all those who would have migrated had the policy not been enacted are—at least partly—forced to join the local employment pool. More abundant (thus cheaper) labor dampens the incentive for firms to adopt capital-intensive technologies, as we observe. Under this interpretation, in Italy, the Quota Acts effectively implied that more-exposed districts experienced a disproportionate increase in labor supply, relative to less-exposed ones. Districts that experienced more emigration until 1924 were more exposed to the quotas because pull factors were disproportionately more effective there.[11] We document that population in these districts grew comparatively more relative to districts that were less exposed to the Quota Acts. We provide supportive evidence of this mechanism, showing that (i) emigrants did not substitute the United States with other arrival destinations—neither internal nor international—and (ii) emigration outflows toward unrestricted countries, i.e., countries that did not promulgate IRPs, did not increase. Hence, districts that had been supplying relatively more U.S.-bound emigrants ended up having more "missing" migrants, i.e., people who would have migrated had the Quota Acts not been enacted. This mechanism generates a spatially segmented positive labor supply shock. If our directed technical adoption interpretation is correct, we would expect to observe increased industrial employment in more-treated districts.

To further assess the soundness of the directed technical adoption hypothesis and validate it against alternative mechanisms, we study how employment across sectors reacted to the IRP-induced labor supply shock. We focus primarily on the two biggest sectors at the time, agriculture and manufacturing.[12] We find that employment in manufacturing grew considerably in districts that were comparatively more exposed to the Quota shock. This finding is consistent with directed technical adoption: firms in manufacturing substituted capital goods with more abundant, therefore cheaper, labor provided by missing migrants. By contrast, in agriculture, we find no sizable increase in employment. A possible explanation for this finding is that agriculture in this period was a largely labor-intensive activity, hence the incentive for manufacturing firms to enlarge their labor stock following the Quota shock was larger than for agriculture firms. Because industrial employment grew and agricultural employment did not, the share of workers engaged in manufacturing increased.

Identification, and therefore a causal interpretation of our estimates, may fail if conditional varia-

[10] This approach mirrors that of Abramitzky *et al.* (2019a), who document that the Quota Acts induced a negative labor supply shock in *U.S.* counties whose intensity depended on the prevailing origin of immigrants across European countries. In a similar spirit, Beerli *et al.* (2021) show that a reform that granted free access to the Swiss labor market to European workers increased natives' wages and benefitted Swiss firms.

[11] Several studies have documented that emigration location choices tend to persist over time (e.g. Gould, 1980b; Brum, 2019; Fontana *et al.*, 2020; Spitzer & Zimran, 2020).

[12] We repeat the entire analysis at the manufacture-sector level. We find that sectors where technology adoption drops the most, are also the ones where employment increases the most.

tion in U.S. emigration rates was still systematically correlated with economic performance. Historical evidence provided by Spitzer & Zimran (2020) suggests that this is unlikely. Information diffusion and local social networks were the decisive factors influencing emigrants' location decisions. While we cannot test the baseline identification assumption, we develop two instrumental variables (IVs) to deal with residual endogeneity concerns. In the first validation exercise, we develop an IV along the lines of Tabellini (2020). This allows us to fix the cross-sectional variation in emigrant origin to a given—early—point in time, and to predict a district's emigration using the time-series variation in aggregate outflows, dropping emigrants from that district. Our second IV exploits variation stemming from the timing of when districts became connected to the railway system, in the spirit of Sequeira *et al.* (2020). Because railways drastically reduced transportation costs, they fostered out-migration. Moreover, U.S. emigration boomed as districts got "closer" to transoceanic emigration ports. We thus leverage time variation in the evolution of the railway network to instrument U.S. emigration, and we confirm all the baseline results. Both instruments confirm the main results.

This paper is related to three streams of literature. First, we speak to the several contributions seeking to investigate the impact of emigration on sending countries, as opposed to the much more developed literature studying the economic and social effects of immigration.[13] This literature identifies human-capital accumulation as the key driver of economic growth fostered by emigration; it is fueled either by return migrants or by increased returns to schooling (Beine *et al.*, 2008; Dustmann *et al.*, 2011; Dinkelman & Mariotti, 2016; Akram *et al.*, 2017; Fernández-Sánchez, 2020). Evidence by Becker *et al.* (2020) in the context of forced migrations echoes these findings. We inform this literature by studying a different mechanism whereby emigration fosters the adoption of labor-saving technologies. We emphasize that this channel operates plausibly independently from human-capital accumulation.

Second, we contribute to the literature that studies the relationship between technology adoption and the supply of production inputs. Beyond the path-breaking contributions by Hicks (1932) and Habakkuk (1962), Hornbeck & 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. Similar to their paper, we emphasize the labor supply-shock mechanism. However, we focus on technology adoption, and leverage exogenous

[13] Borjas (1995, 2014) produced two influential reviews of this literature. Dustmann *et al.* (2016) discuss why empirical works studying immigration reach conflicting conclusions. Abramitzky & Boustan (2017) surveyed papers studying historical and contemporary U.S. immigration. Hatton & Williamson (2005) and Ferrie & Hatton (2014) provided two complementary works studying the role of immigration from the standpoint of global economic history. Clemens (2011) instead surveyed the literature studying the effects of emigration on sending countries.

variation in a DiD framework.[14] Several studies document the importance of technology adoption as a key driver of long-run growth, particularly in developing countries (Suri, 2011; Bryan *et al.*, 2014; Juhász *et al.*, 2022). Gerschenkron (1962) argues that technology adoption was a pivotal factor that enabled countries at the periphery of the industrialized world, such as Italy, to catch up with leading industrial nations. Moreover, while Andersson *et al.* (2022) study the effect of a labor *shortage*, this paper documents how excess labor stemming from immigration restriction policies shapes the adoption of new technologies.

Third, by virtue of its setting, this paper is related to the large, growing literature investigating the exceptionally broad social phenomenon represented by the Age of Mass Migration (for a review, see Abramitzky & Boustan, 2017). We owe our baseline empirical strategy to the approach pioneered by Abramitzky *et al.* (2019a), who leverage differential exposure to the Quota Acts to study how labor scarcity affected the United States. Several papers study both the short-run (Abramitzky *et al.*, 2014; Tabellini, 2020) as well as the long-run (Burchardi *et al.*, 2020; Sequeira *et al.*, 2020) effects of Transatlantic migration. Focusing on emigration countries, Karadja & Prawitz (2019) document that the mass migration fostered the demand for political change in Sweden. Circling back to Italy, Hatton & Williamson (1998) study the aggregate determinants of Italian emigration. Spitzer & Zimran (2020) validate the Gould (1980b) 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. In terms of methodology, we build the first highly comprehensive geographically disaggregated dataset of Italian emigrants during the years when the bulk of Italian mass migration took place (1900–1914). We also present newly digitized district-level data from population and industrial censuses. In terms of new findings, we show that the massive outflow of unskilled labor leaving Europe toward the Americas was unlikely to have hampered the structural shift towards manufacturing, even at the periphery of the (slowly) industrializing Old World. Our results suggest that the opposite impact prevailed: immigration *restriction* was what likely threatened economic modernization in Italy.

We structure the paper as follows. Section 2 describes Italian mass migration, the policies that shaped it, and the key economic characteristics of early 20th-century Italy. In Section 3, we discuss our data-collection contribution and our sources. In Section 4, we detail our empirical strategy, and we present our three sets of results. Section 5 presents our key robustness checks and our IV exercises. Section 6 concludes.

[14] We do not cover innovation, both because Italy performed poorly by standard indicators of innovation and because Italian firms were not on the technological frontier during this period (Vasta, 1999; Nuvolari & Vasta, 2015).

2 Historical Context

2.1 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 & Williamson, 1997). Even though Bandiera, *et al.* (2013) document that return 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 & Williamson, 1998, p. 75). Gould (1980a) vividly describes late-19th-century Italy as the archetypal case of 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, along with other Southern and Eastern European countries, didn't start experiencing mass emigration until the 1880s. The country's migration patterns over the 1870–1925 period display substantial time variation. Until the 1880s, its emigration rate remained relatively modest, and the bulk of its migrants hailed from Northern regions. Prohibitively high transportation costs and prevailing poverty in rural Southern areas largely inhibited migration from the *Mezzogiorno*.[15] 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 & Williamson, 1998, p. 100). By the 1890s, Italy had become the global leader both in sheer numbers of emigrants and in emigration rate, which grew from 5‰ in 1880 to a peak of 25‰ in 1913 (Hatton & Williamson, 1998, p. 95). Again, only Ireland had emigration rates comparable to Italy's during the Age of Mass Migration.

Italian emigration collapsed during World War 1 (WW1) but quickly regained momentum in the years immediately following the war. The epoch effectively came to an end by the early 1920s, when the U.S. Congress enacted a series of 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 the 1880s, Italy was a young nation rife with regional disparities spanning cultural and economic

[15] This term refers to Southern Italy, corresponding to NUTS-2 areas ITC and ITH. Regions within these areas are Lazio, Abruzzi e Molise, Campania, Puglie, Basilicata, Calabrie, Sicilia and Sardegna.

dimensions (Mack Smith, 1997). The resulting geographically segmented migratory patterns largely reflected this substantial heterogeneity and provide the backbone of our empirical strategy. Until the early 1880s, the vast majority of 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. The share of U.S.-bound migrants increased substantially over time in every Italian region. By the 1910s, the United States had become the primary transoceanic destination for all of Italy, though Northern migrants still tended to prefer continental European destinations.

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 a process of information diffusion. Information about emigration opportunities required time to spread across the country, and this diffusion accelerated as the volume of emigration increased. This process implied that emigration from different localities followed an S-curve, whereby emigration started slow, then picked up the pace, until eventually leveling off at saturation. 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 & Williamson, 1998, p. 99). Gould's hypothesis further strengthens our identification scheme. We leverage differential exposure of Italian districts to the U.S. Quota Acts to estimate the impact of a restrictive migration policy on economic development. Had migration decisions been exclusively driven by local economic conditions in the first place, our exclusion restriction may have turned weaker.[16]

Transportation costs may have also influenced international migration patterns. Systematic data on ticket fares are, to the best of our knowledge, lacking. Anecdotal evidence suggests that the price of a ticket from Naples to New York be around 170-190 *lire* at 1900-prices (Gomellini & O'Grada, 2011). By contrast, a third-class train from Naples to Milan would cost 100 *lire*, and one to Paris or Berlin would make another 100 (Camera dei Deputati, 1907, p. 14873). Gomellini & O'Grada (2011) suggest that a Southern unskilled laborer would make about 500 *lire* if he stayed at home, while in New York the figure would be around 2000-2500 *lire*. Compounding wage differentials between Italy and the US, these figures highlight that for the Southern population transatlantic migration was a far cheaper option than both internal relocations as well as continental out-migration. Differences in transportation costs, however, are unlikely to explain the choice between transoceanic destinations. Pérez (2021) documents that a ticket from Naples to Buenos Aires in 1902 would cost 170 *lire*. For Southern emigrants, social

[16] Spitzer & Zimran (2020) provide evidence consistent with Gould's diffusion hypothesis. They show that emigration began in a few districts in the 1870s and 1880s, then subsequently spread to nearby districts over time through immigrants' social networks.

networks rather than transportation costs, therefore, influenced the preferred emigration destination.

In the United States, Italian emigration was part of the "second wave" of immigration, coming mostly from Southern and Eastern Europe. Compared to first-wave countries such as England and Germany, poorer second-wave nations tended to supply less-educated, less-skilled migrants who experienced harder living conditions, assimilated more slowly and played economic catch-up with the natives for longer (Daniels, 2002; Abramitzky & Boustan, 2017; Albert *et al.*, 2021, p. 121). Italian emigrants, typically unskilled agricultural workers, were no exception. Because we exploit a migration policy shift to assess the impact of emigration on economic development, the potential endogenous selection of migrants may be relevant for our results.[17] Spitzer & Zimran (2018) nonetheless show that migrants from Southern regions, who constituted the bulk of transoceanic migration, were positively selected.

One last, largely overlooked component of labor migration in Italy during the Age of Mass Migration is internal 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. Second, internal mobility was largely temporary and seasonal, inherently different from transoceanic migration (Gallo, 2012, p. 32). Third, internal migrations reflected historically deep-rooted, persistent economic relationships between regions that are unlikely to influence our results on economic modernization in the 1930s (Gallo, 2012, p. 38).

2.2 Migration Policy in Italy and the United States

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, p. 55). The first such attempt at that was the 1888 Crispi-De Zerbi law, which introduced and regulated the contract of emigration between the migrant and the migration agency. The law was manifestly inadequate, however, to deal with the waves of migration that unfolded starting in the 1890s: it regarded emigration as an artificial phenomenon instigated by migration agencies and attempted to centralize its governance. Apart from a small measure to control ticket fares, it effectively failed (Foerster, 1924, p. 477).

[17] Consider the case of negative migrant selection. The additional manpower forced to remain in Italy by the restrictive U.S. migration policy shock would be of relatively low quality. This would confound and downward bias our estimated impact of migration on economic development.

Italian policymakers came to realize that emigration was more likely to *make* laws, rather than *abide* them (Foerster, 1924, p. 475). The 1901 emigration law was passed under the renewed understanding that emigration was no artificial phenomenon and that it could bear positive effects on Italy. 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 data on migrants. 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, p. 43).

Throughout this period, Italy either failed at or abstained from, enforcing emigration restrictions (Foerster, 1924, p. 501). The open-border policy enacted by the Italian government, coupled with (if not driven by) the overwhelming tide of migration flows, implies that emigration featured as a first-order dimension of Italian economic and social development.

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 such share ever since (Abramitzky *et al.*, 2014). The first lasting attempt to limit immigration was the Chinese Exclusion Act, which effectively halted Chinese immigration until its repeal in 1943.[18] In 1895, a bill was introduced by Henry Cabot Lodge requiring that a literacy test be administered to each immigrant upon arrival. Congress voted for the bill, but it was vetoed by President Cleveland in 1897. Two other such proposals were vetoed by Presidents Taft and Wilson in 1912 and 1915, respectively (Koven & Götzke, 2010, p. 130). A literacy-test law was eventually passed in 1917, but it was largely ineffective thanks to rising literacy rates in Europe (Goldin, 1994).

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, the economic and social conditions of immigrants. The Commission's 41-volume report favored "old" immigration countries such as England and Germany over "new," mainly Southern and Eastern European ones. The commission opined that because immigration from second-wave countries displayed higher return rates, migrants were less likely to assimilate (Higham, 1955). The highly influential report shaped numerous migration policy interventions. When immigration ramped up again after WW1, 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.

[18] The Chinese Exclusion Act was built on the 1875 Page Act, which banned Chinese women from immigrating. To date, these are the only U.S. laws to have explicitly targeted one ethnic group.

The 1921 Emergency Quota Act envisaged a (temporary) annual quota of 360,000 immigrants from Europe.[19] 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, disadvantaged countries newer to mass migration, consistent with the recommendations of the Dillingham Commission.

Abramitzky *et al.* (2019a) note that the 1924 Immigration Act had a highly heterogeneous impact on immigration across different 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 extremely small. The quotas assigned to Northern and Western European countries were comparatively generous. For our purposes, the 1921 and 1924 laws (henceforth, the Quota Acts) effectively halted Italian mass migration to the United States. Since the 1890s, America had been absorbing 30% to 40% of all Italian emigration, so the Quota Acts represented a major policy shock for Italy.

2.3 Technology Adoption and Economic Growth in Italy

Italy entered the Age of Mass Migration in the 1880s. The country was in the midst of an agrarian crisis (Toniolo, 2014, pp. 60-73) that followed two decades of stagnation. The period from 1895 to 1913 was the only time until the 1950s "economic miracle" in which Italy managed to outperform and narrow the income gap with the leading industrial nations. In the 1920s and 1930s, during the Fascist period, Italy was still a mainly agricultural country, featuring low income per capita and stagnating productivity (Cohen & Federico, 2001, p. 23). 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 agricultural policies resulted in neither substantial intervention nor sizeable progress (Zamagni, 1990, p. 262). All in all, growth slowed after 1925 and regional disparities further widened (Cohen & Federico, 2001, p. 15). 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 regional inequality.

We relate the migration shock to diminished investment in capital goods, especially technologically advanced ones, and to a shift to labor-intensive production routines. Italy was nowhere near the technological frontier throughout the period, and skill premia actually *declined* from the 1890s onward (Vasta, 1999; Federico *et al.*, 2019). Like today's developing countries, Italy lagged behind large

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

industrial nations in research-and-development expenditures, and it imported substantial amounts of foreign technology, both patents, and machinery. Whenever possible, Italian firms bundled different vintages of capital, adding new machines to existing ones instead of renovating the whole stock (Cohen & Federico, 2001, p. 51). 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, we find that 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 process of modernization they had been undergoing prior to the Quota Acts.

3 Data

Our analysis spans the years 1881 to 1936. We collected data from a number of sources; we stacked the data by census years and analyzed them at the *circondario* (henceforth, "district") level of aggregation.[20] In 1921, there were 216 districts, each consisting of a variable number of municipalities (see Online Appendix section A for a complete description of the data). Because districts were abolished in 1927, all subsequent data are collected at the municipality level and aggregated at the 1921-district boundaries. Table I reports summary statistics for the variables in our final dataset.

3.1 Emigration

Italian official emigration statistics are of limited scope because out-migration flows were recorded at the province-level of aggregation (Hatton & Williamson, 1998). Province-level data are not suited for quantitative analysis, because provinces were relatively large: in 1921, there were only 60 provinces that together contained a population of approximately 20 million. This naturally limits the use of official statistics for an econometric exercise. We nonetheless digitize province-level emigration outflows and use them to validate the series we derive from the dataset that we assemble (see Online Appendix section A.1.3).

To overcome this issue and study the Italian mass migration to the United States, we collected administrative records of Italians who entered the country between 1892 and 1930 through the Ellis

[20]Population censuses were taken in 1881, 1901, 1911, 1921, 1931, and 1936. We do not include data prior to 1901 in our baseline analysis, except for population. 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. In Online Appendix section A.2 we discuss more in detail the sources that we digitized and present a visual summary of all the variables we analyze.

Island immigration station.[21] This was by far the largest, though not the only, immigration gateway during this period.[22] Administrative records report, for the vast majority of migrants, name and surname, year of arrival, age, municipality of origin, and sailing ship. In this study, we concentrate on the migration year and the municipality of origin. Ultimately, we collected approximately 2.7 million individual observations spanning the years 1890 to 1930.

Because all data were recorded by U.S. officials, the municipality variable displays frequent coding errors. We adapted the matching procedure from Abramitzky *et al.* (2014), using a sound-spelling similarity metric to account for orthographic and misspelling errors[23]. We then set a threshold measure below which we accepted the best-matched municipality and above which we dropped the observation; we then ran robustness checks around this threshold. In our preferred specification, we were able to match 1.6 million migrants to their origin municipality. Among those, 800,000 are coded with no error. We mapped each municipality to the district it belonged to in 1921, then we computed district-level yearly flows. To the best of our knowledge, this is the most comprehensive data spanning the whole Age of Mass Migration for Italy, at this level of aggregation.[24] In figure I, we plot the overall country-level yearly inflow of emigrants who landed in Ellis Island from 1890 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, then was definitively shut down by the Quota Acts in 1921 and 1924. Our data are consistent with both comprehensive U.S. immigration data and overall Italian migration patterns (Brum, 2019; Sequeira *et al.*, 2020). In Figure II, we plot the geographical distribution of migrants across districts. The upper panel displays variation in the emigrants-to-population ratio, i.e., the emigration rate. The lower panel reports unconditional variation in the U.S. emigrants-to-population ratio, which is the baseline measure for treatment exposure. Both figures normalize emigration by population in 1880 and report the resulting standardized series.

[21] These records are freely available at `heritage.statueofliberty.org`. We run queries over a comprehensive pool of 20,000 Italian surnames over 1890–1930 period. In Online Appendix section A.1.3 we document that our newly constructed series correlates well with existing—albeit less granular—emigration data from official statistics.

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

[23] In section A.1.1 in the Online Appendix we discuss more in detail the methodology we used to correct coding errors. In section A.1.2 we show that immigrants whose origin municipality was not recorded represent, in every year, less than 1% of the overall sample.

[24] The only other geographically disaggregated data available to date for this period are those collected by Brum (2019) and Fontana *et al.* (2020). Both, however, focus on the pre-1900 period. Our dataset is thus the only one covering the years when the bulk of the mass migration took place (1900–1914).

3.2 Population

We digitize information from six population censuses: in 1881, 1901, 1911, 1921, 1931, and 1936. The main outcome variable is the share of workers in industrial sectors. This variable, as well as total employment in several other sectors, is available for each district between 1901 and 1921. We digitized the 1931 and 1936 census data at the municipality level, then aggregated them at the district level. More granular data on employment for major manufacturing sectors are, unfortunately, only available until 1921. For the remaining years, we digitized them from manufacturing censuses, with the caveat that these are at the province level and are imputed to districts, as described in the next paragraph. The population of each municipality was compiled by the Italian statistical office (ISTAT), and we aggregated it by districts. We computed the k-urbanization rate of a given district as the share of people living in municipalities of population k or higher in that district, relative to the district's population. In some robustness checks, we control for the altitude, area, and population density of the districts.

3.3 Economic Activity

To measure shifts in the adoption of capital-intensive technology, we digitized province-level data from the 1911, 1927, and 1937 manufacturing censuses. Manufacturing censuses gathered information on the universe of firms operating in each province at the time of census completion; they provide valuable information about the amount and vintage of capital goods employed by firms. We collect 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 in those years (Gaggl *et al.*, 2021). This allows us to disentangle the possibly differential impact of the labor supply shock induced by the migration shock on different technology vintages. Industrial census data are available only at the province level. To impute them to districts, we regressed province-level outcome variables against the number of workers in each sector, controlling for population, province, and year-fixed effects. Then, from the resulting OLS estimates, we predicted the associated district-level variables.[25]

[25] In Online Appendix section A.2.1 we explain how we conduct the imputation of province-level data to districts. We then validate our imputation methodology by comparing imputed and measured variables.

3.4 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.[26] The dataset provides rich information on Italian military personnel who died during WW1. Importantly for our analysis, it includes the municipality of origin of each soldier. Because we conducted our analysis at the district level, we collapse the dataset from municipalities to 1921 districts, and we measured the war's severity in a given district as the ratio between deaths and population in 1910. In Tables B.3 and B.11, we report all our results, further controlling for this measure interacted with a posttreatment indicator, and we confirm our baseline estimates.

To implement our railway instrumental variable, we digitized the entire Italian railway network over 1839–1926 period.[27] For each railway section, we know all the stations it is connected to. Stations are generally labeled in terms of the municipality they were located in. 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, as well as the distance it covered and the traction system the trains employed. We use these data to construct the Italian railway network. To capture its evolution over time, we took snapshots of the network at decade frequency.

4 Results

4.1 Empirical Strategy

In this section we explain the baseline empirical strategy we apply to estimate the causal impact of the Quota Acts on technology adoption and the dynamics of labor supply. Our identification relies on geographic variation in emigration patterns and intensity across districts in the pre-quota period.[28] Consider for the sake of argument two ideal districts; call them A and B. From 1890 to 1924, many Italians emigrated from both districts. However, most emigrants from district A headed toward the

[26] Death records were collected 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.* (2020) were among the first to use them in the economics literature.

[27] 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 the best of our knowledge, this is the first paper to use these data.

[28] This identification scheme therefore mirrors that of Abramitzky *et al.* (2019a), who exploit different immigration patterns by country of origin across U.S. counties and the Quota Acts shock to estimate the economic effects of *immigration*.

United States, whereas none from district B did. District A will thus be more exposed to the emigration restriction shock relative to district B. This is the case because social networks and information diffusion exerted a powerful pull, influencing potential emigrants through previous generations' emigrants (Spitzer & Zimran, 2020). This induced substantial persistence in emigration patterns by country of destination. Districts that had experienced higher emigration toward the United States before the Quota Acts were therefore comparatively more exposed to the migration restriction shock relative to those districts whose emigrants headed mainly toward European and South American countries.

Reality was more nuanced than our example. Emigrants left from all districts and headed to numerous destinations, hence the intensity of quota exposure varies smoothly with respect to the relative emigrant outflows to the United States. Importantly, the existing dispersion of U.S.-bound emigrants by district of origin shown in Figure II ensures that emigration location choices were not systematically correlated with economic development. In other words, we allow the decision to emigrate to be correlated with economic performance at home. What we restrict to be conditionally orthogonal to economic performance is the decision of *where* to emigrate.[29] Our identification assumption—in jargon, parallel trends—thus relies on the key assumption that districts with similar relative emigration outflow but with different destinations would not have undergone differential development patterns had the Quota Acts not been enacted. The wide divide between Northern and Southern regions could threaten our identification scheme. In Online Appendix Tables B.3 and B.11, we show that our baseline results are robust if we include a large set of covariates measured before the Acts, interacted with a year time trend, as further controls. In particular, we show that including an interaction between a Southern dummy and a posttreatment indicator does not qualitatively alter the results. This implies that our estimated effects do not critically depend on a Northern-Southern comparison.

We measure quota exposure of district c as

$$\text{QE}_c = \frac{1}{\text{Population}_{c,1880}} \sum_{t=1890}^{1924} \text{US Emigrants}_{c,t} = \frac{\text{US Emigrants}_c}{\text{Population}_{c,1880}} \tag{1}$$

where $\text{Population}_{c,1880}$ is the population of district c in 1880, and $\text{US Emigrants}_{c,t}$ is the number of emigrants who headed to the United States over the period. Since mass outmigration started in the 1890s, in equation (1) we normalize the total number of U.S. emigrants with district population in 1880 to ensure that the measure for quota exposure does not conflate confounding variation due to aggregate emigration. Quota exposure in equation (1) can be further decomposed as

$$\text{QE}_c = \underbrace{\frac{\text{US Emigrants}_c}{\text{Emigrants}_c}}_{\text{Intensive margin} \equiv \text{IM}_c} \times \underbrace{\frac{\text{Emigrants}_c}{\text{Population}_{c,1880}}}_{\text{Extensive margin} \equiv \text{EM}_c} \tag{2}$$

where Emigrants_c is the total number of emigrants. The intensive margin (IM) of exposure measures

[29] In Section 5.2, we present a simple instrumental variable that further addresses the possible residual correlation between intensity of exposure to the Quota Acts and economic performance of districts.

the relative importance of the United States as an emigration destination; the extensive margin (EM) measures the relative importance of emigration overall. For a district to have high quota exposure, we thus require that (i) cumulative emigrants are a non-negligible share of the 1900 population, and (ii) a non-negligible share of those emigrants headed toward the United States. By contrast, districts with both little overall and little U.S.-bound emigration are at the bottom of the distribution of QE. In our preferred specification, we control for the extensive margin to compare districts with similar emigration rates but different destinations, hence exposure. This is because, while the decision to emigrate is likely endogenous to economic development, the destination should be conditionally quasi-random. In Section 5, we show that results are robust to two different instrumental variables exploiting a shift-share design, as well as time-varying access to the railway network. We construct a measure for EM using province-level data of total emigration available in the census, and we assume constant emigration rates within each province.[30] Figure II plots the geographical variation in EM and QE. We view the figure as supportive evidence that variation in QE is quasi-exogenous upon conditioning on the extensive emigration margin.

Quota exposure defined in equation (1) serves as our baseline treatment. Our dataset is a panel of districts, observed every census year between 1901 and 1936. Throughout the rest of the paper, we estimate variations on the following DiD model:

$$y_{c,t} = \gamma_c + \gamma_t + \boldsymbol{x}'_{c,t}\boldsymbol{\beta} + \delta_1 \left(\text{EM}_c \times \text{Post}_t\right) + \delta_2 \left(\text{QE}_c \times \text{Post}_t\right) + \varepsilon_{c,t} \tag{3}$$

where y is the log-difference of a generic outcome variable, $\boldsymbol{x}$ is a vector of additional controls, and Post_t is an indicator that is equal to one if $t > 1924$.[31] The baseline specification includes district and time fixed-effects, and standard errors are heteroskedasticity-robust and clustered at the district level unless otherwise specified. Baseline controls are labor market slackness and population. The geographic variation in the treatment is shown in the bottom panel of Figure II, where we normalize total U.S. emigration outflows by 1880 population. The term δ_2 then captures the impact of the emigration restriction shock on the outcome variable y. In all regressions, we control for the emigration rate (EM) because our identification scheme relies on the fact that districts with similar emigration rates but different destinations would not have undergone differential development patterns had the Quota Acts not been enacted. In a series of robustness checks (discussed in detail in Section 5), we control for variation due to WW1, measurement errors in the years following the Quota Acts due to changes in registration procedures at Ellis Island, and possible correlation between QE and the error term.

[30] Since district-level data on overall migration do not exist, we cannot test this assumption. However, using district-level U.S. emigration figures, we find that within-province U.S. emigration rates do not substantially differ across districts.

[31] Congress passed the first restrictive migration law—the Emergency Quota Act—in May 1921. The Immigration Act of 1924 further restricted the number of Italians allowed in the US every year. The choice between 1921 and 1924 as the treatment year is however immaterial since we do not observe districts within the two Acts.

There is evidence, moreover, that emigration fosters economic ties, chiefly through international trade, between immigration and emigration countries (*e.g.* Dunlevy & Hutchinson, 1999). We account for this by including the interaction between US GDP and Quota exposure as a further control. This captures demand-type shocks which US emigration districts could be exposed to, depending on the state of the US business cycle.

Causal inference on estimates of model (3) requires that the treatment and control groups were on the same trend before the treatment (the Quota Acts) occurred. Because no census was taken in 1891, to test the parallel trends assumption we need to interpolate data points between 1881 and 1901. In the Online Appendix—in Figures C.1, C.2, and C.3—we report the results of these event-study regressions and provide convincing evidence in favor of the parallel-trends assumption. All figures report the estimated coefficient of our baseline treatment interacted with decade dummies. Under the parallel-trends assumption, we expect all coefficients before the treatment period not to be statistically significantly different from zero, as we observe at standard confidence levels. In Table II, we instead report correlations between the outcome variables we collect and the measure for quota exposure, conditional on the extensive emigration margin, population, and province fixed effects for 1911 and 1921. This exercise is not ideal in that we cannot clean for year-fixed effects, but it nonetheless strongly suggests that the treatment and control groups are comparable at all standard confidence levels before the treatment period. In fact, we find that none of the outcome variables we examine has a significantly different-from-zero correlation with the treatment before 1921.

4.2 Emigration and Technology Adoption

We study how technology adoption and investment in capital goods by manufacturing firms responded to the IRP shock. To do this, we collect several proxies for capital investment from the manufacturing census, and we report estimates of model (3) for these various outcomes. Our two baseline measures of investment in capital goods are the number of engines and their installed horsepower capacity. We distinguish between traditional mechanical engines and technologically advanced electrical ones. The electrical engine, in particular, was a defining innovation of the Second Industrial Revolution, yielding substantial productivity gains relative to older mechanical engines (Mokyr, 1998). Importantly, electrical engines were more labor-saving than electrical ones. We, therefore, interpret investment in electrical engines as a proxy for the adoption of advanced, labor-saving technology, a key driver of long-run economic growth (Juhász *et al.*, 2022).

U.S. observers evocatively described the turn of the 20th century as the Age of Electricity. In 1900, horsepower produced by electrical engines accounted for a mere 5% of overall consumption for production purposes. Two decades after, this figure had risen to 50% (David, 1990). Though productivity growth was relatively slow to manifest, it nonetheless became apparent starting in the early 1920s.

Italian firms were latecomers to technology adoption (Cohen & Federico, 2001). Hence, it seems plausible that well into the 1930s, electricity represented a major source of potential productivity growth. Despite the large productivity gains they could yield, Italian firms were slow to adopt electrical engines. Capital stocks in the early phase of adoption were a patchwork of different engine vintages. All these implied that, in the United States, capital-per-worker increased following the introduction of electrical engines (David, 1990). We document a different pattern in Italy in the aftermath of the IRP shock.

Table III reports the baseline results. We employ six outcome variables to measure investment in capital goods and technology adoption, and we estimate the causal impact of the Quota Acts in model (3), controlling for the extensive emigration margin, population, labor-market slackness, and district and year fixed effects. From left to right, the columns display the total number of firms, the number of firms with at least one engine of any vintage, the sheer number of mechanical and electrical engines, and the horsepower of mechanical and electrical engines. As in all other regression tables, the first row displays the DiD coefficient δ_2.[32] We find that investment in mechanical and electrical engines alike declined substantially in more-exposed districts, whether such exposure is measured as the sheer number of installed engines or in terms of generated horsepower. In terms of magnitude, however, the effect of the IRP shock is stronger for electrical engines. Our results are qualitatively unchanged if we restrict the estimation sample to Southern regions.[33]

To rationalize this finding, we build on Andersson *et al.* (2022), who hypothesized that emigration fosters invention and adoption of labor-saving technology because it makes labor a relatively scarce production input. We take the specular perspective, arguing that the Quota Acts, and IRPs more broadly, induced a geographically segmented positive labor supply shock. Districts that before the Acts had experienced high U.S.-bound emigration rates were more exposed to the policy shock, because they ended up having disproportionately more "missing migrants." If missing migrants at least partly joined the local employment pool, then those districts were subject to a positive labor supply shock. On the other hand, districts whose emigrants headed toward destinations other than the United States did not undergo any such shock, because emigration to those countries remained unrestricted after the Quota Acts. Directed technical change and adoption theory thus suggests that firms in treated districts would be motivated to decrease investment in capital goods and to substitute capital with labor, which had

[32] The negative coefficient associated to the interaction between the extensive emigration margin and the post-treatment indicator could reflect the fact that emigration districts were negatively selected.

[33] Southern regions include all but EU NUTS 2 ITC and ITH regions. In other words, we drop Aosta Valley, Piedmont, Lombardy, Liguria, Trentino-Alto Adige, Veneto, Friuli-Venezia Giulia, and Emilia-Romagna.

become a more abundant production input following the IRP-induced shock. We devote the rest of the paper to validating this hypothesis.

An obvious corollary of this hypothesis is that production technologies in more heavily treated districts should become more labor-intensive. We assess this in Table IV. To measure labor intensity in production, we calculate the ratio of the number of workers employed in manufacturing to all the previous outcome variables. We thus measure how labor-intensive production technologies were across districts. We find that the number of industrial workers per unit of capital increased. This again holds if we measure capital in terms of the number of installed engines, or in terms of horsepower generated. In terms of magnitude, the effect of the IRP is comparable across vintages—a 1% increase in QE leads to a 0.6% increase in the worker-to-capital ratio for both electrical and mechanical engines.

Finally, we ask whether the effects of the IRP shock are distributed evenly across industrial sectors. To answer this, we repeat the exercise of Table III for each sector recorded by the manufacturing census.[34] We end up with six sectors, whose estimated DiD coefficients for the various outcomes we report in Figure III. We document sizable heterogeneity across sectors. Firms in relatively backward First Industrial Revolution sectors, particularly textiles and construction, reduced investment in capital goods. This effect is stronger for more-advanced electrical engines. On the other hand, we find that capital investment and adoption of electrical engines by firms in modern sectors, such as chemicals and metallurgy, display a less-marked decrease.[35] The sector-level analysis yields sharper predictions for our directed technical adoption hypothesis. Under this interpretation, we would expect employment in First Industrial Revolution sectors to grow more than in modern ones because firms in the former sectors were apparently eager to substitute capital for newly available labor. We evaluate this prediction in Section 4.4.

4.3 Emigration and Population Growth

Here, we document that districts more exposed to the migration shock experienced subsequent higher population growth. We view this as evidence confirming our narrative, whereby emigration restriction imposes a positive labor supply shock on the emigrants' country of origin. We thus estimate model (3), setting population growth as the outcome variable; we report the resulting estimates in Table V. We compare the estimates obtained from the baseline continuous treatment, as well as those with a

[34] We do not include "other industries" or "public service industries" in the analysis—the former is a residual category with little economic meaning, and data for the latter are not available in later censuses.

[35] We broadly classify manufacturing sectors based on narrative historical evidence presented by Mokyr (1998). Textiles and construction are therefore more closely associated with the First Industrial Revolution, whereas chemicals and steel-working refer to the Second Industrial Revolution (sometimes called the technological revolution).

categorical dummy treatment equal to one for districts whose exposure is above the median, and zero otherwise. In all regressions, we control for the extensive emigration margin, population, labor-market slackness, and district and year fixed effects.

The estimated DiD coefficient (δ_2) confirms that districts that were more exposed to the Quota Acts experienced higher population growth. This effect is always statistically different from zero. Importantly, significance does not vanish if we restrict the sample only to Southern districts, where the exclusion restriction is sharper. We view this result as confirming that our measure of quota exposure is sound. Districts with more outstanding U.S.-bound emigrant stocks experienced less emigration, which triggered higher population growth in the years following the Quota Acts. Though studying the precise mechanism driving this result is beyond the purpose of this paper, this finding is consistent with pull factors, such as social networks and information diffusion, exerting better influence in more-exposed districts. Table V shows that the significance and magnitude of the DiD coefficient δ_2 both increase once we control for the extensive margins of U.S.-bound emigration.

Implicitly, Table V provides evidence against mechanisms that could threaten our source of identifying variation. The mechanism we emphasize relies on the fact that at least some of the missing migrants join the local workforce. This may not hold if potential U.S.-bound migrants substituted their decision by either (i) emigrating to unrestricted countries or (ii) migrating internally. In Online Appendix Table B.2 and Figure C.6, we provide evidence against both interpretations. However, if either international or internal substitution were in place, we would not observe any positive effect of IRP exposure on population growth, because missing migrants in exposed districts would not be missing altogether.

4.4 Emigration and Industrialization

In the previous subsection, we provide evidence that the Quota Acts increased labor supply in exposed districts. We now ask whether this translated into increased employment and, if so, whether there is heterogeneity across sectors. Historical scholarship suggests that emigrants could, potentially, take on industrial jobs. First, Italian emigrants to the United States were largely unskilled workers who took low-qualification jobs in manufacturing (Abramitzky & Boustan, 2017). Second, Italian firms during this period relied mostly on unskilled workers and employed labor-intensive production technologies (Cohen & Federico, 2001, p. 60). Hence, the increased supply of unskilled labor could be compatible with the demand by firms. To test this, we estimate model (3), taking as outcome variables changes in the number of workers employed in agriculture and manufacturing, as well as changes in the share of workers employed in both sectors as a fraction of overall employment.[36] As an alternative measure for broader modernization, we use the urbanization rate, calculated as the share of citizens living in municipalities with more than 5,000 inhabitants.[37]

In Table VI, we show that while agricultural employment did not significantly react to the Quota Acts, industrial employment increased substantially.[38] This effect is consistent with the evidence presented in Table III, which documents that firms in *manufacturing* decreased their investment in capital goods following the IRP shock. Taken together, these results suggest that manufacturing firms in exposed districts took advantage of more abundant labor unleashed by the IRPs and substituted capital investment with (now cheaper) labor. This evidence is therefore consistent with our directed technical adoption narrative.

In Table VII, we repeat the exercise but consider changes in the *share* of industrial and agricultural workers as the main outcome variable. We interpret the share of industrial workers as one further indicator of industrialization, whereas the opposite holds with respect to the share of workers employed in agriculture.[39] Because overall employment hardly reacts to the Quota Acts, industrial employment grows and agricultural employment does not, and the share of workers employed in manufacturing increases. Similarly, the share of workers employed in industry surged. Because industrial firms were the driving force behind economic and social progress during this period, Table VII may suggest that the Quota Acts contributed to the modernization of the Italian economy, pushing comparatively more workers into modern industrial sectors. Finally, in the last column, we report that urbanization hardly increased in exposed districts. This might be driven by the fact that manufacturing firms were not located in urban centers, as shown in figure C.9.

In Figure III, we document sizable heterogeneity in capital investment and technology adoption decisions across sectors. We now ask whether the directed technical adoption mechanism allows the reconciliation of these dynamics with changes in sector-level industrial employment. We therefore estimate the baseline DiD model for the six sectors whose employment was collected in the population

[36] We harmonize the definition of industrial firms across censuses. For instance, transportation firms were not recorded as industrial firms in 1931, though they were in all other censuses.

[37] Urbanization has been widely used as a proxy for economic modernization. Among others, see Boustan *et al.* (2018) and Sequeira *et al.* (2020). We set the urban threshold at 5,000 inhabitants as this was the median city size before the Mass Migration (1881).

[38] The OLS estimates report a modest decrease in agriculture employment. The estimated coefficient is marginally significant at the 10% level and small in magnitude. Moreover, the IV estimates of the agriculture coefficient are not significant. We conclude that agriculture employment did not react to the Quota shock.

[39] Our theory predicts that the *number* of workers employed in manufacturing in exposed districts should increase, whereas we do not expect any such effect on agriculture. In turn, this implies that the *share* of workers employed in manufacturing should increase and that the share in agriculture should decrease.

and manufacturing censuses. The outcome variable in each regression is the growth rate in sector employment, and we control for aggregate manufacturing employment growth. This is because we are interested in understanding which industrial sectors grew more relative to the increase in aggregate industrial employment. We report the results of this exercise in Table VIII, where the first row displays the estimated impact of quota exposure. Employment dynamics reflect the heterogeneity in capital investment decisions. Employment in agriculture and fishing in more-exposed districts decreased. On the other hand, firms in First Industrial Revolution sectors—chiefly textiles, but construction as well—increased their labor stock. Moreover, we find that employment in the two distinctively Second Industrial Revolution sectors—namely chemicals and metallurgy—reacted less to the IRP shock, although we still find an increase in comparatively more-exposed districts. These results are entirely consistent with evidence reported in Figure III. Our results suggest that faced with more-abundant unskilled labor, firms in textiles and construction substituted capital with labor, increasing employment and cutting investment in capital goods. By contrast, industrial firms in the agriculture sector reduced their overall labor stock and increased investment in capital goods. High value-added sectors did not respond as much to the labor supply shock, displaying smaller changes in their employment stock and investment in physical capital. All these findings are consistent with the baseline directed technical adoption narrative, and therefore provide evidence in favor of our proposed mechanism.

4.5 Discussion and Alternative Mechanisms

We have documented that the Quota Acts, arguably one of the most sudden and restrictive immigration restriction policies in modern history, led to decreased investment in capital goods and hampered technology adoption in more-exposed districts. To rationalize these findings, we showed that the IRP induced a larger positive labor supply shock in more-exposed districts. Throughout the paper, we have interpreted this evidence through the lens of directed technical change and adoption theory. 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 & 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 that we estimate 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 find that the bulk of employment gains and capital investment losses materialized in First Industrial Revolution sectors. These occurred in traditionally low-skilled and labor-intensive manufacturing, especially in Southern regions (A'Hearn, 1998). Hence it is unlikely that high-skilled workers would be comparatively more productive there. Second, we run a battery of robustness checks—see Online Appendix Tables B.3 and B.11. When we include the literacy rate as a proxy for average human capital in our regressions, results hold.

Along with 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, we consequently abstracted from including remittances in our analysis, more so given that existing data are of questionable reliability at best. Remittance dynamics 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. Inasmuch as within-household cash transfers result in aggregate savings, remittances may accrue to overall investment dynamics (Rapoport & 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., Dinkelman & Mariotti, 2016; Fernández-Sánchez, 2020). 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. To quantify this concern, we run several robustness checks where we control for average human capital. The results of these exercises fully confirm our baseline estimates.

A plausible concern for our empirical strategy is that after the Quota Acts, emigrants simply substituted the United States with either internal or international unrestricted destinations.[40] Our main argument against this interpretation is backed by evidence in Table V. If emigrants substituted the United States with other destinations, we would expect no effect of exposure to the Quota Acts on population growth. Given the persistence of demographic dynamics, it is unlikely that alternative explanations can

[40] If the Quotas fostered labor mobility within Italy, our estimates may fail to reflect the productivity gains this could induce (Bryan & Morten, 2019).

account for such a sharp, sizable increase that is correlated with the conditionally exogenous variation we exploit. Disaggregated emigration data toward countries other than the United States does not exist. However, in Figure C.6, we report aggregate outflows toward the four main emigration destinations, before and after the treatment period(s). We show that the United States is the only country where immigration significantly departs from its historical level, except during WWI.[41] Moreover, the sheer numbers of internal migrations cannot account for the drop in U.S.-bound out-migration (Gallo, 2012). In Table B.2, we show that in no Southern region did the gross outflow to Northern regions from 1921 to 1931 exceed 10% of U.S.-bound emigrants from 1910 to 1920. Qualitative and quantitative evidence alike, therefore, call for dismissing the emigration substitution argument.

A second 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 throughout this paper against this interpretation. Historical scholarship, however, notes that assimilation patterns of Italian immigrants in the United States and Argentina during this period substantially differed (Klein, 1983).[42] 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, whereas 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, namely wages and output (productivity). Studying wages would be informative because directed technical adoption hinges on the relatively more abundant labor becoming relatively cheaper. An analysis of wages could reveal this pattern, which we currently implicitly assume. Geographically disaggregated data on wages, unfortunately, do not exist. Productivity would, in turn, be key to investigating the

[41] These four countries are the United States, France, Argentina, and Brazil. Taken together, emigrants heading toward these destinations accounted for 70% of the total outflow. We predict the number of emigrants after 1924 using historical emigration before 1914. We show that the United States was the only country whose inflow falls relative to the prediction based on historical data after the Quota Acts.

[42] 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.

welfare effects of the Quota Acts. However, disaggregated data on output were not recorded until 1936; hence, we lack a time series covering the period we study.

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., Grogger & Hanson, 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.

5 Robustness Checks

In this section, we summarize our main robustness checks.[43] We essentially address two empirical problems. First, we provide evidence that our results so far are robust to alternative measures of treatment exposure across districts. Second, we propose two simple instrumental variables to deal with potential endogeneity issues relating to our estimates.

5.1 Alternative Measures of Treatment Exposure

There are two margins along which measured quota exposure may be subject to mismeasurement. First, while most Ellis Island records after 1900 report the district of origin, this is not true for the years 1890 to 1900. Records for these years most often only report "Italy" as the origin of a migrant.[44] Similarly, after the 1924 Emergency Act was enacted, Ellis Island authorities largely stopped recording immigrants' municipalities of origin. If there were systematic patterns underlying whether migrants were recorded with their district of origin or were simply recorded as Italian, then our measure would suffer from bias. Second, as discussed in Section 2, though emigration collapsed during WW1, it did not completely dry out. During the war, districts closer to emigration ports are in fact disproportionately represented relative to previous shares.[45] This induces spurious variation in measured quota exposure,

[43] See the Online Appendix, Sections B and C, for detailed tables reporting the results which we discuss here.

[44] Online Appendix section A.1.2 reports the number of migrants whose origin we label as missing. The share of Ellis Island immigrants with missing origin never exceeds 1% of the overall number of immigrants in any given year over the period 1892-1924.

[45] Throughout this period, emigrants could sail overseas only from Naples, Palermo, or Genoa. In Online Appendix section A.1.3 we show that the correlation between our newly constructed emigration series and official statistics is lowest during the WW1

as we would impute higher exposure to some districts by sole virtue of their geographic position.

The first robustness check we thus consider restricts the sample years over which quota exposure is computed. In our baseline specification of equation (1), we measure the exposure of a given district as the share of people who migrated from that district from 1890 to 1924, relative to that district's population in 1880. To make sure that emigration registration procedures and WW1 do not induce systematic measurement error in our estimates, we introduce two other treatment variables. As a first alternative, we consider only emigrants who left no later than 1921. Then, we further restrict the sub-sample to the years before the outbreak of WWI. The first alternative measure seeks to control for the fact that the Ellis Island database lacks information about the municipality of origin for a high number of Italian migrants after 1921. We thus aim to clean for possible measurement error due to the nonrandom selection of registered district locations. The second exposure measure drops emigrants who left after WWI started, as emigration opportunities were possibly affected by proximity to departure ports. In particular, emigrants from districts nearer to ports could be over-represented.

Our baseline results are robust to these different measures of quota exposure, as shown in Online Appendix tables B.17-B.18-B.19. Most likely, this is because the bulk of emigration took place before 1914, hence restricting the sample to the years before WW1 does not substantially affect our estimated treatment exposure. In particular, though districts closer to ports are over-represented in emigration statistics during WW1, the absolute number of emigrants was negligible relative to previous years, as WWI induced a marked collapse in those districts as well. Finally, emigrants lacking a recorded district of origin constitute the majority for the post-1924 period. Yet, we find no noticeable pattern inducing nonrandom recording across districts. Hence, measured quota exposure should not be mismeasured whether we include those years or not, as confirmed by the estimated coefficients. One further concern is that our results might be driven by remote migration patterns. According to the Gould (1980a) hypothesis, in fact, out-migration from any given region would eventually saturate over time. Hence, it might be that our estimated effects are driven by districts whose out-migration stretches back to years before the Quota shock becomes salient. Similarly, one may wonder whether it is instead more recent emigration waves that drive the results. In Online Appendix tables B.17-B.18-B.19 we address these concerns by constructing two measures of Quota Exposure which assign increasing or decreasing weights on more recent out-migration flows. We find that all our baseline results hold.

years. We thus report robustness regressions excluding those years from our measured Quota Exposure, and confirm all our baseline estimates.

5.2 Shift-Share Instrumental Variable

A possible concern for our identification strategy is that geographical variation in exposure to the U.S. immigration quotas was not conditionally random across districts. While we provided historical and quantitative evidence against this argument, ultimately the exclusion restriction cannot be formally tested. We, therefore, develop an instrument close in spirit to that presented in Card (2001) and Tabellini (2020) to address a similar—although specular—issue.

Let $\omega_{cr}^{T} \equiv \sum_{\tau=0}^{T} \text{US Emigrants}_{c,\tau} / \text{US Emigrants}_{T}$ be the share of emigrants from district c in region—or province—r until time T ($\text{US Emigrants}_{c,T}$) relative to total emigration (US Emigrants_{T}). We predict total emigrant outflow from district c from the following "zero-stage" equation:

$$\widehat{\text{US Emigrants}}_{cr}^{T} = \omega_{cr}^{T} \times \sum_{\tau=1890}^{1924} \sum_{c' \notin r} \text{US Emigrants}_{c',\tau} = \omega_{cr}^{T} \times \text{US Emigrants}_{-r} \tag{4}$$

In the first stage, we instrument QE_{cr} using $\widehat{\text{US Emigrants}}_{cr}^{T}$, then we plug the resulting predicted $\widehat{\text{QE}}_{cr}^{T}$ into the second-stage regression to estimate the baseline model (3). To strengthen the validity of our OLS estimates, we pick T to be before the bulk of the Mass Migration period. Thus, predicted district-level U.S. emigration outflows wash out spurious variation in U.S. emigration due to emigration—endogenously—affecting economic development in emigration districts, conditional on district and year-fixed effects.

The instrumental variable (4) exploits two sources of variation. Cross-sectional variation is embedded in the (ω_{cr}^{T}) term. It captures heterogeneity in the origin districts of migrants at a given point in time (t). We can modulate the choice of T so that the distribution of emigrants across districts is more plausibly driven by exogenous information diffusion, and less so by economic outcomes (Spitzer & Zimran, 2020). Time series variation, captured by (US Emigrants_{-r}), is driven by changes in the aggregate emigration outflow, excluding the instrumenting district c, and possibly all other districts in the same region (or province). This "leave-out" strategy ensures that our instrument is not correlated with the economic performance of districts in region r, hence mitigating the concern that quota exposure could be correlated with district-level economic performance hence inducing endogeneity and bias our estimated coefficients. By changing T, we address the possible concern that WWI altered the composition of Italian emigrants to the United States in a spatially nonrandom fashion.

In Table B.12, we summarize the results of the first-stage regressions, where we vary measured quota exposure as discussed in Section 5.1. We also control for different baseline years T in the construction of the Shift-Share Instrument to make sure emigration patterns reflect district-level variation, which is not correlated with economic performance. The first stage is statistically significant because the instrument has high explanatory power, as we would expect for emigration—and immigration—patterns exhibiting substantial persistence. Minor changes arise in the first stage when comparing results for the two different baseline years considered, 1895 and 1900. An advantage of picking T less than 1906 is that we wash out variation induced by the Messina-Reggio Calabria earthquake (Spitzer *et al.*, 2020). Tables IX and X compare results from the OLS estimation and from the second stage of our IV regression for different outcome variables, specifically, measures for capital investment, industrialization, urbanization, and population growth. No major differences arise between the two estimations. However, IV regression on population growth yields slightly higher estimates: downward bias in the OLS could arise if the conditional identifying variation was regionally clustered within the South. This however affects neither the sign nor the significance of the results.

5.3 Railway-Access Instrumental Variable

Several recent papers call for caution on the use of Bartik instrumental variables (Goldsmith-Pinkham *et al.*, 2020; Jaeger *et al.*, 2018). In our context, the proposed shift-share IV suffers from endogeneity issues if the initial spatial variation of migration patterns was correlated with economic development at baseline. To address this concern, we develop an IV based on the timing when Italian districts became connected to the railway network, similarly to Sequeira *et al.* (2020). In general, gaining access to the railway system in this period drastically reduced transportation costs for potential emigrants, hence increasing the total migration outflow. On top of this, the rationale behind our instrument is that transoceanic migration required a district to be connected to an emigration port.[46] Specifically, because U.S.-bound emigrants could leave only from Genoa, Naples, or Palermo, we leverage variation in the timing when districts became connected to one of these ports to instrument actual U.S.-bound migration outflows.

Let $\text{RA}_{cr,t}$ denote an indicator variable that returns the value one if district c in region r is connected to the railway system in decade t, and zero otherwise. We define railway access to emigration ports $\text{RAP}_{cr,t}$ as follows:

$$\text{RAP}_{cr,t} \equiv \text{RA}_{cr,t} \times \min\left\{d_t(c,\text{Naples}), d_t(c,\text{Palermo}), d_t(c,\text{Genoa})\right\}^{-1} \tag{5}$$

where $d_t(c,i)$ is the geodesic distance over the railway network in decade t between district c and emigration port i.[47] Because the network evolves over time, we allow the geodesic distance between each

[46] As Calabrese (2017, pp.52, 90) puts it:

> "The lack of railroads contributed to the isolation. [...] It was only between 1880 and 1900 that over 1,250 miles of railroad were constructed in region [Basilicata], making it more accessible for travel and facilitating emigration. [...] From Potenza and towns in the western part of Basilicata, migrants could travel to Naples by railroad. The building up of infrastructure in Basilicata aided emigrants in traveling to their port of departure."

[47] In graph theory, the geodesic distance is defined as the shortest path between two nodes. More formally, let the railway system in decade t—call it $\mathcal{N}_t$—be defined as the pair (V, E), where V is the set of nodes, and $E = \{(u,v)|(u,v) \in V^2,\, u \neq v\}$ is

district and the closest emigration port to reflect this time variation. A natural test of the hypothesized role of the railway system in shaping the direction of emigration would be to observe a positive correlation between our measured access $\text{RAP}_{cr,t}$ and the relative share of emigrants headed toward the United States.[48] Evidence presented in the next paragraph confirms this.

Following Sequeira *et al.* (2020), we estimate the following "zero-stage" model:

$$\begin{aligned}\text{US Emigrant Share}_{cr,t} &= \alpha_c + \alpha_{r,t} + \beta\text{US Emigrant Share}_{cr,t-1} + \gamma\text{RAP}_{cr,t-1} \\ &\quad + \delta\left(\text{Industrialization}_{r,t-1} \times \text{RAP}_{cr,t-1}\right) + \zeta\left(\text{RAP}_{cr,t-1} \times \text{ Emigrants}_{r,t-1}\right) \\ &\quad + \boldsymbol{x}'_{cr,t}\boldsymbol{\eta} + \varepsilon_{cs,t}\end{aligned} \tag{6}$$

where t denotes decades spanning the 1890-1920 period; α_c and $\alpha_{r,t}$ denote district and region-by-year fixed effects; $\text{Emigrants}_{r,t-1}$ is the total number of emigrants leaving region r, where $c \in r$, during decade t, normalized by the total population in that region in 1881; $\text{Industrialization}_{r,t-1}$ is the share of workers employed in manufacturing in region r,[49] and $\boldsymbol{x}_{cr,t}$ is a set of controls consisting of lagged population, a South dummy interacted with lagged railway access, and labor-market slackness. The outcome of interest, $\text{US Emigrant Share}_{cr,t}$, is the share of U.S. emigrants from district c in region r in decade t over district c's population in 1881, and $\text{US Emigrant Share}_{cr,t-1}$ is its lagged value. Our main coefficient of interest is ζ. This captures how changes in railway closeness to emigration ports influenced U.S.-bound emigration during periods of high *vis-à-vis* low overall aggregate emigration, accounting for the district population in 1881, i.e., before the mass emigration began. We thus expect the estimate of ζ to be positive. In turn, we expect the estimate of γ to be close to zero, because it reflects how railway access affected U.S. emigration in decades with little overall emigration. The estimated coefficients of regression (6) confirm these predictions (for the sake of brevity, we do not report them). One may suspect that the construction of the railway was not random across districts, because more-affluent areas were connected before poorer ones, so we include the interaction between the share of industrial workers and railway access as one further control.

The estimation equation (6) yields a set of estimated coefficients that allow us to construct a predicted aggregate series of the share of U.S. emigrants, which we then aggregate up across decades

the set of edges. Let $\mathbf{A}$ denote the adjacency matrix associated to E, where for every couple of vertices $v, u \in V$, $A_{uv} = 1$ if there is an edge between u and v, and zero otherwise. The (geodesic) distance $d(u, v)$ between the two vertices is the minimum r such that $[\mathbf{A}^r]_{uv} = 1$ (Newman, 2018).

[48] Clearly, emigration toward South America would have equally benefited from railway connection to emigration ports. However, U.S.-bound emigrants easily outnumbered emigrants bound for South America in this period.

[49] Controlling for the share of workers employed in manufacturing serves a twofold purpose. On one hand, it washes out variation in U.S. emigration due to more affluent districts being granted access to the railway system relatively sooner than backward ones (Sequeira *et al.*, 2020). Second, the timing of connection to the railway may itself affect economic development, for instance through increased specialization and industrialization (*i.a.* Donaldson, 2018; Donaldson & Hornbeck, 2016). This would generate endogenous variation, which we wash out when constructing the instrument.

as follows:

$$\widehat{\text{QE}}_{cr} \equiv \sum_{t=1890}^{1920} \hat{\zeta} \left(\text{RAP}_{cr,t-1} \times \text{Emigrants}_{r,t-1}\right) \tag{7}$$

We instrument quota exposure with $\widehat{\text{QE}}_{cr}$, then we estimate the resulting instrumented DiD model in a standard two-stage-least-squares setting.

Table B.12 reports the results of the first-stage regressions. The "RA region" column reports the results of the baseline instrument, whereas the "RA total" column uses a variation on equation (6) where, instead of the aggregate number of emigrants in the region, we plug in the overall nationwide number of emigrants. We find that there is a strong and positive association between the synthetic and the actual series of U.S.-bound emigrants. Although the F statistics using the railway instrument are not as high as those of the Bartik IVs, these nonetheless provide evidence suggesting that the instrument is not weak. Tables IX and X compare the second-stage results with the OLS estimates for, respectively, technology adoption and population and employment variables. The railway IV always confirms the baseline estimates in sign and magnitude and, in most cases, preserves their significance.

6 Conclusion

In recent years, immigration has become an increasingly focal and polarizing theme in the public debate. Policymakers exhibit widely divergent opinions about the effects of increased immigration on the economic, social, and cultural security of native populations. Yet, a common perspective can be disentangled. Both proponents and opponents of harsher immigration-restriction policies judge them in terms of their effects on their own country, that is the country *subject to* immigration. Few mention, possibly due to relatively scarce evidence, that immigration policies may entail important, even determinant, effects on sending countries. This asymmetric attention in favor of receiving countries is worrisome, given that sending nations often experience greater economic hardship and social distress.

In this study, we explore how restrictive immigration policies shape economic development in sending countries. This poses two empirical challenges. First, emigration is seldom directed toward one—or very few—countries, hence it is difficult to identify the effect of a single immigration policy shift in one such receiving country. Second, migration dynamics are likely affected by preexisting regulations enacted by both receiving and sending countries. To tackle both issues, we study the Italian emigration to the United States during the Age of Mass Migration (1850–1914). Through the 1921 and 1924 Quota Acts, the United States adopted a harshly restrictive immigration policy, which starkly contrasted with the open-border approach that it had maintained almost uninterruptedly since the 1810s. 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.

We find that industrial firms in more-exposed districts underwent sizable reductions in capital investment and a slowdown in technology adoption. These effects are larger for more advanced capital vintages and in relatively backward manufacturing sectors. To rationalize these findings, we advance and validate the hypothesis that IRPs induce a positive labor supply shock on countries sending migrants. Through the lenses of directed technical change and adoption theory, more-abundant labor dampens the incentives for firms to invest in labor-saving, possibly productivity-enhancing, production technologies (e.g., Zeira, 1998; Acemoglu, 2007). We document that population growth increased in comparatively more-treated districts, consistent with the idea that the Quota Acts prevented people who would have migrated from doing so. Our empirical results endorse the directed technical adoption mechanism—we observe that in highly exposed districts, industrial employment increased while agricultural employment did not. Shifting our analysis to manufacturing sectors, we find that sectors where capital investment decreased the most were also the ones that absorbed the bulk of the labor supply shock induced by the Quota Acts. This is consistent with the idea that firms in relatively backward industrial sectors substituted capital-intensive production technologies with labor, which the IRP shock made more abundant (and cheaper).

Taken together our results indicate that immigration restriction policies exert substantial effects on the economic development of sending countries. An immigration restriction shock impresses upward pressure on the labor supply driven by foregone migrants in the sending country. In our setting, this dampened the incentive for manufacturing firms to adopt productivity-enhancing technology. Faced with more abundant labor, firms substituted capital with more labor-intensive production technologies. Because technology adoption is a well-known driver of long-run growth (Juhász *et al.*, 2022), evidence in this paper suggests that immigration restriction policies have potentially long-lasting effects on the economic development of sending countries. The external validity of these findings is not obvious. However, we argue that neither the Italian economy nor emigrants' characteristics during the 1920s were fundamentally different from many of today's developing countries. Hence, we believe history can inform the contemporary debate on this crucial issue.

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Tables

Table I: Summary Statistics

	N. of Obs.	Mean	Std. Dev.	10 pct.	50 pct.	90 pct.
Panel A: Demographics and Geography						
Area	1070	121.08	77.12	45.93	98.31	240.66
Altitude	1070	0.33	0.22	0.07	0.31	0.63
Population	1066	165.25	156.88	53.37	122.36	319.56
5-Urbanization	1066	0.60	0.26	0.25	0.59	0.95
10-Urbanization	1066	0.37	0.27	0.00	0.31	0.80
15-Urbanization	1066	0.28	0.26	0.00	0.24	0.63
Panel B: Emigration						
Emigration (1890-1930)	1080	284.82	266.57	57.82	238.57	496.41
Emigration (1890-1921)	1080	259.69	241.90	52.57	212.73	453.95
Emigration (1890-1914)	1080	230.64	226.87	42.55	185.68	389.12
US Emigration (1890-1930)	1080	73.20	81.88	7.41	43.51	164.73
US Emigration (1890-1921)	1080	67.26	74.89	6.88	40.40	152.31
US Emigration (1890-1914)	1080	57.71	64.79	5.66	36.08	130.94
Panel C: Employment						
Agriculture Workers	1062	42.70	26.99	16.23	37.45	75.12
Manufacture Workers	1069	21.54	32.80	3.97	11.74	45.64
Trade Workers	1070	5.78	9.93	1.09	2.95	10.88
Liberal Professions	1062	2.48	4.46	0.38	1.28	4.66
Public Administration	1062	3.88	7.86	0.59	1.84	7.34
Panel D: Capital and Technology						
Firms	1061	8278.04	9725.53	587.70	5262.13	19054.43
Firms with Engine	1061	1336.61	2032.29	137.06	679.12	3038.19
Mechanical Engines	1061	816.69	672.69	250.94	554.21	1782.77
Electrical Engines	1061	6051.59	21620.63	84.29	1055.33	12809.84
Mechanical Horsepower	1061	96168.86	163951.19	6021.24	26237.85	310569.50
Electrical Horsepower	1061	53083.77	142887.45	660.49	9552.07	134462.30

Notes. This table reports symmary statistics for the variables in our dataset, except sector-specific capital and employment. All variables are in levels. Area, altitude, population, employment, and emigration are expressed in thousands. Section 3 explains how we impute province-level data to districts, and provides details on the sources employed.

TABLE II: BALANCE TABLE

	Level		Growth Rate	
	1911	1921	1911	1921
All Firms	0.017	0.007	0.029	-0.022
	(0.019)	(0.027)	(0.032)	(0.032)
Firms with Engine	0.033	0.027	0.048	-0.012
	(0.036)	(0.066)	(0.087)	(0.110)
Mechanical Engines	0.005	-0.016	0.089	-0.168
	(0.072)	(0.088)	(0.177)	(0.202)
Electrical Engines	0.005	0.004	0.005	-0.001
	(0.010)	(0.009)	(0.020)	(0.022)
Mechanical Horsepower	-0.038	-0.021	-0.095	0.056
	(0.029)	(0.051)	(0.078)	(0.098)
Electrical Horsepower	-0.007	-0.012	-0.004	-0.026
	(0.026)	(0.039)	(0.053)	(0.070)
Population	-0.000	0.000^{**}	-0.037	0.106
	(0.000)	(0.000)	(0.166)	(0.193)
Manufacture Workers	0.005	0.007	-0.028	0.017
	(0.103)	(0.094)	(0.101)	(0.075)
Agriculture Workers	0.031	0.006	-0.144	-0.048
	(0.096)	(0.125)	(0.153)	(0.127)
Trade Workers	-0.050	-0.032	-0.151	0.099
	(0.092)	(0.094)	(0.133)	(0.075)
Liberal Professions	-0.017	0.006	0.005	0.120
	(0.114)	(0.070)	(0.113)	(0.230)
Public Administration	0.065	0.088	0.027	0.036
	(0.128)	(0.204)	(0.105)	(0.129)

Notes. This table reports the correlation between the treatment measure (QE) and the covariates we use as outcome variables, before the Quota Acts were enacted. Quota exposure is defined as the ratio between US emigrants 1890-1924 and 1880-population. All regressions control for the emigration rate, defined as the ratio between emigrants 1890-1914 and 1880-population, and province fixed effects. Standard errors are clustered at the district level. In the first two columns, the outcome variable is in level; in the last two columns, it is defined in growth rate. Dependent variables are standardized to compare coefficients across models. Under validity of the parallel trends assumption, we require all coefficients not to be statistically different from zero.

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

Table III: Investment in capital goods and emigration

	Firm		Engine		Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Quota Exposure × Post	0.128	0.299	-1.015***	-1.098***	-0.613**	-1.268***
	(0.235)	(0.401)	(0.162)	(0.327)	(0.308)	(0.294)
Extensive Margin × Post	-0.093	0.017	0.184*	0.138	-0.271**	0.010
	(0.108)	(0.186)	(0.101)	(0.130)	(0.114)	(0.148)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	207	208	208	207	209	208
Observations	783	785	785	783	787	785
R^2	0.457	0.737	0.844	0.473	0.663	0.841
F-stat	0.772	0.250	12.756	5.578	3.095	7.101
Mean Dep. Var.	0.139	0.128	0.107	0.248	0.020	0.187
Std. Beta Coef.	0.027	0.030	-0.181	-0.187	-0.059	-0.128

Notes. This table displays the effect of being exposed to the Quota Acts on various measures for capital investment and technology adoption. The first and second columns report the effect on, respectively, the number of all firms, and firms with engines. The third and fourth columns show the effect on the number of mechanical and electrical engines; the fifth and sixth display the effect on mechanical and electrical horsepower. All regressions include district and year fixed effects. Additional controls are the log-population and labor market slackness at baseline interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.

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

TABLE IV: LABOR INTENSITY AND EMIGRATION

	Worker/Firm		Worker/Engine		Worker/Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Quota Exposure × Post	0.208	0.184	1.135***	1.050***	0.248	1.212***
	(0.239)	(0.396)	(0.174)	(0.339)	(0.353)	(0.300)
Extensive Margin × Post	0.051	-0.072	-0.235**	-0.114	0.421***	0.005
	(0.142)	(0.162)	(0.103)	(0.132)	(0.125)	(0.150)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	208	207	209	208
Observations	785	787	785	783	786	785
R^2	0.522	0.725	0.837	0.456	0.642	0.836
F-stat	6.364	7.630	23.588	3.584	7.179	5.482
Mean Dep. Var.	-0.082	-0.054	-0.077	-0.258	-0.078	-0.195
Std. Beta Coef.	0.036	0.017	0.188	0.180	0.023	0.123

Notes. This table displays the effect of being exposed to the Quota Acts on various measures for labor intensity in production. The first and second columns report the effect on, respectively, the worker-per-firm and the worker-per-firm with engine ratios. The third and fourth columns show the effect on the ratio between worker and mechanical and electrical engines; the fifth and sixth display the effect the ratio between workers and mechanical and electrical horsepower. All regressions include district and year fixed effects. Additional controls are the log-population and labor market slackness at baseline interacted with a post-treatment dummy. Outcome variables are defined in growth rate. District fixed effects refer to 1921-*circondari*. Standard errors are always robust and clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.

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

TABLE V: POPULATION GROWTH AND EMIGRATION

	Continuous QE		Categorical QE	
	(1)	(2)	(3)	(4)
Quota Exposure × Post	0.409***	0.449***		
	(0.113)	(0.124)		
Quota Exposure Dummy × Post			0.021***	0.023***
			(0.006)	(0.007)
Extensive Margin × Post		-0.068		-0.051
		(0.055)		(0.053)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Districts	204	204	204	204
Observations	751	751	751	751
R^2	0.452	0.452	0.445	0.445
F-stat	13.337	9.932	13.298	10.086
Mean Dep. Var.	1.042	1.042	1.042	1.042
Std. Beta Coef.	0.219	0.240	0.194	0.210

Notes. This table displays the effect of exposure to the Quota Acts on population growth. Population growth is defined as the decade-on-decade percentage change in population. Continuous QE is the baseline measure defined in (1); Categorical QE equals one if the continuous measure is above 1, and 0 otherwise. All regressions control for log-population and labor market slackness in 1901, interacted with a post-treatment measure. Models in columns (2) and (4) include the emigration rate defined as the number of emigrants 1890-1914 over the 1880-population, interacted with a post-treatment dummy. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE VI: EMPLOYMENT IN INDUSTRY AND AGRICULTURE

	Industry Growth		Agriculture Growth	
	(1)	(2)	(3)	(4)
Quota Exposure × Post	1.827***	1.510***	-0.416*	-0.483*
	(0.427)	(0.475)	(0.159)	(0.176)
Extensive Margin × Post		0.637		0.154
		(0.400)		(0.149)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Districts	205	205	206	206
Observations	742	742	750	750
R^2	0.540	0.542	0.461	0.465
F-stat	6.805	7.004	3.556	3.250
Mean Dep. Var.	0.060	0.060	-0.041	-0.041
Std. Beta Coef.	0.149	0.123	-0.116	-0.135

Notes. This table reports the effect of exposure to the Quota Acts on industrial and agricultural employment growth. Sector employment growth are defines as the decade-on-decade changes in employment. All regressions include district and year fixed effects. Further controls include log-population and labor market slackness in 1901 interacted with a post-treatment dummy. Columns (3) and (4) control for the emigration rate, defined as the number of emigrants 1890-1914 relative to 1880-population, interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are robust and clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.

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

Table VII: Urbanization and Share of Workers Employed in Industry and Agriculture

	Industrialization		Agriculture		Urbanization	
	(1)	(2)	(3)	(4)	(5)	(6)
Quota Exposure × Post	1.457***	1.152***	-0.580***	-0.605***	0.218	0.252*
	(0.356)	(0.410)	(0.145)	(0.156)	(0.145)	(0.148)
Extensive Margin × Post		0.598*		0.066		-0.086
		(0.350)		(0.085)		(0.099)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	205	205	204	204	201	201
Observations	729	729	743	743	742	742
R^2	0.476	0.478	0.510	0.510	0.174	0.174
F-stat	6.085	6.494	5.470	4.049	1.125	1.025
Mean Dep. Var.	0.051	0.051	-0.022	-0.022	1.039	1.039
Std. Beta Coef.	0.153	0.121	-0.172	-0.180	0.094	0.109

Notes. This table reports the effect of exposure to the Quota Acts on urbanization and changes in the share of industrial and agricultural workers relative to overall employment. Urbanization is defined as the share of the population living in cities no smaller than 5,000 inhabitants. The share of sector employment is defined as the ratio between sector and aggregate employment. All regressions include district and year fixed effects. Further controls are log-population and labor market slackness in 1901 interacted with a post-treatment dummy. Columns (2), (4) and (6) control for the emigration rate, defined as the number of emigrants 1890-1914 relative to 1880-population, interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are robust and clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.

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

Table VIII: Changes in Industry Employment by Sector

	Mining	Agriculture	Steel	Construction	Textile	Chemical
Quota Exposure $\times$ Post	0.442	-2.459*	1.379	6.103***	5.651***	0.017
	(0.388)	(1.261)	(1.573)	(1.626)	(1.398)	(0.308)
Extensive Margin $\times$ Post	-0.000	1.029	-1.124	-2.693**	-0.715	0.181
	(0.287)	(1.257)	(1.576)	(1.293)	(0.991)	(0.277)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	194	200	198	200	200	195
Observations	685	776	775	778	774	681
R^2	0.071	0.424	0.106	0.317	0.449	0.450
F-stat	8.152	5.645	5.030	16.662	4.555	1.849
Mean Dep. Var.	0.724	0.422	0.250	0.553	0.291	0.751
Std. Beta Coef.	0.008	-0.134	0.096	0.180	0.124	0.000

Notes. This table displays the effect of exposure to the Quota Acts on changes in employment by manufacture sector. Hence, column "Agriculture" reports the impact of QE on employment in manufacture firms working in agriculture, not that on agriculture. We do not show the "public utility" sector due to data availability, and a residual sector of unassigned firms. All regressions include district and year fixed effects. Further controls are log-population, changes in industrial employment, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.

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

TABLE IX: INVESTMENT IN CAPITAL GOODS AND EMIGRATION - 2SLS

	Firm		Engine		Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Panel A: OLS						
Quota Exposure × Post	0.128	0.299	-1.015***	-1.098***	-0.613**	-1.268***
	(0.235)	(0.401)	(0.162)	(0.327)	(0.308)	(0.294)
Panel B: 2SLS Shift Share						
Quota Exposure × Post	0.429*	0.661*	-0.850***	-0.857***	-0.568*	-1.098***
	(0.233)	(0.398)	(0.164)	(0.318)	(0.329)	(0.287)
Panel C: 2SLS Railway Regional						
Quota Exposure × Post	0.603	-0.822	-0.895**	-1.472*	-0.178	-1.097**
	(0.454)	(1.272)	(0.374)	(0.868)	(0.955)	(0.552)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	207	208	208	207	209	208
Observations	783	785	785	783	787	785
Mean Dep. Var.	0.139	0.128	0.107	0.248	0.020	0.187
Std. Beta OLS	0.027	0.030	-0.181	-0.187	-0.059	-0.128
Std. Beta SS	0.090	0.067	-0.152	-0.145	-0.055	-0.111
Std. Beta RA	0.060	0.077	-0.170	-0.140	-0.073	-0.138

Notes. This table reports the effect of Quota exposure on various measures of capital investment and technology adoption. Panel A presents reduced form estimates. Panel B reports 2SLS estimates based on the instrument defined in (4). Panel C reports 2SLS estimates based on te instrument defined in (6). The first and second columns report the effect on, respectively,the number of all firms, and firms with engines. The third and fourth columns show the effect on the number of mechanical and electrical engines; the fifth and sixth display the effect on mechanical and electrical horsepower. All regressions include district and year fixed effects. Additional controls are log-population and labor market slackness at baseline interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.
*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

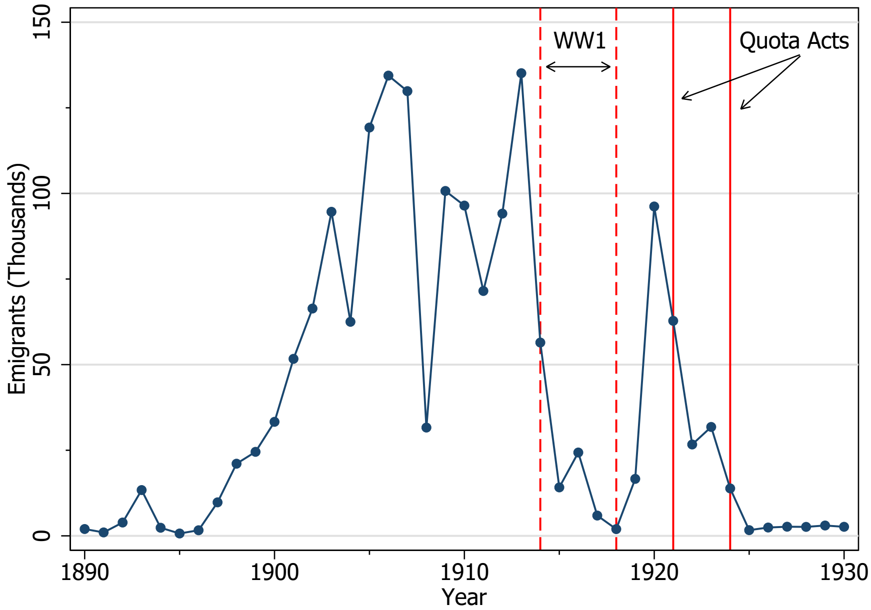
Table X: Population Growth, Employment in Industry and Agriculture

	Population Growth	Industry Growth	Agriculture Growth
Panel A: OLS			
Quota Exposure × Post	0.449***	1.510***	-0.483*
	(0.124)	(0.475)	(0.176)
Panel B: 2SLS Shift Share			
Quota Exposure × Post	0.668***	1.673***	-0.138
	(0.138)	(0.544)	(0.222)
Panel C: 2SLS Railway Regional			
Quota Exposure × Post	0.933***	3.385**	-0.733
	(0.248)	(1.347)	(0.479)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Districts	207	205	209
Observations	754	742	753
Mean Dep. Var.	0.042	0.060	-0.041
Std. Beta Coef. OLS	0.240	0.123	-0.135
Std. Beta Coef. Shift-Share	0.360	0.137	-0.038
Std. Beta Coef. Railway	0.503	0.276	-0.203

Notes. This table reports the effect of exposure to the Quota Acts on industrial and agricultural employment growth. Sector employment growth are defines as the decade-on-decade changes in employment. Panel A presents reduced form estimates. Panel B reports 2SLS estimates based on the instrument defined in (4). Panel C reports 2SLS estimates based on te instrument defined in (6). All regressions include district and year fixed effects. Additional controls are log-population and labor market slackness at baseline interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Standardized betas refer to the baseline Q.E. coefficient.
*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

Figures

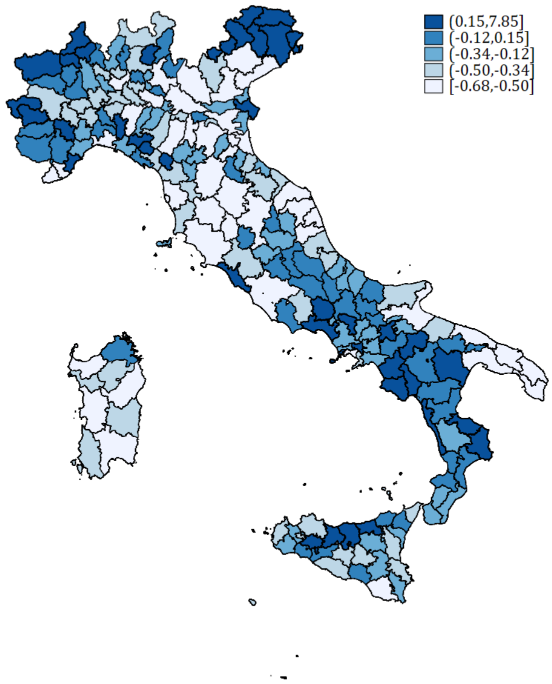
FIGURE I: TOTAL INFLOW OF ITALIAN IMMIGRANTS AT ELLIS ISLAND



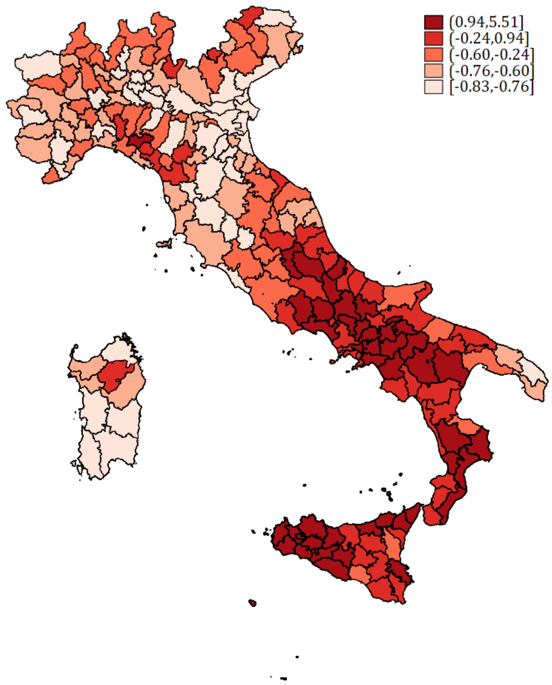
Notes. This figure displays the aggregate number of Italians who registered at the Ellis Island immigration station between 1890-1930. Dashed red lines indicate the period of WW1; solid red lines indicate the 1921 Emergency Quota Act and the 1924 (Johnson-Reed) Immigration Act. Only migrants whose origin we are able to trace are counted in the sum. Refer to the Online Appendix for details on the linking procedure.

Figure II: District-Level Migration Flows, 1890-1924

(a) Emigration Rate

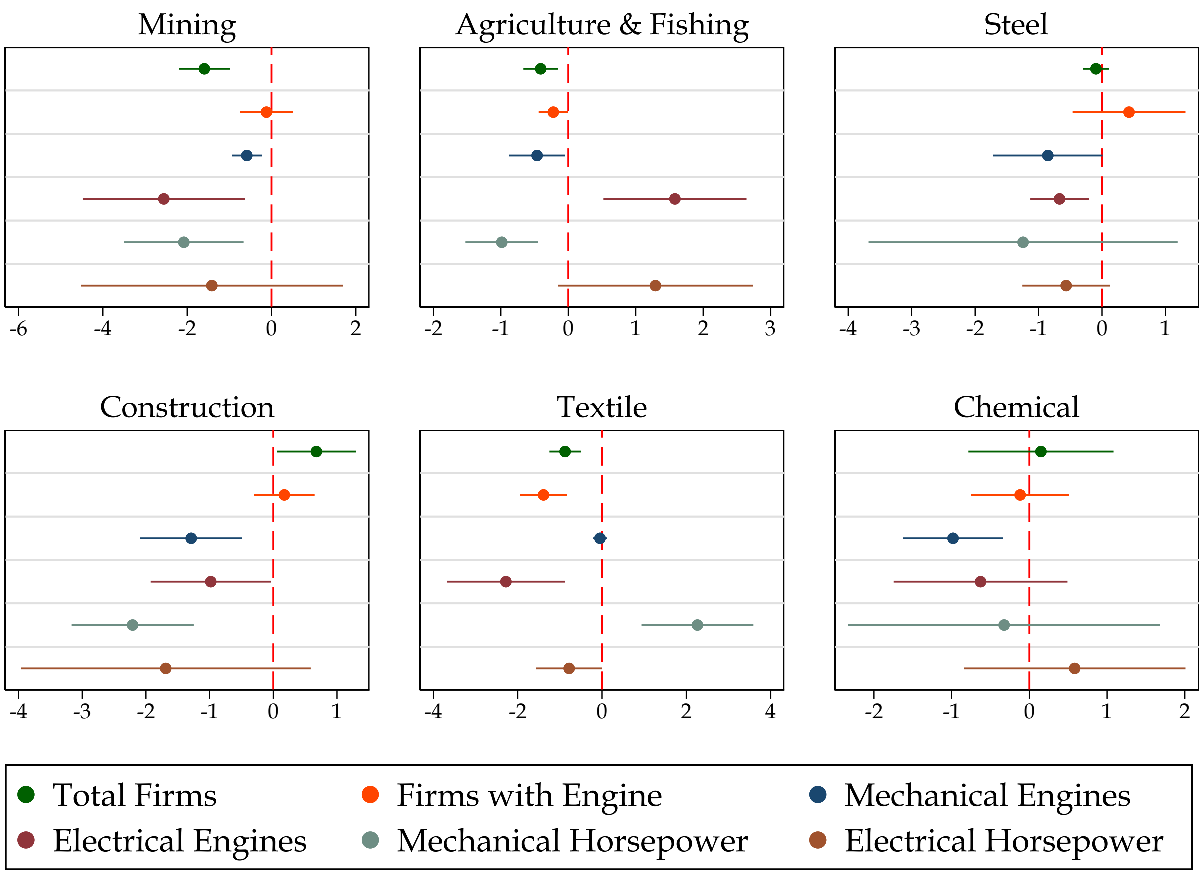


(b) Quota Exposure



Notes. Panel (a) displays variation in the emigrants-to-population ratio (emigration rate). Panel (b) plots the unconditional variation in the US emigrants-to-population ratio (quota exposure). Both figures normalize the number of emigrants by population in 1880, and report standardized variables in log. All figures plot the flows obtained setting $\alpha = .01$ in the matching process. Refer to the Online Appendix for more details and plots for different values of α.

Figure III: Capital Investment and Emigration by Industry Sectors



Notes. This figure displays the effect effect exposure to the Quota Acts on capital investment and technology adoption by manufacture sectors. Each marker reports the estimated coefficient in model (3) where the outcome is the row-variable. Outcomes are the raw count of firms and firms with engines; the number of electrical and electrical engines; mechanical and electrical horsepower. All regressions include district and year fixed effects. Further controls are log-population, average industrial employment growth, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Standard Errors are clustered at the district level. Bands report the 95% confidence intervals.

Online Appendix

The Economic Effects of Immigration Restriction Policies

Evidence from the Italian Mass Migration to the U.S.

Davide M. Coluccia, Lorenzo Spadavecchia

April, 2023

A Data Appendix

A.1 Emigration Data

In this section we document in detail the novel emigration data that we collect. The raw data can be found at https://heritage.statueofliberty.org/. First, we describe how we deal with spelling mistakes occurring the recorded municipality of origin of immigrants. Second, we report the share of emigrants with missing origin municipality. Last, we provide evidence suggesting that our data squares well with less granular data from official statistics records.

A.1.1 Emigration Matching Procedure

This section describes the procedure we follow to match municipalities recorded by Ellis Island US officials to actual Italian *comuni*. Since municipalities changed over time, we first assembled a list of all municipalities that existed between 1890 and 1930 from listed census names. Then along the lines of Abramitzky *et al.* (2014), we run the following matching procedure:

1. Perform manual name cleaning, *e.g.* correcting systematic mistakes and recording shortcuts.
2. Standardize each recorded and actual municipality name using the NYSIIS algorithm trained on Italian phonetics (Atack & Bateman, 1992). This procedure ensures that phonetically identical municipality names have an exact match.
3. For each standardized recorded name which does not have a perfect match in the list of all municipality names, compute the dissimilarity matrix with all those names, according to some metric. Then, pick as a match the *comune* with the lowest dissimilarity.
4. If the distance between a recorded municipality and its best match is lower than some threshold value $\alpha \in [0, 1]$, accept the match. Otherwise, drop the observation.

We evaluate the distance between a recorded municipality name i an actual name j in terms of their Jaro-Winkler similarity d_{ij}:

$$d_{ij} \equiv \widehat{d_{ij}} + \ell p(1 - \widehat{d_{ij}}) \tag{A.1}$$

where

$$\widehat{d_{ij}} \equiv \begin{cases} 0 & \text{if } m = 0 \\ \frac{1}{3}\left(\frac{m}{|i|} + \frac{m}{|j|} + \frac{m-t}{m}\right) & \text{else} \end{cases} \tag{A.2}$$

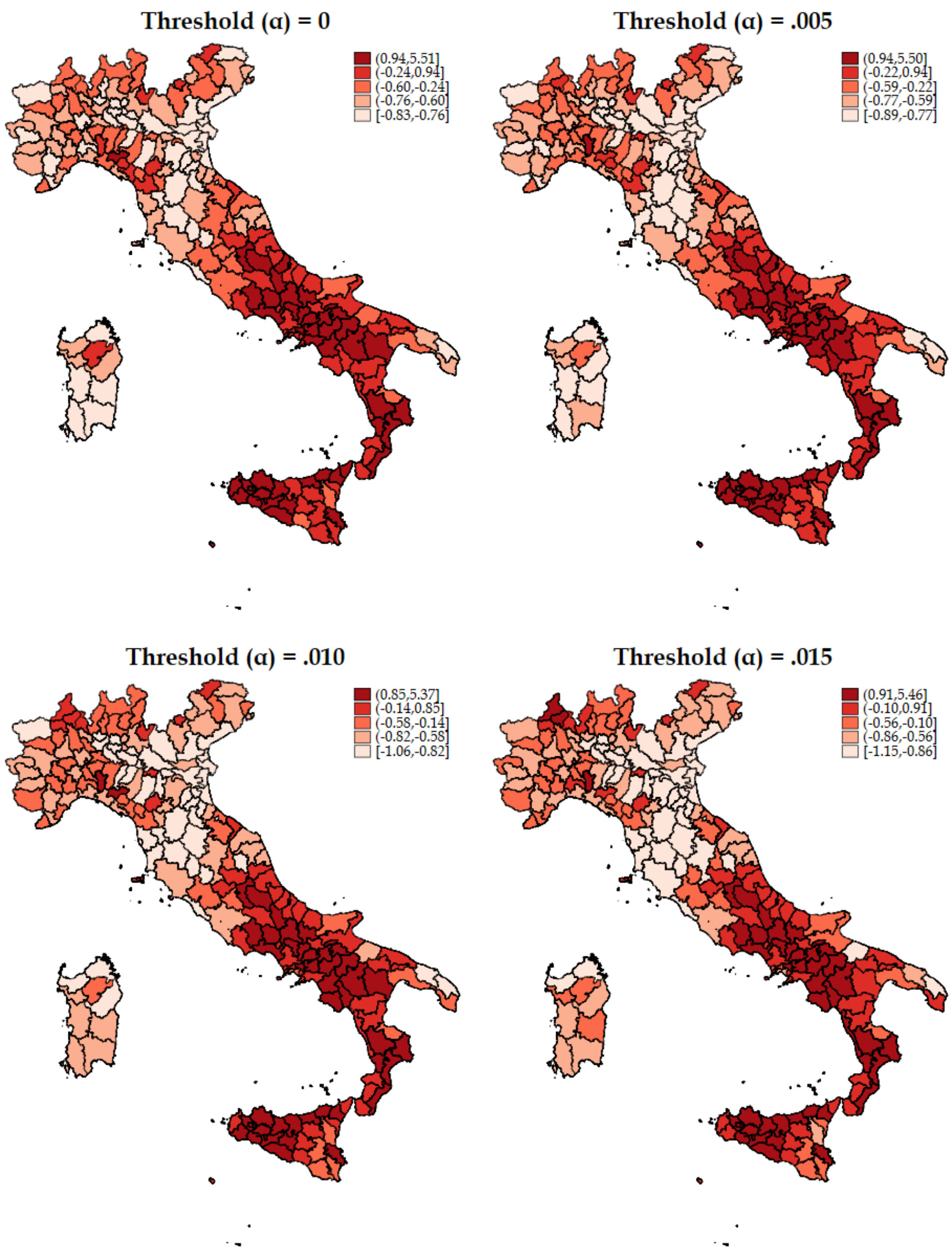
where m is the number of matching characters, $|i|$ is the length of string i, and t is half the number of transpositions, ℓ is the length of common an eventual common prefix no longer than four characters between i and j, and $p = 0.1$ is a constant scaling factor. Two characters are matching only if they are the same and are not farther than $\left\lfloor \frac{\max(|i|,|j|)}{2} \right\rfloor - 1$. Half the number of matching characters in different sequence order is the number of transpositions.[50]

[50]The Jaro-Winkler distance has been recently employed in the economic history literature for intergenerational linking purposes by, among others, Feigenbaum (2018) and Abramitzky *et al.* (2019).

The Jaro-Winker distance has been shown to perform relatively well in linking routines (Abramitzky *et al.*, 2019). In our particular case however, this metric outperforms more standard string dissimilarity metrics like the cosine or the Levenshtein because the Jaro-Winkler assigns a "bonus" score to strings starting with closer initial substrings. We noted that coding errors in municipality names are more frequent at the end of names, hence the comparative advantage of the Jaro-Winkler distance.

The matching procedure assigns to each recorded municipality name its best match among the actual names along with their distance d^*_{ij}. We set a threshold $\alpha \in [0, 1]$, pick all matches j with $d^*_{ij} \leq \alpha$, and drop the others.

FIGURE A.1: DISTRICT-LEVEL MIGRATION FLOWS VARYING α



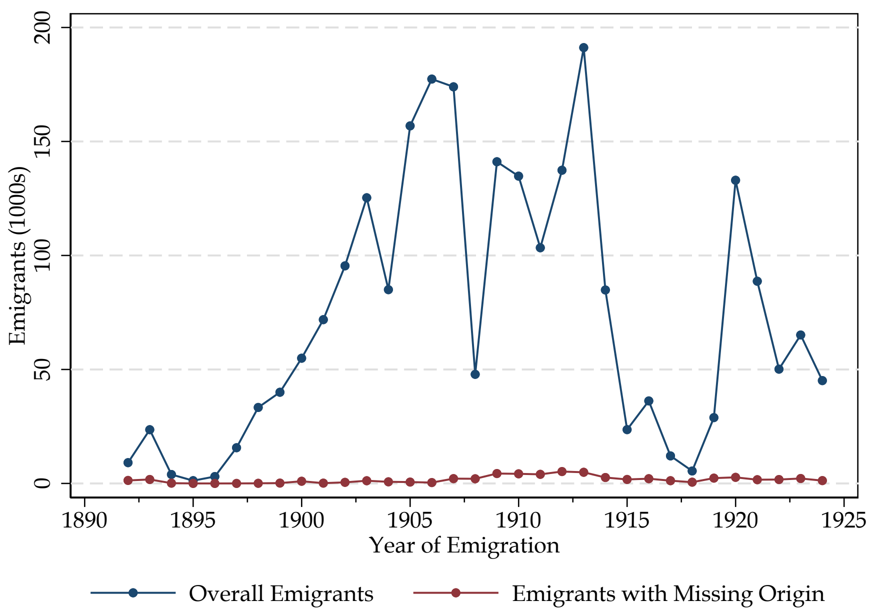
Notes. Each panel plots the number of emigrants across districts over the years 1890-1930. See Appendix A.1.1 for a complete description of the procedure and the meaning of α.

A.1.2 Missing Origin

The Ellis Island records report the origin municipality of Italian immigrants starting in 1892 when the immigration station opened. In picture A.2 we report the total number of recorded Italian immigrants at Ellis Island, along with those whose origin municipality is missing. We consider an origin entry as missing if it is either a proper missing or if the record reports coarse geographical aggregates. These include, among others, "Italy", Italian regions, and similar information which make it unfeasible to back out the district of origin of the immigrant. Since our analysis is run at the district level and the non-missing Ellis Island records report origin at the municipality level, to conduct our analysis we aggregate our individual-level data at the district level.

Figure A.2 suggests that missing origins are a minor concern in our dataset. There is no single year when the share of immigrants with missing origin exceeds 1% of the overall immigrants. Throughout our analysis, we therefore drop immigrants with missing origins from our dataset.

FIGURE A.2: ELLIS ISLAND IMMIGRATION RECORDS: ASSESSMENT OF MISSING ORIGIN



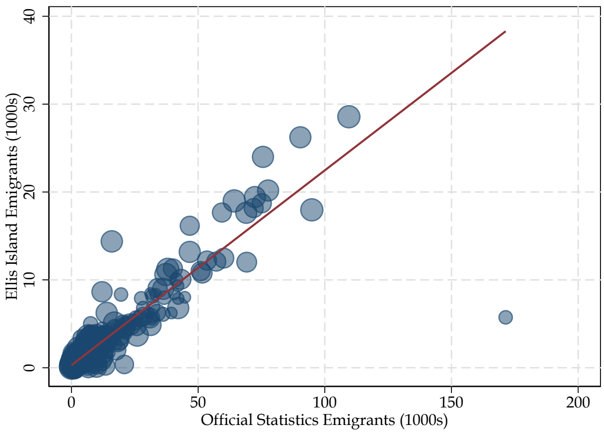
Notes. The blue series reports the total number of Italian immigrants in our sample, over the period 1892-1924. The red series reports the total number of Italian immigrants whose origin is missing in the Ellis Island dataset. We label as "missing" every entry whose origin is either missing, or reports coarse geographical aggregates, such as Italy, and Italian regions or provinces.

A.1.3 Validation of the Ellis Island Data

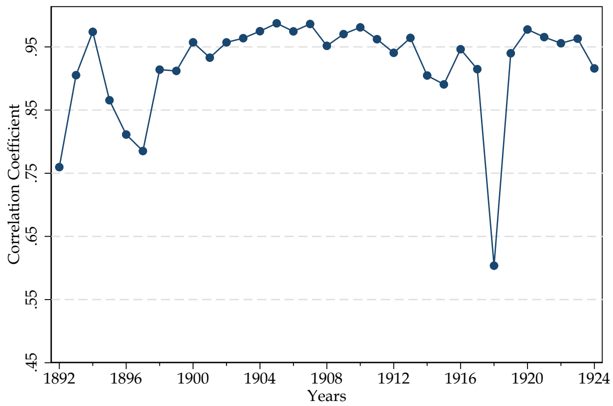
To validate our dataset, we compare it with official statistics data that we digitized from the *Annuario statistico dell'emigrazione italiana dal 1876 al 1925: con notizie sull'emigrazione negli anni 1869-1875*. The data was collected by the Commissioner-General for Emigration, and published by the Italian Statistical Office (ISTAT) in 1926. Data report yearly emigration outflows, broken down by major destination countries, and by region of origin of emigrants. There were 19 regions in Italy before WW1, and 20 thereafter. This implies that official statistics data cannot be used in our analysis, since regions are too few and large. Instead, we use these data as a meaningful validation tool for our dataset.

FIGURE A.3: VALIDATION OF THE ELLIS ISLAND EMIGRATION DATASET

(A) Cross-Sectional Correlation



(B) Time-Series Correlation



Note: Overall adjusted R^2 including region and year FEs: 0.925.

Notes. The left panel displays the cross-sectional correlation between region-level US emigration outflows as recorded in the official data—on the x-axis—and in our dataset—on the y-axis. A dot is a region-year, and the size of each dot is proportional to that region's population in the given decade. The right panel reports the R^2 of a regression where the dependent variable is US emigration as recorded in official statistics, and the explanatory variable is US emigration in our dataset. Each regression only considers observations for one given year. The note also reports the overall R^2 of the associated regression for the entire dataset, controlling for year and region-fixed effects.

In the left-hand panel of figure A.3 we report the cross-sectional correlation between US emigration outflows in our dataset—on the y-axis—and in official statistics—on the x-axis. Each observation is a region year, and the size of each dot is proportional to the population of the region in that year's decade, as registered in the population census. The red line reports the fitted values of the associated linear regression. The figure depicts a strong and positive association between the US emigration series in the two data sets. A similar picture would obtain if we bin-scattered observations. A possible caveat is that our dataset consists of fewer emigrants than reported in the official statistics because we searched 30,000 surnames in the Ellis Island Foundation dataset. Although comprehensive, this is not the universe of Italian surnames. This notwithstanding, figure A.3 attests that our dataset is geographically comparable to the official data.

Figure A.3 (B) reports the time-series correlation between the US emigration outflows series in the two data-sets. More specifically, each dot reports the R^2 of the following regression:

$$\text{Ellis Island US Emigration}_{r,t=T} = \alpha + \beta \text{OS US Emigration}_{r,t=T} + \varepsilon_{r,t=T} \qquad \text{(A.3)}$$

where Ellis Island US Emigration is the US emigration series measured with data from the Ellis Island database; OS US Emigration is the US emigration series measured with official statistics data; r denotes a region, and

$T \in [1892, 1924]$ is a given year. In other words, a dot in a given year T reports the cross-sectional R^2 of a regression including all observations—*i.e.*, one per region—in that year. In the footnote, we also report the R^2 associated with regression (A.3) where we pool observations across years and include year and region-fixed effects. The R^2 is a measure of linear fit between the two series. Hence, we would ideally observe $R^2 = 1$ under perfect collinearity. Results indicate that the correlation between the official series and ours is extremely high over time. Except for the WW1 years, the R^2 is above 75% throughout the sample period and, starting in 1896, always exceeds 90%. In two robustness checks, we confirm that our results remain unchanged even if we drop the periods with relatively low correlation, *i.e.* 1892-1896 and WW1.

The comparison between official statistics data and our series confirms that our measure is a valid proxy for actual US emigration. The main advantage of our dataset is its granularity, which we exploit in our analysis.

A.2 Data Sources

We here describe the sources from which we gathered the data needed for our analysis. Analyses are mainly conducted at the district level—aggregation areas comparable to US counties—which were named "Circondario" and are composed of municipalities (whose number ranges from 7900 to 9000 in our sample period). We collected and digitize district- or municipality-level data from multiple historical sources provided by the Italian Institute of Statistics. The main sources are the Population Censuses and Industrial Censuses. As explained in the previous Section, migration flows by municipality were taken from the Ellis Island database.

We here provide a detailed summary of the sources of our variables of interest for each year of our sample, specifying the geographical level at which data were collected. The historical volumes we digitized can be found at this link. Censuses were held on a 10-year basis. Population Censuses were comprehensive of all information on population, including occupation and alphabetization for the whole period 1901-1921. In 1931 the Census was smaller and did not include information on occupations. The next comprehensive Population Census was held in 1936. In order to fill the gap between the years 1921-1936, we had to take the information on occupation from the 1927 Industrial Census. This resulted in our sample of years for the population's occupations to be: 1901, 1911, 1921, 1927, 1936. As far as it concerns data on the number of firms, engines, and horsepower, they are available in the Industrial Censuses: information was available for the years 1911, 1927, and 1937.

Data on migration flows are gathered at the municipality level from the Ellis Island database, starting from the year 1881. Population at the municipality level was instead collected for all Population Censuses starting from 1861. For the years 1901, 1911, and 1921 data on population by occupation were available at the district level (about 200 units) on the Population Census. For the year 1927, it was instead available in the Industrial Census. In that same year, districts, or "Circondari", were suppressed as administrative units. This means that data on occupations for 1936 had to be collected at the municipality level, for a total of about 8000 municipalities.

Industrial data are from Industrial censuses. The Industrial census was conducted for the first time in 1911, and then again in 1927 and 1937. We digitized these censuses and collected all relevant variables at the province level, *i.e.* the most granular available level of aggregation. Since our analysis is conducted at the district level, we impute these from provinces to districts. In the next section, we explain the details of the imputation procedure.

Table A.1: Visual Summary of Data Sources

Variable	Measurement	Observation Unit	Source	Observed Years
Demographics				
Population	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Area	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Urbanization	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Literacy	Measured	Municipality	Population Censuses	1881-1936, excl.1891
Employment, by Sector				
Manufacture	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Agriculture	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Trade	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Liberal Professions	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Public Administration	Measured	District (1881-1921), Municipality (1931-1936)	Population Censuses	1881-1936, excl.1891
Capital & Industry				
Firms	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Firms with Engine	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Mechanical Engines	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Electrical Engines	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Mechanical Horsepower	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Electrical Horsepower	Imputed	Province, imputed to Districts	Manufacture Censuses	1911, 1927, 1937
Emigration				
US Emigration	Measured	Municipality	Ellis Island Data	1892-1924
Overall Emigration	Imputed	Province, imputed to Districts	Official Statistics of the Commissioner General	1877-1925
Other				
WW1 deaths	Measured	Municipality	Istituto per la storia della Resistenza e della società contemporanea.	1915-1918
Railways	Measured	Municipality	ISTAT – *Sviluppo delle ferrovie italiane dal 1839 al 31 dicembre 1926*	1839-1926

Notes. This table reports all variables used in the paper. The "Measurement" column reports "Measured" if the variable is used in the analysis as it is measured in the source data; instead, it reports "Imputed" if it is measured at a coarser level of aggregation, and is then imputed to districts. The imputation procedure is described in the Data Appendix. The "Observation Unit" returns the level of aggregation at which the variable is measured. The "Source" column displays the type of source the raw data are extracted from. Further references to original sources can be found in the text main body. The "Observed Years" column reports the years when the raw data is available. Literacy data are from Fontana *et al.* (2020).

A.2.1 Imputation of Industrial Census Data

The variables we use to proxy capital investment—namely, the number of firms, number of firms with engines, number of mechanical and/or electrical engines, and mechanical and/or electrical horsepower—are digitized from industrial censuses. The most granular level of aggregation available there are provinces. Provinces were composed of several districts, ranging from one to four. In our analysis, we impute these province-level data to districts. In this section, we describe the details of this imputation procedure.

Let subscript p denote a province-level variable, whereas the same variable with subscript d is at the district level. For every variable y_p we need to impute, we run the following simple OLS regression:

$$y_{p,t} = \alpha_p + \alpha_t + \boldsymbol{x}'_{p,t}\boldsymbol{\beta} + \varepsilon_{p,t} \tag{A.4}$$

where $t \in \{1911, 1927, 1937\}$, and α_t and α_p respectively denote year and province fixed effects. Term $\boldsymbol{x}_{p,t}$ includes a set of province-level regressors. These are total employment as well as the number of employed in agriculture, manufacturing, liberal professions, and public administration. Both y and the variables in $\boldsymbol{x}$ are in logs.

We estimate equation A.4 and retrieve a set of coefficients $\hat{\boldsymbol{\beta}}$. To perform the imputation, we exploit variation of the $\boldsymbol{x}$'s at the district level:

$$y_{d,t} = \boldsymbol{x}'_{d,t}\hat{\boldsymbol{\beta}} \tag{A.5}$$

Notice that, because all regressions include district and year fixed effects, these capture variation which in regressions (A.4) is absorbed by year and province fixed effects.

In table A.2 we compare province-level data from the industrial censuses and imputed variables computed through (A.5), aggregated at province level. The table suggests that there is a strong positive correlation between actual and imputed variables. This is confirmed by a formal test of the statistical significance of the correlation coefficients. These are statistically different from zero—and positive—for all imputed variables, thus suggesting that capital variables computed exploiting district-level variation in the $\boldsymbol{x}$'s correlate with actual province-level variables. We interpret this as evidence supporting our imputation procedure.

A.2.2 Railway data

Data on a district's historical connectivity to the railway network were constructed using information taken from the *Sviluppo delle ferrovie italiane dal 1839 al 31 dicembre 1926* edited by the Italian Statistical Office (*Ufficio Centrale di Statistica*) in 1927. To the best of our knowledge, this is the first paper to use these data. The Italian Statistical Office recorded the year of construction of each railway line connecting two municipalities, providing information on each intermediate station. Hence, we are able to construct the railway network for each year from 1839 to 1926.

As our analysis is carried out at the district level, we obtain a measure of railway access for each district c by aggregating municipality-level data. We build a time-varying dummy—$\text{RA}_{cr,t}$—taking value one if at least one municipality in a given district was connected through the railway to another municipality in a different

Table A.2: Comparison Between Actual and Imputed Capital Variables

	ρ	p-value	β	se(β)	R^2
Firms	0.439***	(0.000)	0.160***	(0.023)	0.193
Firms with Engine	0.470***	(0.000)	0.222***	(0.033)	0.221
Mechanical Engines	0.410***	(0.000)	0.105***	(0.022)	0.168
Electrical Engines	0.492***	(0.000)	0.247***	(0.036)	0.242
Mechanical Horsepower	0.469***	(0.000)	0.197***	(0.036)	0.220
Electrical Horsepower	0.468***	(0.000)	0.217***	(0.035)	0.219

Notes. This table compares measured and imputed capital variables. The imputation procedure is fully pinned down by equations (A.4)-(A.5). Each row compares the imputed and the measured row variable. The imputed row variable is predicted at the district level and then aggregated up to provinces. Column ρ reports Pearson's correlation coefficient between imputed and measured variables, along with its Bonferroni-adjusted p-value. Columns β and se(β) respectively display the coefficient and the standard error, clustered at the province level, of a regression where the dependent variable is imputed and the independent variable is measured. Column R^2 reports the coefficient of linear determination of this regression.

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

district, and zero otherwise. We also construct a measure of the capillarity of the presence of the railway in a given district using the number of train stations in that district for each year.

We build the network of districts connected through the railway in order to obtain the distance between each district c and any of the three departure ports: the districts of Genoa, Naples, and Palermo. Each district constitutes a node of the network. An edge is created between two nodes if at least one municipality of the first district is connected to one municipality of the second district. *De facto*, edges connect adjacent districts, as for each year there is no railway line directly connecting two municipalities in nonadjacent districts without stopping in a train station belonging to the intermediate district.

The distance between two adjacent districts is calculated as the geodesic distance between the centroids. The distance $d_t(c, i)$ between any two districts c and i in the network is hence the shortest path, or geodesic path, between the two nodes. We adopt this measure because we interpret the railway network as a weighted graph where edges are weighted by the distance between two nodes. In this context, the shortest path is the minimum sum of edge weights.

B Additional Tables & Results

Table B.1: Regional Emigration

Region	Emigrants to US					Emigrants to all destinations					Share
	76-87	88-99	00-12	13-25	Total	76-87	88-99	00-12	13-25	Total	
Piemonte	5.2	12.3	109.8	43.4	170.8	353.3	332.5	697.2	527.9	1910.8	8.9
Liguria	8.2	10.8	27.2	10.6	56.8	63.0	51.1	89.0	92.9	296.1	19.2
Lombardia	4.4	11.0	56.7	28.6	100.8	237.9	259.7	675.8	441.6	1615.2	6.2
Veneto	1.0	6.0	52.7	48.4	108.1	486.3	1197.6	1298.2	651.0	3633.1	3.0
Emilia-Romagna	1.3	8.4	62.0	24.0	95.8	60.5	137.7	422.4	178.7	799.2	12.0
Toscana	3.3	12.9	89.6	42.0	147.8	110.7	157.5	412.4	230.6	911.2	16.2
Marche	0.2	2.0	62.0	30.6	94.8	12.7	48.0	280.6	131.1	472.3	20.1
Umbria	0.1	0.5	24.1	11.8	36.6	0.5	6.0	129.9	59.4	195.7	18.7
Lazio	0.02	2.3	109.4	50.1	161.9	0.4	14.0	151.4	72.9	238.6	67.8
Abruzzi e Molise	26.9	68.0	371.0	161.6	627.4	58.3	164.1	585.7	241.6	1049.7	59.8
Campania	44.3	157.5	637.8	241.5	1081.2	131.3	339.6	871.0	360.7	1702485	63.5
Puglie	1.3	12.9	164.7	107.9	286.9	8.1	37.2	283.4	172.4	501.2	57.2
Basilicata	28.4	53.3	108.1	38.5	228.3	74.1	106.5	179.8	70.5	431.0	53.0
Calabrie	15.0	58.5	457.7	125.1	656.3	74.1	178.5	539.8	253.6	1046.1	62.7
Sicilia	12.6	117.2	687.7	356.1	1173.6	26.8	170.9	946.5	516.4	1660.6	70.7
Sardegna	0.01	0.03	8.5	5.7	14.2	1.3	6.2	72.8	43.9	124.1	11.5
Total	152.1	533.9	3029.1	1326.0	5041.3	1699.3	3206.9	7635.8	4045.4	16587.4	30.4

Notes. Regional emigration towards US and total emigration during the period 1876-1925. Figures are in thousands. Column "Share" indicates the percentage of total emigrants towards US relatively to all emigrants from that region in the whole period 1876-1925.
Source: our elaboration on data from the *Annuario statistico dell'emigrazione italiana dal 1876 al 1925: con notizie sull'emigrazione negli anni 1869-1875*, Italian Statistical Office (ISTAT), Roma, 1926.

Table B.2: Internal and International Migrations, 1921-1931

Region	Absolute numbers			Share over Population	
	Population	Internal Migrants	Emigrants	Internal Migrants	Emigrants
Abruzzo	1317.2	19.3	170.3	1.5	12.9
Basilicata	524.5	5.6	52.4	1.1	10.0
Calabria	1257.9	8.2	219.4	0.7	17.4
Campania	2896.6	1.2	248.4	0.0	8.6
Emilia Romagna	2183.4	78.7	165.3	3.6	7.6
Lazio	903.5	-133.8	88.2	-14.8	9.8
Liguria	892.4	-60.5	112.7	-6.8	12.6
Lombardia	3680.6	-198.0	460.6	-5.4	12.5
Marche	939.3	25.2	99.2	2.7	10.6
Piemonte	3070.3	-111.9	469.3	-3.6	15.3
Puglia	1589.1	52.9	117.8	3.3	7.4
Sardegna	682.0	2.8	27.7	0.4	4.1
Sicilia	2927.9	31.7	333.4	1.1	11.4
Toscana	2208.9	27.2	198.0	1.2	9.0
Umbria	572.1	-1.0	37.1	-0.2	6.5
Veneto	2814.2	139.8	639.8	5.0	22.7

Notes. This table reports internal migration and out-migration flows over the period 1921-1931. Column "Population" reports population in 1881. Column "Internal migrants" is the net internal migrant flow. To compute net internal migration flows, we take the difference in the outflow of people leaving a given region and the inflow of people arriving in that region during the decade 1921-1931. Since Census data only report the stock of people born in a given region living in another region in 1921 and 1931, to compute the outflow of people leaving a region during that decade, we take the difference across years of the total number of people born in that region and living in any other Italian region. Similarly, to compute the inflow of people arriving in a region during that decade we take the difference across years of the total number living in that region who were born in any other Italian region. Positive (negative) figures imply a net population loss (gain) due to internal migrations. Column "Emigrants" reports the number of international emigrants. Figures are in thousands. Columns "Share over Population" report net internal and international migration figures, relative to 1881-population. Figures are in percentage terms.
Source: our elaboration on data from the *Annuario statistico dell'emigrazione italiana dal 1876 al 1925: con notizie sull'emigrazione negli anni 1869-1875*, Italian Statistical Office (ISTAT), Roma, 1926, and from *Censimento della Popolazione Italiana*, Italian Statistical Office (ISTAT), Roma, 1921 and 1931.

Table B.3: Robustness Regressions - Changes in Mechanical and Electrical Engines

	Dep. Var.: Changes in Number of Mechanical Engines								Dep. Var.: Changes in Number of Electrical Engines							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Quota Exposure × Post	-0.983***	-1.030***	-1.025***	-0.996***	-0.897***	-0.927***	-0.884***	-0.810***	-0.986***	-1.040***	-1.125***	-1.002***	-1.095***	-1.216***	-1.000***	-0.856**
	(0.171)	(0.180)	(0.182)	(0.182)	(0.204)	(0.223)	(0.236)	(0.233)	(0.331)	(0.337)	(0.317)	(0.306)	(0.311)	(0.336)	(0.357)	(0.353)
Population	-0.096**	-0.077	-0.080	-0.081	-0.066	-0.062	-0.060	-0.052	-0.204**	-0.187**	-0.113	-0.114	-0.127	-0.110	-0.101	-0.085
	(0.048)	(0.048)	(0.050)	(0.050)	(0.051)	(0.052)	(0.053)	(0.052)	(0.090)	(0.092)	(0.088)	(0.088)	(0.088)	(0.086)	(0.085)	(0.085)
Extensive Margin × Post		0.122	0.126	0.108	0.119	0.122	0.110	0.112		0.132	0.046	-0.035	-0.049	-0.037	-0.101	-0.098
		(0.112)	(0.112)	(0.114)	(0.112)	(0.111)	(0.115)	(0.115)		(0.134)	(0.128)	(0.138)	(0.140)	(0.139)	(0.150)	(0.148)
Agriculture × Post			-0.017	-0.047	-0.023	-0.032	-0.035	-0.033			0.317***	0.205**	0.182*	0.149	0.131	0.136
			(0.039)	(0.050)	(0.064)	(0.066)	(0.066)	(0.066)			(0.065)	(0.089)	(0.098)	(0.105)	(0.100)	(0.101)
Urbanization × Post				-0.031	-0.021	-0.019	-0.019	-0.019				-0.118**	-0.127**	-0.119**	-0.120**	-0.120**
				(0.027)	(0.031)	(0.031)	(0.031)	(0.031)				(0.049)	(0.052)	(0.052)	(0.051)	(0.052)
Literacy × Post					0.033	0.031	0.018	0.018					-0.031	-0.041	-0.105	-0.103
					(0.040)	(0.040)	(0.045)	(0.045)					(0.056)	(0.056)	(0.065)	(0.065)
WW1 × Post						-0.019	-0.021	-0.023						-0.078	-0.086	-0.090*
						(0.032)	(0.032)	(0.032)						(0.054)	(0.054)	(0.054)
South × Post							-0.009	-0.009							-0.047	-0.046
							(0.018)	(0.018)							(0.029)	(0.029)
US GDP Growth × QE								-0.046***								-0.086***
								(0.008)								(0.013)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	208	208	208	208	208	208	208	208	208	208	208	208	208	208	208
Observations	801	801	801	801	801	801	801	801	801	801	801	801	801	801	801	801
R2	0.787	0.788	0.787	0.788	0.788	0.788	0.787	0.792	0.482	0.482	0.506	0.514	0.514	0.515	0.517	0.531
F-stat	17.011	13.123	10.585	8.495	7.004	6.078	5.477	7.864	5.144	4.115	7.583	8.404	7.717	6.756	6.827	10.443
Mean Dep. Var.	0.109	0.109	0.109	0.109	0.109	0.109	0.109	0.109	0.253	0.253	0.253	0.253	0.253	0.253	0.253	0.253

Notes. This table displays the effect of exposure to the Quota acts on the number of mechanical and electrical engines. All regressions include district and year fixed effects. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.4: Robustness Regressions - Changes in Mechanical and Electrical Horsepower

	Dep. Var.: Changes in Horsepower by Mechanical Engines								Dep. Var.: Changes in Horsepower by Electrical Engines							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Quota Exposure × Post	-0.741**	-0.520*	-0.481	-0.492*	-0.243	-0.038	-0.151	0.070	-1.183***	-1.089***	-1.081***	-1.048***	-0.939***	-0.857***	-0.871**	-0.803**
	(0.303)	(0.305)	(0.298)	(0.295)	(0.337)	(0.356)	(0.392)	(0.378)	(0.261)	(0.269)	(0.273)	(0.272)	(0.302)	(0.326)	(0.340)	(0.340)
Population	-0.019	-0.088	-0.125	-0.125	-0.090	-0.119	-0.123	-0.094	-0.111*	-0.149**	-0.156**	-0.155**	-0.141**	-0.151**	-0.152**	-0.142**
	(0.094)	(0.096)	(0.101)	(0.101)	(0.103)	(0.105)	(0.105)	(0.104)	(0.063)	(0.065)	(0.066)	(0.065)	(0.066)	(0.067)	(0.069)	(0.069)
Extensive Margin × Post		-0.520***	-0.478***	-0.471***	-0.433***	-0.457***	-0.423***	-0.416***		-0.235**	-0.228**	-0.251**	-0.239**	-0.244**	-0.241**	-0.238**
		(0.120)	(0.117)	(0.119)	(0.117)	(0.119)	(0.120)	(0.120)		(0.116)	(0.115)	(0.117)	(0.116)	(0.118)	(0.120)	(0.120)
Agriculture × Post			-0.157*	-0.147	-0.084	-0.029	-0.020	-0.011			-0.029	-0.067	-0.040	-0.016	-0.015	-0.013
			(0.088)	(0.099)	(0.103)	(0.109)	(0.110)	(0.109)			(0.059)	(0.072)	(0.088)	(0.094)	(0.095)	(0.094)
Urbanization × Post				0.010	0.036	0.021	0.022	0.023				-0.039	-0.029	-0.034	-0.034	-0.034
				(0.046)	(0.049)	(0.048)	(0.048)	(0.047)				(0.038)	(0.043)	(0.043)	(0.043)	(0.043)
Literacy × Post					0.084	0.101*	0.134*	0.137*					0.036	0.043	0.047	0.048
					(0.059)	(0.059)	(0.074)	(0.073)					(0.056)	(0.058)	(0.066)	(0.066)
WW1 × Post						0.135*	0.140*	0.135*						0.054	0.054	0.052
						(0.074)	(0.075)	(0.074)						(0.051)	(0.053)	(0.053)
South × Post							0.024	0.024							0.003	0.003
							(0.032)	(0.032)							(0.026)	(0.026)
US GDP Growth × QE								-0.133***								-0.043***
								(0.025)								(0.011)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	209	209	209	209	209	209	208	208	208	208	208	208	208	208
Observations	804	804	804	804	804	804	804	804	802	802	802	802	802	802	802	802
R2	0.635	0.639	0.641	0.640	0.640	0.642	0.642	0.654	0.794	0.796	0.796	0.796	0.796	0.796	0.795	0.797
F-stat	5.579	8.255	7.340	6.731	5.278	5.230	4.718	7.525	8.866	8.101	6.917	5.824	4.978	4.571	4.138	7.539
Mean Dep. Var.	0.026	0.026	0.026	0.026	0.026	0.026	0.026	0.026	0.189	0.189	0.189	0.189	0.189	0.189	0.189	0.189

Notes. This table displays the effect of exposure to the Quota acts on the horsepower generates by mechanical and electrical engines. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.5: Robustness Regressions - Labor intensity of Technology: Mechanical and Electrical Engines

	Dep. Var.: Changes in Worker per Mechanical Engine								Dep. Var.: Changes in Worker per Electrical Engine							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Quota Exposure × Post	1.094***	1.212***	1.184***	1.235***	1.207***	1.221***	0.952***	0.840***	1.199***	1.261***	1.287***	1.212***	1.532***	1.629***	1.203***	0.992***
	(0.177)	(0.186)	(0.184)	(0.184)	(0.207)	(0.228)	(0.224)	(0.217)	(0.322)	(0.323)	(0.318)	(0.312)	(0.315)	(0.358)	(0.375)	(0.371)
Population	0.390***	0.357***	0.395***	0.395***	0.392***	0.390***	0.377***	0.364***	0.490***	0.472***	0.439***	0.438***	0.479***	0.466***	0.445***	0.428***
	(0.052)	(0.052)	(0.052)	(0.050)	(0.052)	(0.054)	(0.057)	(0.058)	(0.091)	(0.092)	(0.091)	(0.092)	(0.088)	(0.089)	(0.085)	(0.086)
Extensive Margin × Post		-0.269**	-0.336***	-0.377***	-0.380***	-0.382***	-0.315***	-0.317***		-0.141	-0.086	-0.029	0.013	-0.002	0.120	0.128
		(0.105)	(0.097)	(0.098)	(0.099)	(0.099)	(0.107)	(0.107)		(0.129)	(0.131)	(0.135)	(0.137)	(0.133)	(0.151)	(0.151)
Agriculture × Post			0.175***	0.110**	0.104	0.108	0.128*	0.122*			-0.147**	-0.072	0.003	0.029	0.072	0.065
			(0.044)	(0.050)	(0.064)	(0.067)	(0.066)	(0.065)			(0.069)	(0.092)	(0.105)	(0.113)	(0.103)	(0.103)
Urbanization × Post				-0.067***	-0.070**	-0.071**	-0.072**	-0.072**				0.083	0.114**	0.107*	0.110**	0.109*
				(0.025)	(0.030)	(0.030)	(0.029)	(0.029)				(0.054)	(0.056)	(0.057)	(0.056)	(0.056)
Literacy × Post					-0.009	-0.007	0.068	0.066					0.102*	0.110*	0.245***	0.240***
					(0.039)	(0.040)	(0.047)	(0.047)					(0.059)	(0.061)	(0.071)	(0.071)
WW1 × Post						0.009	0.018	0.021						0.059	0.080	0.084
						(0.040)	(0.037)	(0.036)						(0.066)	(0.061)	(0.061)
South × Post							0.056***	0.056***							0.097***	0.097***
							(0.019)	(0.019)							(0.032)	(0.032)
US GDP Growth × QE								0.069***								0.111***
								(0.012)								(0.013)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	208	208	208	208	208	208	208	208	208	208	208	208	208	208	208
Observations	784	784	784	784	784	784	784	784	784	784	784	784	784	784	784	784
R2	0.787	0.790	0.797	0.799	0.799	0.799	0.802	0.811	0.540	0.540	0.543	0.546	0.549	0.549	0.559	0.579
F-stat	30.935	25.223	24.419	22.054	18.849	16.724	15.967	19.526	13.089	10.230	8.316	7.232	8.589	7.440	8.890	17.873
Mean Dep. Var.	-0.069	-0.069	-0.069	-0.069	-0.069	-0.069	-0.069	-0.069	-0.214	-0.214	-0.214	-0.214	-0.214	-0.214	-0.214	-0.214

Notes. This table displays the effect of exposure to the Quota acts on the worker-per-mechanical engine and worker-per-electrical engine ratios. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.6: Robustness Regressions - Labor intensity of Technology: Mechanical and Electrical Horsepower

	Dep. Var.: Changes in Worker per Mechanical Horsepower								Dep. Var.: Changes in Worker per Electrical Horsepower							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Quota Exposure × Post	0.960***	0.862**	0.808***	0.897***	0.622*	0.433	0.373	0.066	1.455***	1.495***	1.465***	1.500***	1.323***	1.270***	1.011***	0.868***
	(0.328)	(0.333)	(0.306)	(0.302)	(0.352)	(0.374)	(0.425)	(0.402)	(0.258)	(0.265)	(0.263)	(0.268)	(0.296)	(0.318)	(0.338)	(0.327)
Population	0.275***	0.303***	0.379***	0.379***	0.345***	0.372***	0.369***	0.333***	0.446***	0.434***	0.473***	0.474***	0.452***	0.459***	0.446***	0.432***
	(0.091)	(0.091)	(0.092)	(0.093)	(0.096)	(0.101)	(0.101)	(0.100)	(0.056)	(0.055)	(0.058)	(0.058)	(0.059)	(0.061)	(0.063)	(0.064)
Extensive Margin × Post		0.224*	0.100	0.039	0.005	0.034	0.051	0.047		-0.093	-0.164	-0.190	-0.212	-0.205	-0.135	-0.135
		(0.123)	(0.124)	(0.131)	(0.138)	(0.133)	(0.136)	(0.137)		(0.124)	(0.123)	(0.131)	(0.133)	(0.132)	(0.134)	(0.135)
Agriculture × Post			0.338***	0.251**	0.184	0.131	0.137	0.123			0.184***	0.145**	0.104	0.089	0.113	0.108
			(0.099)	(0.108)	(0.117)	(0.126)	(0.125)	(0.125)			(0.060)	(0.073)	(0.081)	(0.083)	(0.081)	(0.081)
Urbanization × Post				-0.096**	-0.123**	-0.111**	-0.111**	-0.114**				-0.042	-0.059	-0.056	-0.055	-0.056
				(0.045)	(0.049)	(0.049)	(0.049)	(0.048)				(0.037)	(0.040)	(0.041)	(0.040)	(0.040)
Literacy × Post					-0.089	-0.105*	-0.086	-0.095					-0.057	-0.061	0.017	0.015
					(0.057)	(0.058)	(0.073)	(0.072)					(0.047)	(0.048)	(0.054)	(0.053)
WW1 × Post						-0.118	-0.115	-0.109						-0.033	-0.022	-0.018
						(0.082)	(0.082)	(0.080)						(0.052)	(0.048)	(0.047)
South × Post							0.013	0.013							0.057**	0.057**
							(0.035)	(0.034)							(0.023)	(0.023)
US GDP Growth × QE								0.167***								0.079***
								(0.023)								(0.012)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	209	209	209	209	209	209	208	208	208	208	208	208	208	208
Observations	787	787	787	787	787	787	787	787	785	785	785	785	785	785	785	785
R2	0.658	0.658	0.667	0.669	0.669	0.670	0.670	0.689	0.837	0.836	0.840	0.840	0.840	0.840	0.842	0.847
F-stat	6.525	6.115	7.875	8.833	7.042	6.708	6.242	13.979	31.240	24.003	21.232	18.434	15.649	13.830	14.040	19.302
Mean Dep. Var.	0.013	0.013	0.013	0.013	0.013	0.013	0.013	0.013	-0.149	-0.149	-0.149	-0.149	-0.149	-0.149	-0.149	-0.149

Notes. This table displays the effect of exposure to the Quota acts on the worker-per-mechanical horsepower and worker-per-electrical horsepower ratios. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.7: Robustness Regressions - Population Growth

	Dep. Var.: Population Growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quota Exposure × Post	0.408***	0.446***	0.422***	0.443***	0.515***	0.469***	0.342**	0.284**
	(0.113)	(0.124)	(0.120)	(0.120)	(0.134)	(0.132)	(0.134)	(0.136)
Population	0.146***	0.142***	0.165***	0.166***	0.180***	0.183***	0.178***	0.179***
	(0.030)	(0.030)	(0.031)	(0.030)	(0.032)	(0.033)	(0.034)	(0.033)
Extensive Margin × Post		-0.065	-0.091	-0.109*	-0.101*	-0.094*	-0.058	-0.058
		(0.055)	(0.057)	(0.059)	(0.055)	(0.053)	(0.051)	(0.052)
Agriculture × Post			0.095***	0.072***	0.090***	0.078**	0.089***	0.088***
			(0.024)	(0.026)	(0.031)	(0.031)	(0.030)	(0.030)
Urbanization × Post				-0.026**	-0.020	-0.017	-0.017	-0.019
				(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
Literacy × Post					0.024	0.019	0.059***	0.059***
					(0.017)	(0.016)	(0.019)	(0.019)
WW1 × Post						-0.030*	-0.021	-0.020
						(0.017)	(0.015)	(0.015)
South × Post							0.029***	0.029***
							(0.008)	(0.008)
US GDP Growth × QE								0.018**
								(0.008)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	204	204	204	204	204	204	204	204
Observations	751	751	751	751	751	751	751	751
R2	0.453	0.454	0.475	0.479	0.480	0.482	0.495	0.501
F-stat	13.726	10.139	10.400	12.096	14.920	14.928	16.897	15.768
Mean Dep. Var.	1.042	1.042	1.042	1.042	1.042	1.042	1.042	1.042

Notes. This table displays the effect of exposure to the Quota Acts on population growth. Population growth is defined as the decade-on-decade percentage change in population. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.8: Robustness Regressions - Changes in Industrial Employment

	Dep. Var.: Industry Workers Growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quota Exposure × Post	1.825***	1.497***	1.471***	1.469***	1.457***	1.413**	1.173*	0.996*
	(0.427)	(0.476)	(0.477)	(0.488)	(0.552)	(0.591)	(0.604)	(0.581)
Population	0.206*	0.243**	0.262**	0.261**	0.259*	0.266*	0.255*	0.213
	(0.123)	(0.123)	(0.126)	(0.127)	(0.137)	(0.142)	(0.143)	(0.142)
Extensive Margin × Post		0.652	0.619	0.621	0.616	0.631	0.709*	0.701*
		(0.403)	(0.404)	(0.409)	(0.420)	(0.427)	(0.422)	(0.419)
Agriculture × Post			0.077	0.079	0.075	0.064	0.081	0.068
			(0.082)	(0.094)	(0.108)	(0.111)	(0.112)	(0.110)
Urbanization × Post				0.001	0.000	0.003	0.002	0.000
				(0.058)	(0.061)	(0.062)	(0.062)	(0.061)
Literacy × Post					-0.004	-0.008	0.053	0.052
					(0.072)	(0.073)	(0.085)	(0.084)
WW1 × Post						-0.026	-0.014	-0.009
						(0.065)	(0.065)	(0.065)
South × Post							0.047	0.046
							(0.037)	(0.037)
US GDP Growth × QE								0.136***
								(0.042)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	205	205	205	205	205	205	205	205
Observations	742	742	742	742	742	742	742	742
R2	0.541	0.543	0.543	0.542	0.541	0.540	0.540	0.548
F-stat	6.777	6.951	6.664	5.616	5.194	4.603	4.602	4.748
Mean Dep. Var.	0.060	0.060	0.060	0.060	0.060	0.060	0.060	0.060

Notes. This table displays the effect of exposure to the Quota Acts on changes in industrial employment. Industrial employment growth is defined as the decade-on-decade percentage change in industrial employment. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

TABLE B.9: ROBUSTNESS REGRESSIONS - CHANGES IN THE SHARE OF INDUSTRIAL WORKERS

	Dep. Var.: Changes in Share of Industrial Workers							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quota Exposure × Post	1.455***	1.139***	1.118***	1.237***	1.204**	1.168**	1.154**	0.888
	(0.356)	(0.411)	(0.412)	(0.425)	(0.465)	(0.473)	(0.520)	(0.538)
Population	0.074	0.105	0.124	0.134	0.129	0.134	0.134	0.078
	(0.090)	(0.088)	(0.092)	(0.093)	(0.096)	(0.099)	(0.101)	(0.097)
Extensive Margin × Post		0.613*	0.579	0.509	0.497	0.509	0.513	0.488
		(0.353)	(0.351)	(0.360)	(0.372)	(0.382)	(0.390)	(0.384)
Agriculture × Post			0.072	0.004	-0.005	-0.014	-0.013	-0.028
			(0.059)	(0.075)	(0.096)	(0.101)	(0.104)	(0.101)
Urbanization × Post				-0.077	-0.081	-0.078	-0.078	-0.074
				(0.053)	(0.061)	(0.062)	(0.062)	(0.061)
Literacy × Post					-0.012	-0.014	-0.011	-0.022
					(0.064)	(0.063)	(0.080)	(0.079)
WW1 × Post						-0.020	-0.020	-0.019
						(0.071)	(0.071)	(0.069)
South × Post							0.003	-0.001
							(0.036)	(0.035)
US GDP Growth × QE								0.173***
								(0.035)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	205	205	205	205	205	205	205	205
Observations	729	729	729	729	729	729	729	729
R2	0.477	0.479	0.479	0.480	0.479	0.478	0.477	0.500
F-stat	6.068	6.487	5.568	5.131	4.430	3.894	3.522	7.913
Mean Dep. Var.	0.051	0.051	0.051	0.051	0.051	0.051	0.051	0.051

Notes. This table displays the effect of exposure to the Quota Acts on changes in the share of industrial workers relative to total employment. The share of industrial workers is defined as the ratio between industrial workers and total employment. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are always clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Literacy is the number of people who could read and write as a share of the overall population in 1901. South is a dummy equal to zero if the district is in the EU NUTS 2 ITC or ITH region, and one otherwise. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.10: Robustness Regressions - Technology Adoption in Selected Manufacture Sectors

	Dep. Var.: Mechanical Engines in Construction Firms							Dep. Var.: Electrical Engines in Textile Firms						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Quota Exposure × Post	-1.267***	-1.184***	-1.207***	-1.187***	-1.185***	-1.012**	-0.996**	-2.323***	-2.346***	-2.216***	-2.329***	-2.131***	-2.081***	-2.082***
	(0.385)	(0.396)	(0.393)	(0.383)	(0.411)	(0.423)	(0.413)	(0.692)	(0.720)	(0.705)	(0.715)	(0.758)	(0.760)	(0.760)
Population	0.316**	0.297**	0.316**	0.317**	0.316**	0.337**	0.303**	0.316	0.322	0.199	0.200	0.154	0.160	0.161
	(0.133)	(0.136)	(0.138)	(0.138)	(0.141)	(0.139)	(0.143)	(0.207)	(0.208)	(0.220)	(0.221)	(0.226)	(0.227)	(0.227)
Extensive Margin × Post		-0.181	-0.210	-0.225	-0.225	-0.216	-0.227		0.050	0.205	0.287	0.242	0.245	0.245
		(0.159)	(0.161)	(0.167)	(0.165)	(0.164)	(0.166)		(0.439)	(0.427)	(0.458)	(0.456)	(0.456)	(0.456)
Agriculture × Post			0.094	0.076	0.076	0.078	0.211			-0.530***	-0.421**	-0.362	-0.361	-0.372
			(0.097)	(0.126)	(0.134)	(0.134)	(0.163)			(0.157)	(0.212)	(0.227)	(0.227)	(0.287)
Urbanization × Post				-0.020	-0.020	-0.020	-0.026				0.115	0.090	0.090	0.088
				(0.071)	(0.070)	(0.070)	(0.069)				(0.131)	(0.133)	(0.133)	(0.142)
WW1 × Post					0.002	-0.004	-0.175*					0.171	0.169	0.179
					(0.080)	(0.080)	(0.102)					(0.143)	(0.143)	(0.162)
US GDP Growth × QE						-0.123***	-0.124***						-0.033	-0.033
						(0.033)	(0.033)						(0.025)	(0.025)
Construction Employment × Post							0.001*							
							(0.000)							
Textile Employment × Post														-0.000
														(0.000)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	208	208	208	208	208	208	209	209	209	209	209	209	209
Observations	786	786	786	786	786	786	786	791	791	791	791	791	791	791
R2	0.808	0.808	0.808	0.807	0.807	0.811	0.811	0.874	0.873	0.876	0.876	0.876	0.876	0.876
F-stat	5.352	4.724	4.134	3.747	3.590	4.663	4.524	21.263	17.038	16.080	13.346	12.327	10.956	9.865
Mean Dep. Var.	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.472	0.472	0.472	0.472	0.472	0.472	0.472

Notes. This table displays the effect of exposure to the Quota acts on the number of mechanical and electrical engines in construction and textile manufacture firms. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Sector Employment is the 1901-number of manufacture workers. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.11: Robustness Regressions - Employment Growth in Selected Manufacture Sectors

	Dep. Var.: Changes in Employment in Construction Firms							Dep. Var.: Changes in Employment in Textile Firms						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Quota Exposure × Post	4.611***	6.103***	6.103***	6.359***	5.520***	6.192***	6.203***	5.247***	5.651***	5.651***	5.977***	5.167***	6.795***	6.724***
	(1.411)	(1.626)	(1.627)	(1.635)	(1.669)	(1.896)	(1.895)	(1.266)	(1.398)	(1.399)	(1.336)	(1.318)	(1.439)	(1.465)
Population	0.027	-0.095	-0.091	-0.103	0.055	0.113	0.146	-0.518	-0.550	-0.549	-0.559	-0.404	-0.288	-0.243
	(0.339)	(0.359)	(0.377)	(0.374)	(0.384)	(0.382)	(0.386)	(0.340)	(0.343)	(0.365)	(0.360)	(0.350)	(0.334)	(0.330)
Extensive Margin × Post		-2.693**	-2.703**	-2.887**	-2.342*	-2.288*	-2.290*		-0.715	-0.720	-0.964	-0.432	-0.364	-0.237
		(1.293)	(1.277)	(1.328)	(1.316)	(1.311)	(1.304)		(0.991)	(1.006)	(1.025)	(1.029)	(1.019)	(0.997)
Agriculture × Post			0.016	-0.128	-0.277	-0.281	-0.424			0.007	-0.170	-0.325	-0.291	-0.711*
			(0.257)	(0.274)	(0.294)	(0.295)	(0.377)			(0.302)	(0.355)	(0.342)	(0.344)	(0.398)
Urbanization × Post				-0.157	-0.073	-0.069	-0.060				-0.192	-0.112	-0.105	-0.196
				(0.167)	(0.164)	(0.164)	(0.163)				(0.181)	(0.178)	(0.177)	(0.183)
WW1 × Post					-0.460**	-0.470**	-0.291					-0.458**	-0.472**	-0.149
					(0.204)	(0.205)	(0.280)					(0.181)	(0.182)	(0.226)
US GDP Growth × QE						-0.369*	-0.369*						-0.807***	-0.805***
						(0.196)	(0.196)						(0.122)	(0.123)
Construction Employment × Post							-0.001							
							(0.001)							
Textile Employment × Post														-0.001**
														(0.000)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	200	200	200	200	200	200	200	200	200	200	200	200	200	200
Observations	778	778	778	778	778	778	778	774	774	774	774	774	774	774
R2	0.315	0.318	0.317	0.316	0.318	0.329	0.328	0.451	0.450	0.449	0.449	0.451	0.499	0.501
F-stat	20.249	16.662	14.117	10.407	8.837	8.022	7.963	5.665	4.555	3.925	4.339	5.170	7.683	8.530
Mean Dep. Var.	0.553	0.553	0.553	0.553	0.553	0.553	0.553	0.291	0.291	0.291	0.291	0.291	0.291	0.291

Notes. This table displays the effect of exposure to the Quota acts on the the the growth rate of workers employed in construction and textile manufacture firms. All regressions include district and year fixed effects. Outcome variables are defined in growth rate. Standard errors are clustered at the district level. Extensive margin is the emigration rate defined as the ratio between 1890-1914 emigration and 1880-population. Agriculture is the number of agriculture workers in 1901. Urbanization is the share of people living in cities no smaller than 10,000 inhabitants in 1901. Sector Employment is the 1901-number of manufacture workers. WW1 is the number of deaths due to the First World War, divided by 10,000.

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

Table B.12: First Stage Regressions

	Shift Share			Railway	
	Pre 1924	Pre WW1	Pre Quota	RAP total	RAP region
IV QE	0.778***	0.833***	0.791***	3.398***	8.255***
	(0.038)	(0.038)	(0.039)	(1.169)	(2.317)
Extensive Margin × Post	0.012	-0.001	0.011	0.205***	0.187**
	(0.015)	(0.012)	(0.015)	(0.077)	(0.072)
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Number of Districts	207	207	207	207	207
Observations	754	754	754	754	754
KP Wald rk F	414.366	483.861	422.069	8.456	12.692

Notes. This table reports the result of the first stage instrumental variable estimation. The instrument (IV Quota Exposure) in the first three columns is defined in (4). The first column reports the correlation between QE and its instrument over the full sample (1890-1939). Instrument in column (2) restricts the emigrant outflow to the pre-WW1 period (1890-1914). Column (3) reports the results when considering emigrants over the pre-Quota period (1890-1924). In the last two columns, the instrument is defined as in equation (6). Results in column “RA total” use aggregate emigration instead of regional emigration. All regressions partial out district and year fixed effects. Further controls are population, the emigration rate and labor market slackness in 1901 interacted with a post-treatment dummy. K-P F-stat refers to the Kleibergen-Paap F-statistic for weak instrument.

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

TABLE B.13: URBANIZATION AND SHARE OF WORKERS EMPLOYED IN INDUSTRY AND AGRICULTURE - 2SLS

	Urbanization	Industrialization	Agriculture
Panel A: OLS			
Quota Exposure × Post	-0.410***	1.316***	-0.606***
	(0.109)	(0.414)	(0.153)
Panel B: 2SLS Shift Share			
Quota Exposure × Post	-0.332***	1.382***	-0.603***
	(0.124)	(0.474)	(0.177)
Panel C: 2SLS Railway Regional			
Quota Exposure × Post	-0.866**	2.379	-1.091***
	(0.359)	(1.545)	(0.393)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Districts	205	207	208
Observations	995	731	746
Mean Dep. Var.	0.279	0.044	-0.031

Notes. This table reports the effect of exposure to the Quota Acts on urbanization and changes in the share of industrial and agricultural workers relative to overall employment. Urbanization is defined as the share of the population living in cities no smaller than 10,000 inhabitants. The share of sector employment is defined as the ratio between sector and aggregate employment. Panel A presents reduced form estimates. Panel B reports 2SLS estimates based on the instrument defined in (4). All regressions include district and year fixed effects. Further controls are log-population, labor market slackness in 1901 interacted with a post-treatment dummy and the emigration rate, defined as the number of emigrants 1890-1914 relative to 1880-population, interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are robust and clustered at the district level.

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

Table B.14: Labor intensity and emigration - 2sls

	Worker/Firm		Worker/Engine		Worker/Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Panel A: OLS						
Quota Exposure × Post	0.208	0.184	1.135***	1.050***	0.248	1.212***
	(0.239)	(0.396)	(0.174)	(0.339)	(0.353)	(0.300)
Panel B: 2SLS Shift Share						
Quota Exposure × Post	0.482*	0.563	1.264***	0.699**	-0.251	0.964***
	(0.276)	(0.428)	(0.190)	(0.327)	(0.403)	(0.294)
Panel C: 2SLS Railway Regional						
Quota Exposure × Post	1.432**	1.453	1.588***	1.725*	-0.337	1.531**
	(0.700)	(1.200)	(0.524)	(0.984)	(1.157)	(0.753)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	208	207	209	208
Observations	785	787	785	783	786	785
Mean Dep. Var.	-0.082	-0.054	-0.077	-0.258	-0.078	-0.195

Notes. This table displays the effect of being exposed to the Quota Acts on various measures for labor intensity in production. The first and second columns report the effect on, respectively, the worker-per-firm and the worker-per-firm with engine ratios. The third and fourth columns show the effect on the ratio between worker and mechanical and electrical engines; the fifth and sixth display the effect the ratio between workers and mechanical and electrical horsepower. Panel A presents reduced form estimates. Panel B reports 2SLS estimates based on the instrument defined in (4). All regressions include district and year fixed effects. Further controls are log-population, labor market slackness in 1901 interacted with a post-treatment dummy and the emigration rate, defined as the number of emigrants 1890-1914 relative to 1880-population, interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are robust and clustered at the district level.

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

Table B.15: Capital Investment and Emigration by Industry Sectors - 2SLS

	Mining		Agriculture		Steel		Construction		Textile		Chemical	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Panel A: Total Firms												
Quota Exposure × Post	-1.593***	-0.879***	-0.409***	-0.209*	-0.095	0.020	0.677**	0.674**	-0.876***	-0.548**	0.149	-0.264
	(0.306)	(0.323)	(0.130)	(0.119)	(0.103)	(0.111)	(0.314)	(0.328)	(0.188)	(0.216)	(0.474)	(0.434)
Panel B: Firms with Engine												
Quota Exposure × Post	-0.119	0.023	-0.222**	-0.036	0.426	0.715	0.173	0.303	-1.388***	-1.015***	-0.119	-0.255
	(0.321)	(0.357)	(0.110)	(0.112)	(0.451)	(0.467)	(0.241)	(0.247)	(0.282)	(0.325)	(0.320)	(0.283)
Panel C: Mechanical Engines												
Quota Exposure × Post	-0.586***	-0.427**	-0.462**	-0.129	-0.853*	-0.817*	-1.289***	-0.759	-0.047	0.042	-0.982***	-0.897**
	(0.180)	(0.206)	(0.211)	(0.215)	(0.437)	(0.437)	(0.407)	(0.460)	(0.082)	(0.081)	(0.327)	(0.355)
Panel D: Electrical Engines												
Quota Exposure × Post	-2.553***	-1.974**	1.581***	1.525**	-0.669***	-0.465**	-0.982**	-0.226	-2.280***	-1.749**	-0.628	-0.059
	(0.976)	(0.946)	(0.538)	(0.605)	(0.234)	(0.227)	(0.479)	(0.421)	(0.711)	(0.803)	(0.567)	(0.545)
Panel E: Mechanical Horsepower												
Quota Exposure × Post	-2.079***	-1.522*	-0.985***	-0.363	-1.244	-1.528	-2.209***	-1.293**	2.264***	1.346*	-0.324	0.172
	(0.719)	(0.802)	(0.274)	(0.290)	(1.235)	(1.189)	(0.486)	(0.596)	(0.673)	(0.706)	(1.018)	(1.002)
Panel F: Electrical Horsepower												
Quota Exposure × Post	-1.415	-1.041	1.293*	1.606*	-0.565	-0.592	-1.689	-0.330	-0.780*	-0.418	0.583	0.810
	(1.577)	(1.667)	(0.735)	(0.823)	(0.350)	(0.360)	(1.155)	(1.022)	(0.397)	(0.413)	(0.723)	(0.818)
Observations	785	785	782	782	787	787	786	786	787	787	785	785
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	206	206	209	209	209	209	209	209	209	209

Notes. This table displays the effect of QE on employment by manufacture sector. OLS and 2SLS columns respectively report reduced-form and shift-share instrumental variable estimates. All regressions include district and year fixed effects, log-population and 1901-labor marked slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level.

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

Table B.16: Changes in Industry Employment by Sector - 2SLS

	Mining	Agriculture	Steel	Construction	Textile	Chemical
Panel A: OLS						
Quota Exposure $\times$ Post	0.442	-2.459*	1.379	6.103***	5.651***	0.017
	(0.388)	(1.261)	(1.573)	(1.626)	(1.398)	(0.308)
Panel B: 2SLS						
Quota Exposure $\times$ Post	0.419	-2.275	2.757*	5.912***	7.077***	0.158
	(0.494)	(1.583)	(1.575)	(2.183)	(1.327)	(0.361)
Observations	685	776	775	778	774	681
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	194	200	198	200	200	195
Observations	685	776	775	778	774	681
F-stat	8.111	5.319	5.982	15.309	8.373	1.828
Mean Dep. Var.	0.724	0.422	0.250	0.553	0.291	0.751

Notes. This table displays the effect of exposure to the Quota Acts on changes in employment by manufacture sector. Hence, column "Agriculture" reports the impact of QE on employment in manufacture firms working in agriculture, not that on agriculture. We do not show the "public utility" sector due to data availability, and a residual sector of unassigned firms. Panel A presents reduced form estimates. Panel B reports 2SLS estimates based on the instrument defined in (4). All regressions include district and year fixed effects. Further controls are log-population, changes in industrial employment, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level.

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

Table B.17: Population Growth Varying the Measurement of Quota Exposure

	Baseline	Weighted		Alternative periods		
	(1)	(2)	(3)	(4)	(5)	(6)
QE × Post	0.449***					
	(0.124)					
QE × Post: decreasing weight		1.001**				
		(0.386)				
QE × Post: increasing weight			2.328***			
			(0.551)			
QE× Post: 1902-1905				1.664***		
				(0.411)		
QE× Post: 1906-1909					1.241***	
					(0.414)	
QE× Post: 1910-1913						0.953**
						(0.436)
Extensive Margin × Post	-0.068	-0.045	-0.083	-0.087	-0.047	-0.025
	(0.055)	(0.057)	(0.055)	(0.056)	(0.055)	(0.057)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	204	204	204	204	204	204
Observations	751	751	751	751	751	751
R2	0.452	0.441	0.459	0.459	0.444	0.436
F-stat	9.932	8.538	11.094	11.375	8.965	7.966
Mean Dep. Var.	1.042	1.042	1.042	1.042	1.042	1.042
Std. Beta Coef.	0.240	-0.008	0.268	0.266	0.198	0.149

Notes. This table displays the effect of exposure to the Quota Acts on population growth. Different measures of Quota Exposure are used, as further robustness. Hence, column "Baseline" reports the estimate for Quota Exposure as defined and used throughout the paper. Column "Weighted" reports the coefficients for two measures of Quota Exposure constructed using an exponential smoothing with coefficient 0.9: "decreasing weight" assigns lower weight to US emigration further back in time; "increasing weight" assigns lower weight to more recent US emigration. Column "Alternative periods" shows instead the estimates for Quota Exposure constructed using only US emigration from selected sub-periods of time: we use three different periods, respectively 1902-1905, 1906-1910, 1910-1913. All regressions include district and year fixed effects. Further controls are log-population, changes in industrial employment, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level.

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

Table B.18: Employment in Manufacture Varying the Measurement of Quota Exposures

	Baseline	Weighted		Alternative periods		
	(1)	(2)	(3)	(4)	(5)	(6)
Quota Exposure × Post	1.510***					
	(0.475)					
QE × Post: decreasing weight		4.241***				
		(1.389)				
QE × Post: increasing weight			6.848***			
			(2.304)			
QE× Post: 1902-1905				5.356***		
				(1.615)		
QE× Post: 1906-1909					4.647***	
					(1.483)	
QE× Post: 1910-1913						4.702***
						(1.458)
Extensive Margin × Post	0.637	0.675*	0.626	0.578	0.698*	0.724*
	(0.400)	(0.406)	(0.404)	(0.400)	(0.398)	(0.399)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	205	205	205	205	205	205
Observations	742	742	742	742	742	742
R2	0.542	0.542	0.542	0.543	0.542	0.541
F-stat	7.004	6.400	6.579	7.275	6.653	6.870
Mean Dep. Var.	0.060	0.060	0.060	0.060	0.060	0.060
Std. Beta Coef.	0.123	0.119	0.119	0.126	0.116	0.116

Notes. This table displays the effect of exposure to the Quota Acts on industrial employment growth. Different measures of Quota Exposure are used, as further robustness. Hence, column "Baseline" reports the estimate for Quota Exposure as defined and used throughout the paper. Column "Weighted" reports the coefficients for two measures of Quota Exposure constructed using an exponential smoothing with coefficient 0.9: "decreasing weight" assigns lower weight to US emigration further back in time; "increasing weight" assigns lower weight to more recent US emigration. Column "Alternative periods" shows instead the estimates for Quota Exposure constructed using only US emigration from selected sub-periods of time: we use three different periods, respectively 1902-1905, 1906-1910, 1910-1913. All regressions include district and year fixed effects. Further controls are log-population, changes in industrial employment, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level.

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

Table B.19: Investment in Capital Goods Using Time-Weighted Quota Exposure

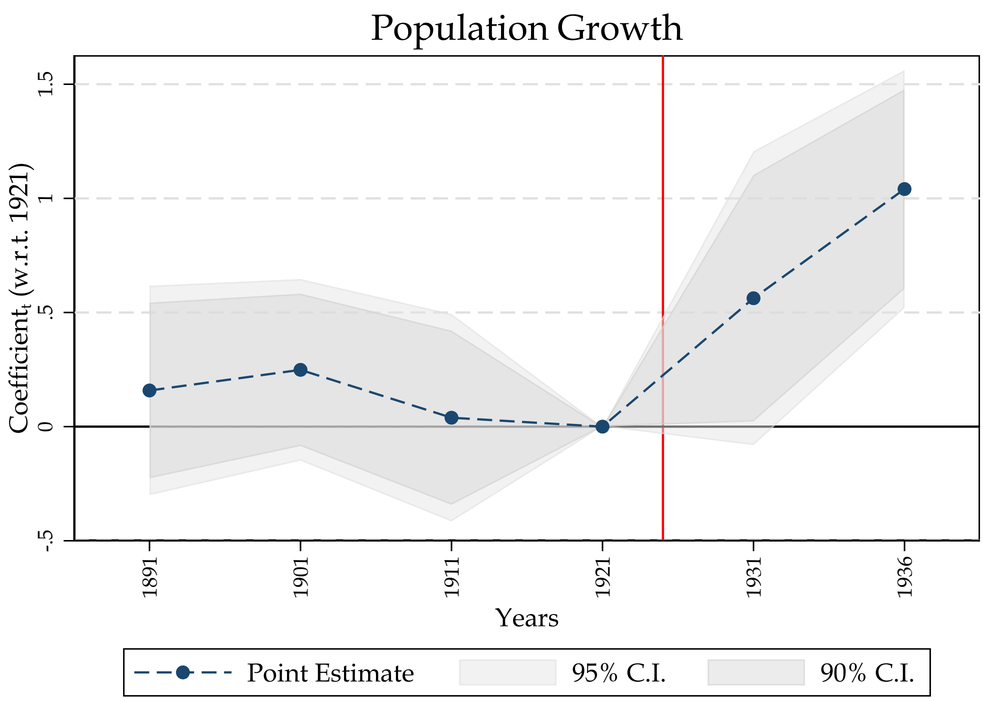
	Firm		Engine		Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
QE × Post: decreasing	0.641	1.075	-3.000***	-3.331***	-2.008**	-4.014***
	(0.724)	(1.161)	(0.493)	(0.930)	(0.985)	(0.965)
Extensive Margin × Post	-0.086	-0.037	0.215**	0.015	-0.260**	-0.009
	(0.116)	(0.185)	(0.103)	(0.135)	(0.117)	(0.151)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	208	207	207	209	208
Observations	785	785	783	784	785	784
R2	0.963	0.834	0.426	0.953	0.834	0.936
F-stat	0.418	0.272	13.464	4.478	4.915	10.468
Mean Dep. Var.	0.766	0.582	0.018	0.793	0.270	0.828
Std. Beta Coef.	0.012	0.030	-0.367	-0.057	-0.048	-0.088

Notes. This table displays the effect of exposure to the Quota Acts on changes on various measures for capital and investment and technology adoption. Quota Exposure is constructed using an exponential smoothing with coefficient 0.9. In this case, we assigns lower weight to US emigration further back in time. All regressions include district and year fixed effects. Further controls are log-population, changes in industrial employment, the emigration rate and 1901 labor market slackness interacted with a post-treatment dummy. Outcome variables are defined in growth rate. Standard errors are clustered at the district level.

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

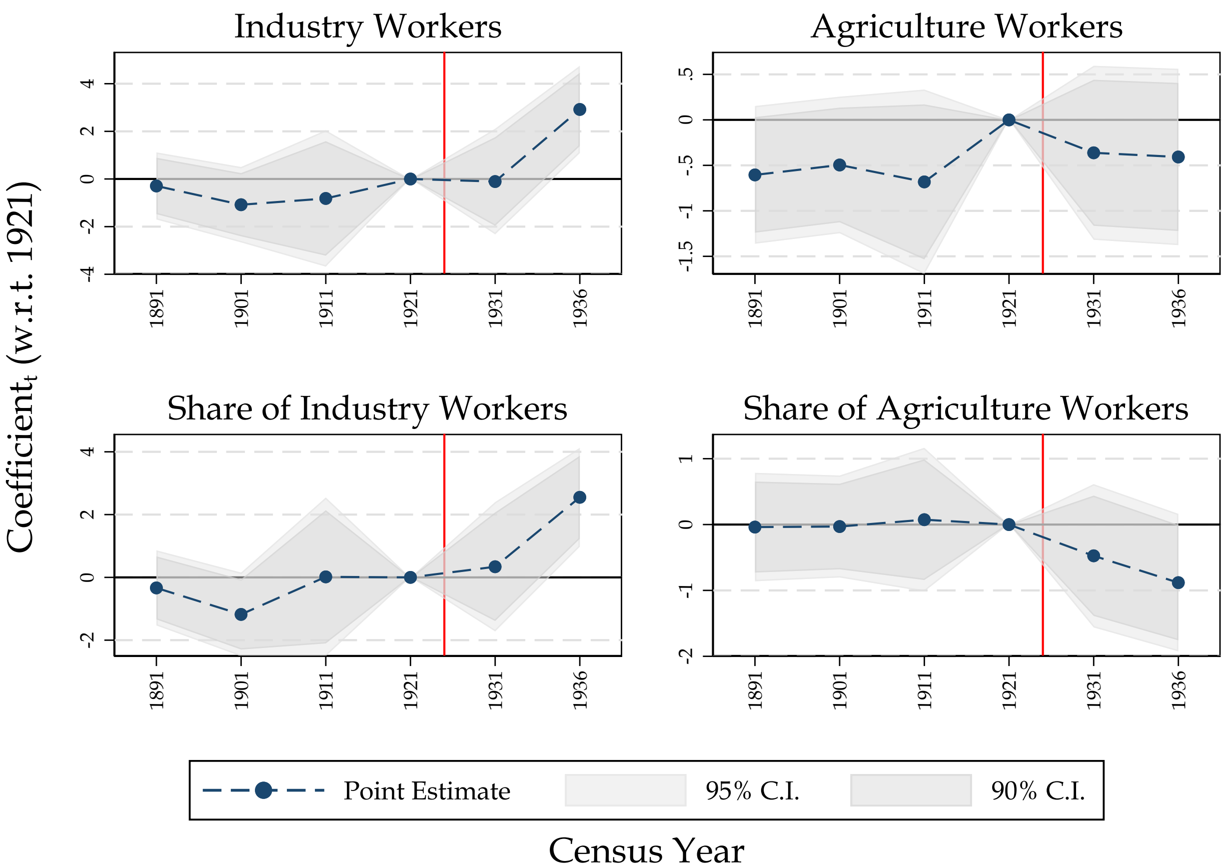
C Additional Figures

Figure C.1: Event-Study of Population Growth and the Quota Acts



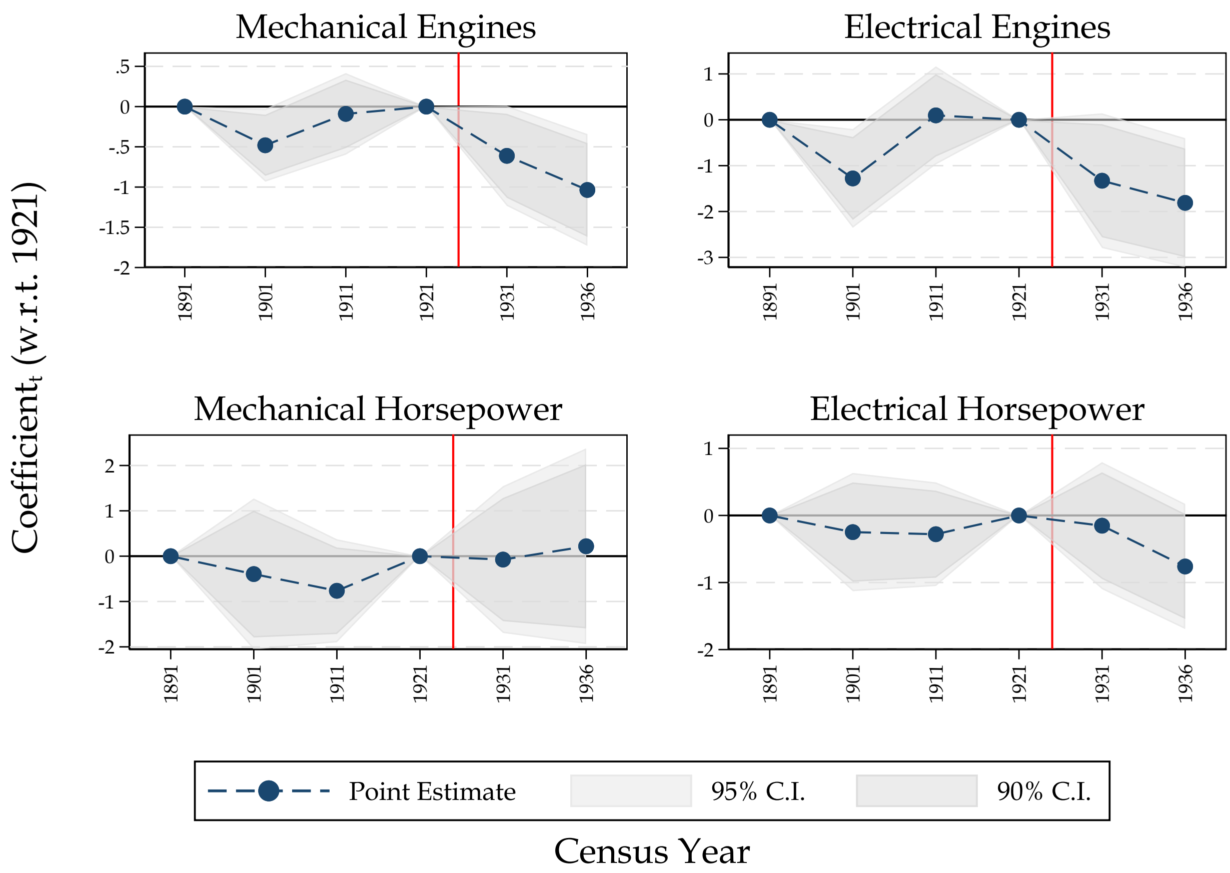
Notes. This figure plots the coefficient of the treatment measure (QE) interacted with census-decade time dummies. Regressions include district and year fixed effects, and region-by-year fixed effects. Further controls are the population in level, and 1901 labor market slackness interacted with census-decade dummies. Standard errors are clustered at the district-by-year level. Bands report 90% and 95% confidence levels. The red line indicates the 1924 (Johnson-Reed) Quota Act.

Figure C.2: Event-Study of Industrial and Agriculture Employment



Notes. This figure plots the coefficients of the treatment measure (QE) interacted with census-decade time dummies. Regressions include district and year fixed effects, and region-by-year fixed effects. Further controls are the population in level, and 1901 labor market slackness interacted with census-decade dummies. Standard errors are clustered at the district-by-year level. Bands report 90% and 95% confidence levels. The red line indicates the 1924 (Johnson-Reed) Quota Act.

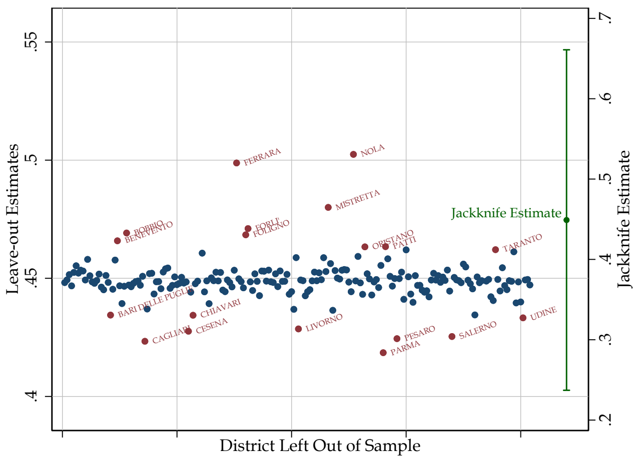
FIGURE C.3: EVENT-STUDY OF TECHNOLOGY ADOPTION AND CAPITAL INVESTMENT



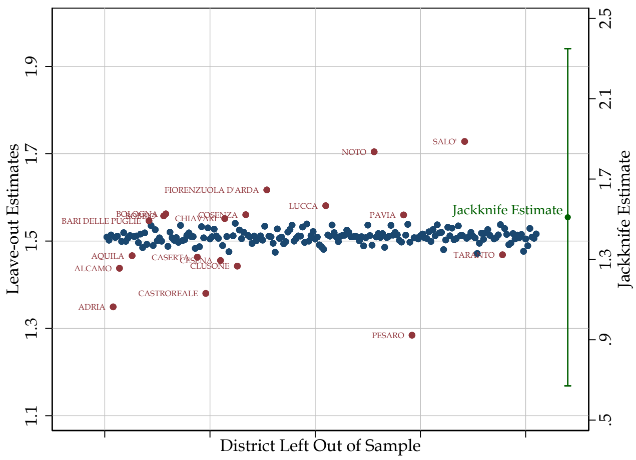
Notes. This figure plots the coefficients of the treatment measure (QE) interacted with census-decade time dummies. Regressions include district and year fixed effects, and region-by-year fixed effects. Further controls are the population in level, and 1901 labor market slackness interacted with census-decade dummies. Standard errors are clustered at the district-by-year level. Bands report 90% and 95% confidence levels. The red line indicates the 1924 (Johnson-Reed) Quota Act. For capital variables, 1931 actually refers to the 1927 Census of Manufacture.

Figure C.4: Jackknife Estimation Routine

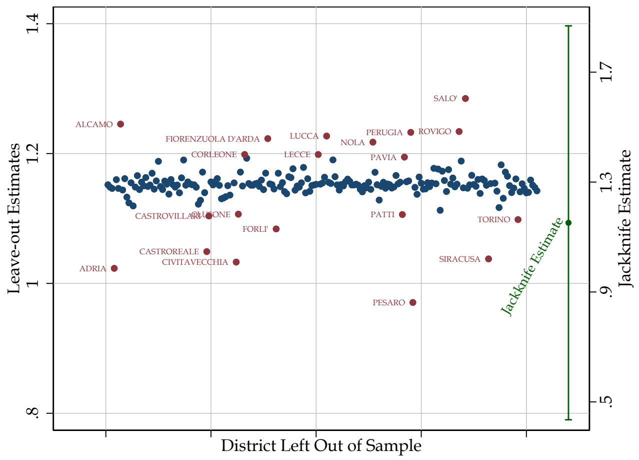
(a) Population Growth



(b) Industrial Employment

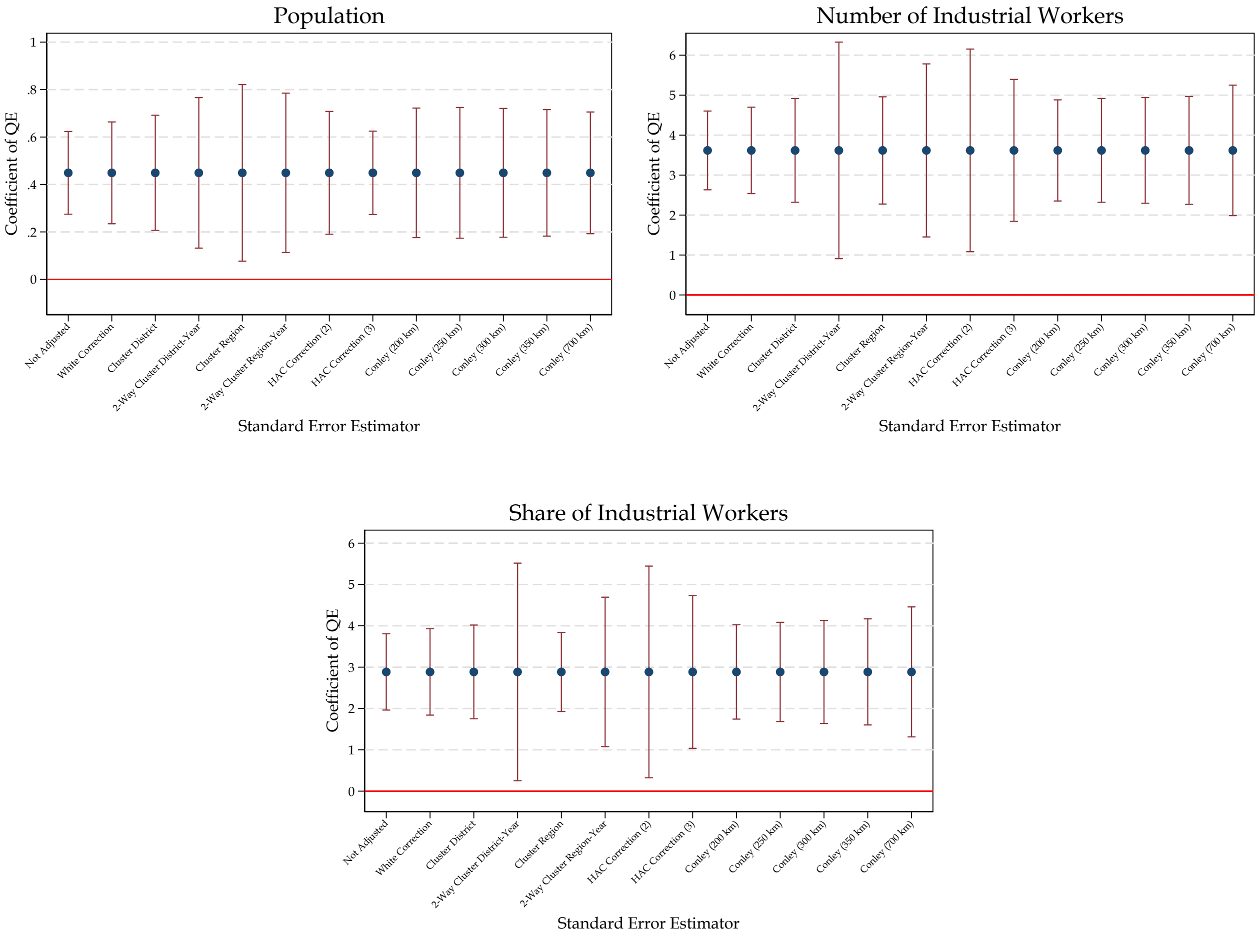


(c) Share of Industrial Workers



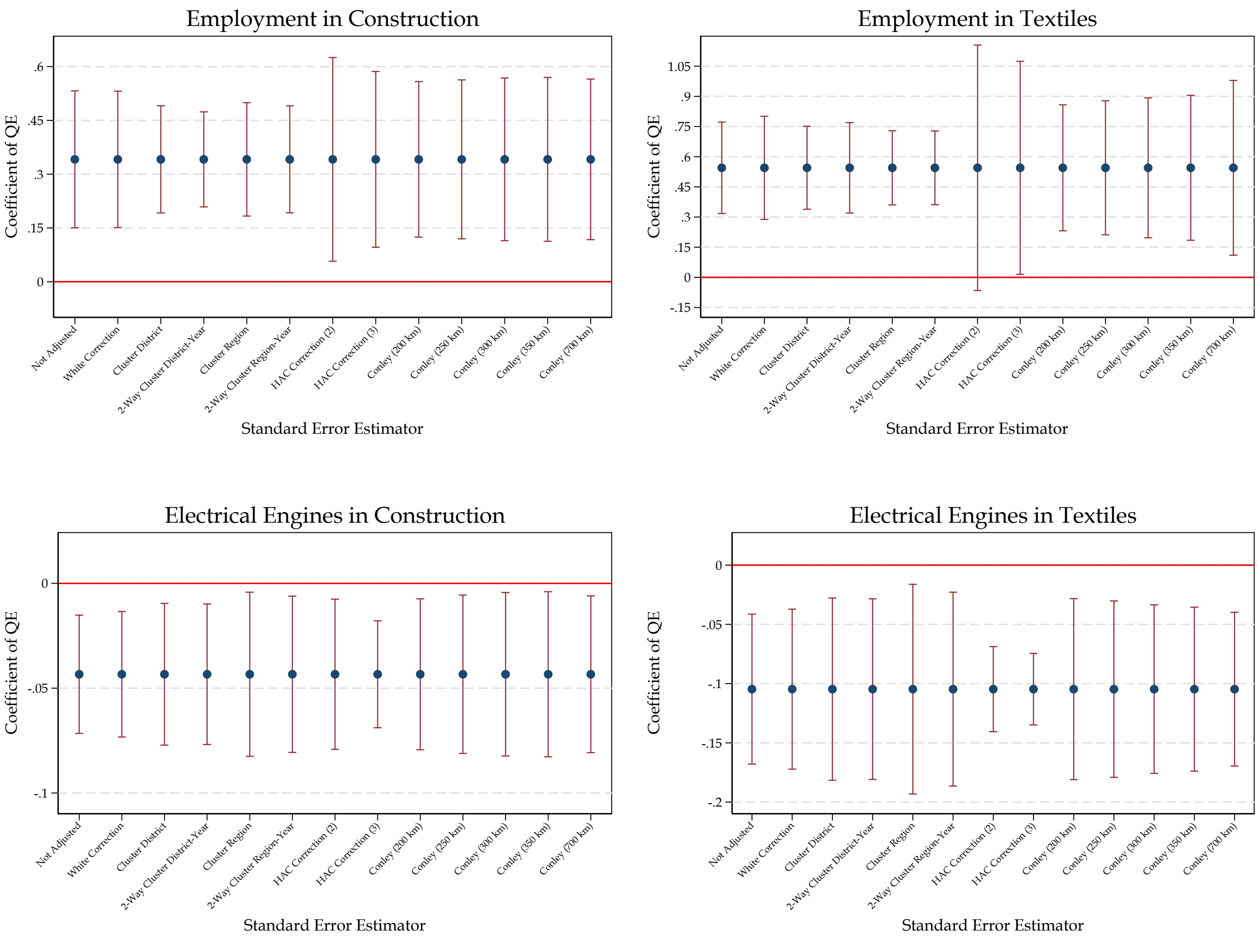
Notes. For each dependent variable shown in the header, each blue dot (on the left y-axis) reports the coefficient of Quota Exposure in the baseline difference-in-differences model dropping one district at a time. Red dots (on the left y-axis) are coefficients above and below respectively the 95th and the 5th percentiles. The green dot (on the right y-axis) reports the Jackknife estimator of the same coefficient, along with its 90% confidence bands.

Figure C.5: Standard Error Analysis



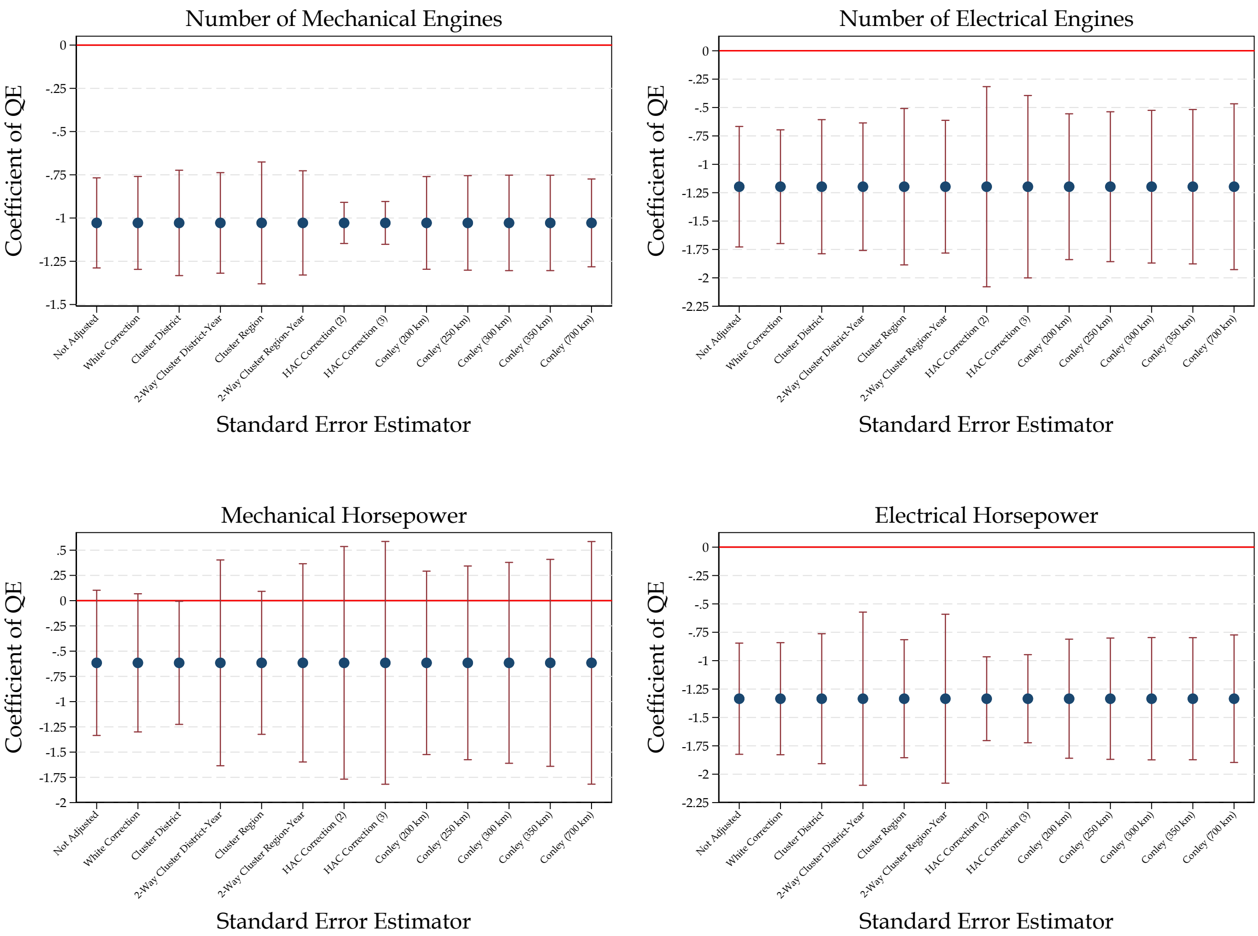
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard errors which allow for heteroskedasticity; several clustered standard errors allowing for within-group autocorrelation; the Driscoll & Kraay (1998) correction for autocorrelation at two different time lags; several Conley (1999) estimates allowing for time and spatial autocorrelation. For the Conley SEs, we set maximal time-autocorrelation at 2 lags, and vary the radius of spatial autocorrelation.

Figure C.5: Continued from previous page



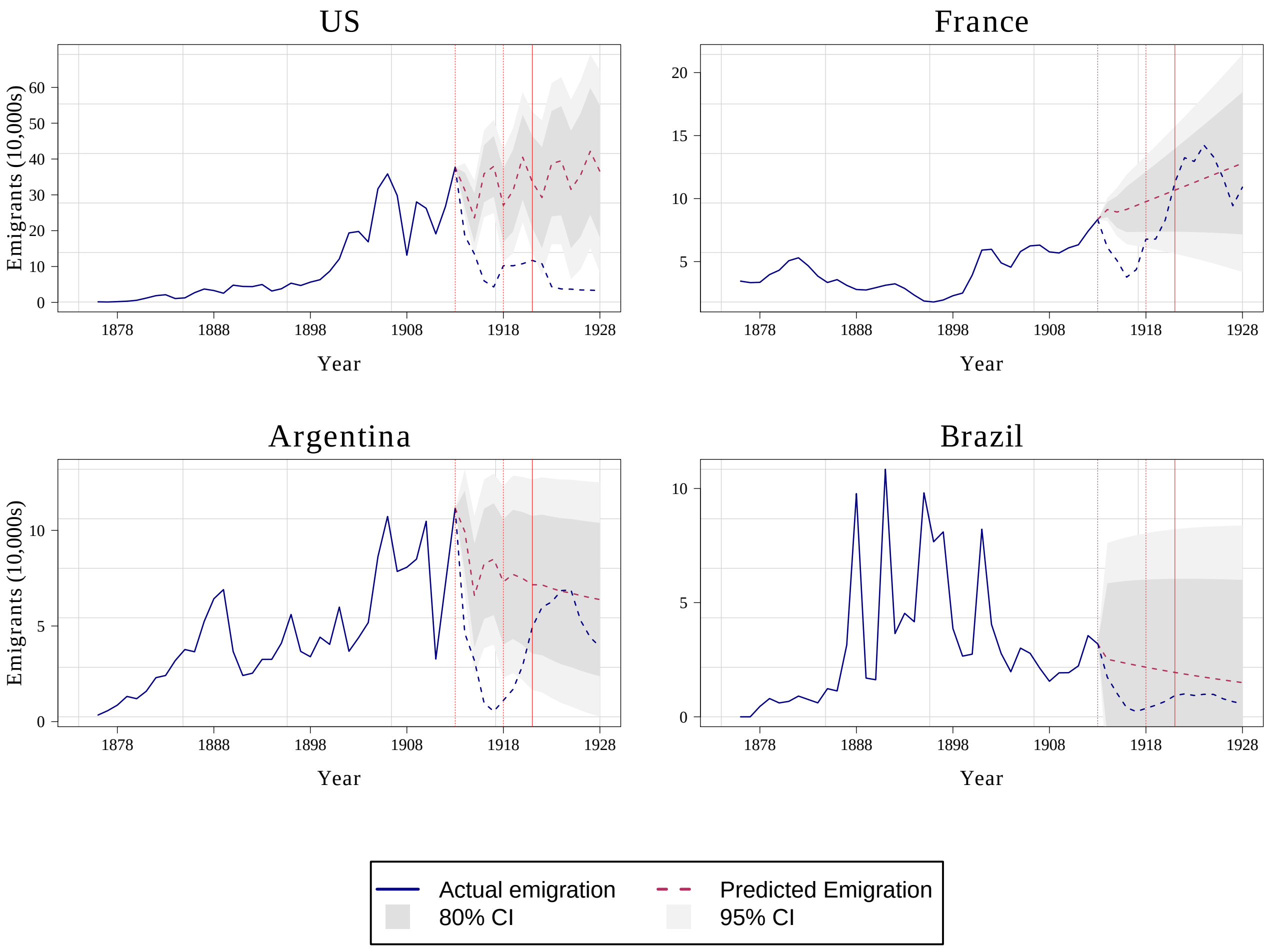
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard errors which allow for heteroskedasticity; several clustered standard errors allowing for within-group autocorrelation; the Driscoll & Kraay (1998) correction for autocorrelation at two different time lags; several Conley (1999) estimates allowing for time and spatial autocorrelation. For the Conley SEs, we set maximal time-autocorrelation at 2 lags, and vary the radius of spatial autocorrelation.

Figure C.5: Continued from previous page



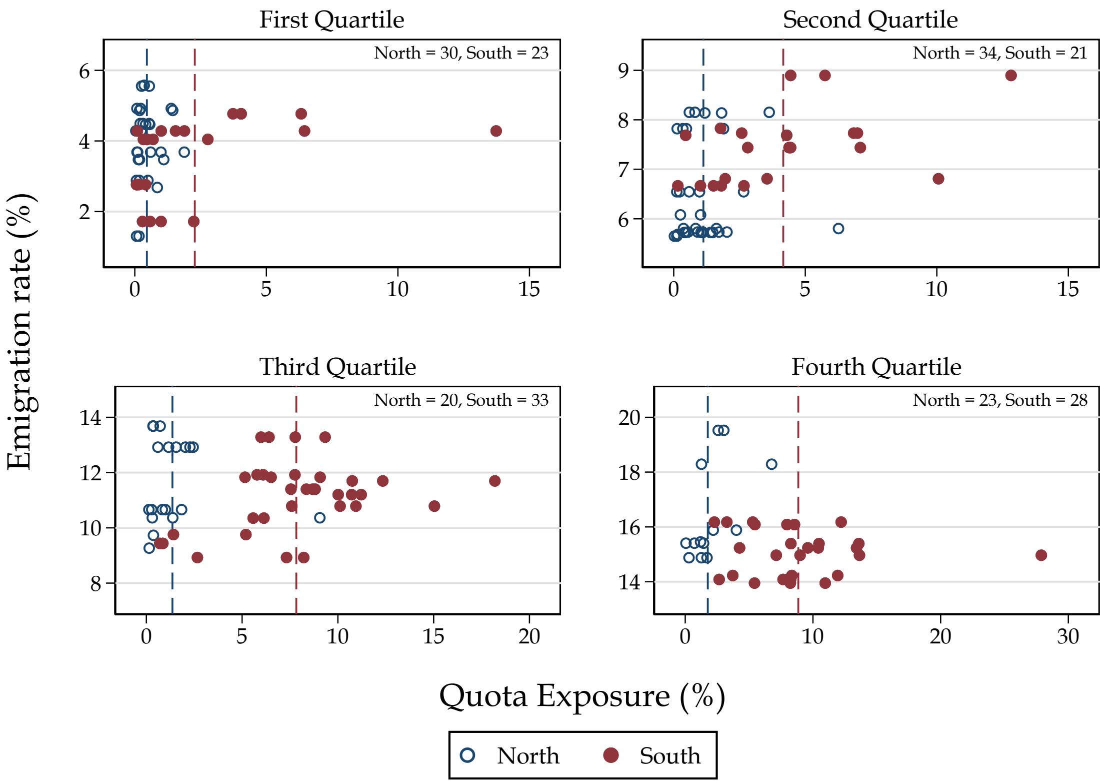
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard errors which allow for heteroskedasticity; several clustered standard errors allowing for within-group autocorrelation; the Driscoll & Kraay (1998) correction for autocorrelation at two different time lags; several Conley (1999) estimates allowing for time and spatial autocorrelation. For the Conley SEs, we set maximal time-autocorrelation at 2 lags, and vary the radius of spatial autocorrelation.

Figure C.6: Emigration towards main destination countries



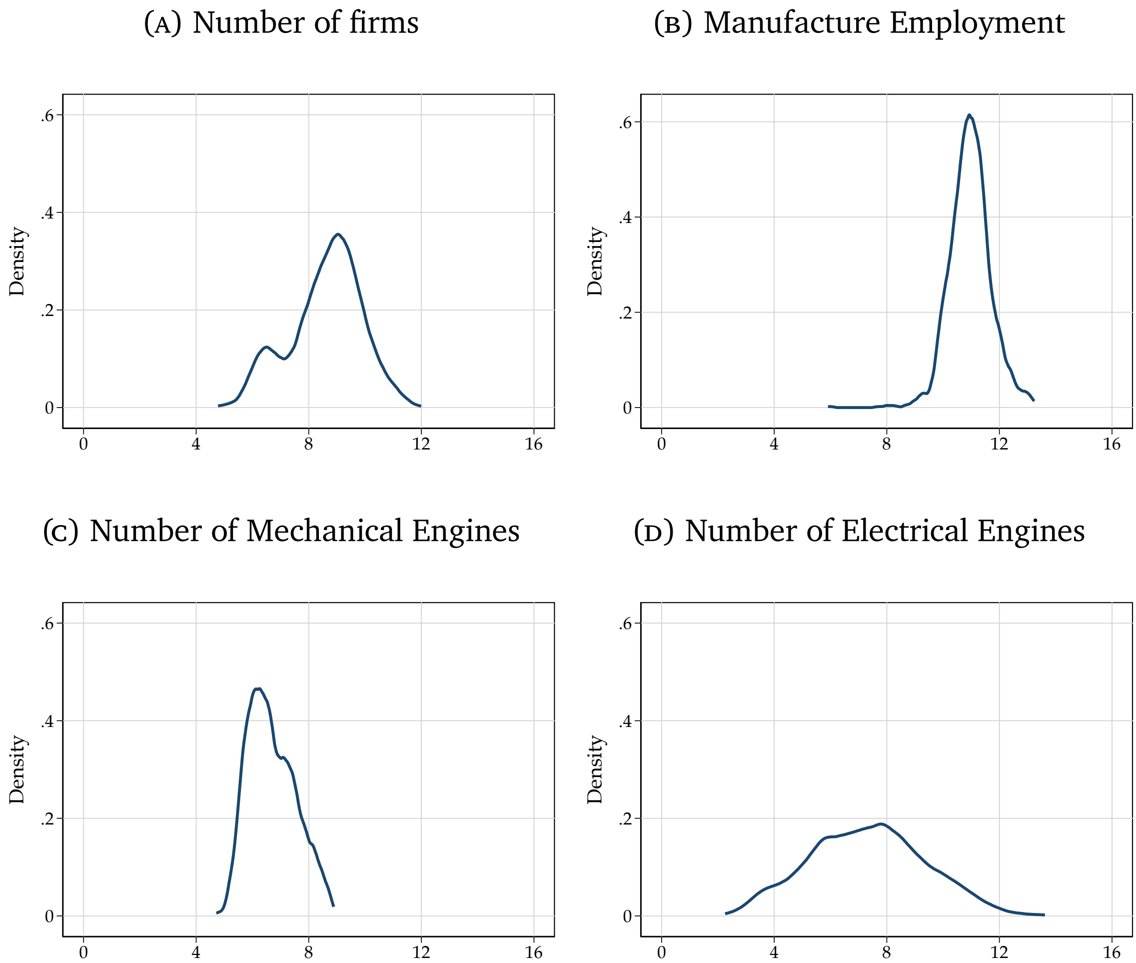
Notes. These figures plot the number of Italian emigrants towards the main destination countries over the period 1876-1930. Overall, these countries account for about the 70% of total emigration from Italy during the whole period. The blue line represents the actual number of migrants (and its moving average starting from WWI). The red line reports the predicted number of migrants obtained from an ARIMA model estimated over the historical number of emigrants before WW1. Bands plot 95% and 80% confidence interval for the predicted values. The figures suggest that predictions based on historical emigration patterns reflect variation in the post-Quota period for all destination countries but the US.

Figure C.7: Counties by Quota Exposure and Emigration Rate's Quartile



Notes. Each dot represents a district and reports its emigration rate (%, on the y-axis) and its quota exposure (%, on the x-axis). Panels are split by quartiles of the emigration rate. Blue dots are for districts in northern regions; red dots are for districts in southern regions. Red and blue vertical lines display the mean quota exposure for northern and southern regions, respectively. In each panel, on the top-right we report the number of northern and southern districts in the plot. This figure shows that conditional on the emigration rate, northern districts display substantially lower quota exposure despite sizable emigration rate. Hence, our identifying variation conditionally compares northern *vis-à-vis* southern districts, instead of exploiting within-South variation.

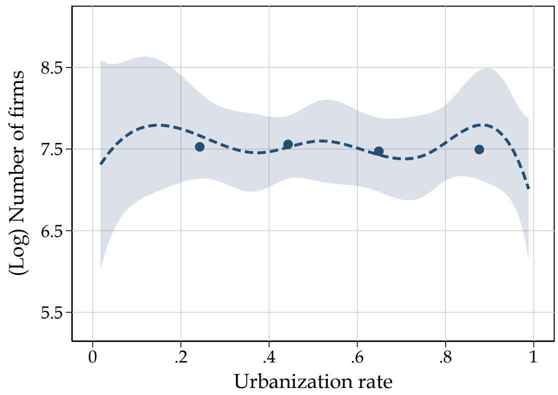
FIGURE C.8: DISTRIBUTION OF CAPITAL AND LABOR



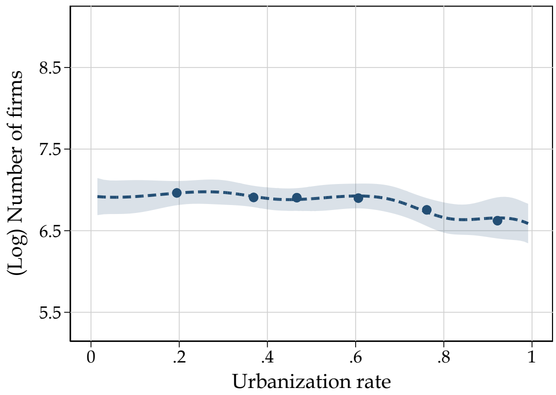
Notes. Each line represents the density plot of capital and labor variables we use throughout our analysis. Variables are expressed in logarithm. We plot the distribution over the whole sample period, 1881-1936. On the top left we show the distribution of the number of firms in each district. On the top right we show the distribution of the total number of workers. On the bottom, we show the distributions for the number of mechanical (left panel) and electrical (right panel) engines.

Figure C.9: Number of firms and Urbanization Rate in the Pre Quota Period

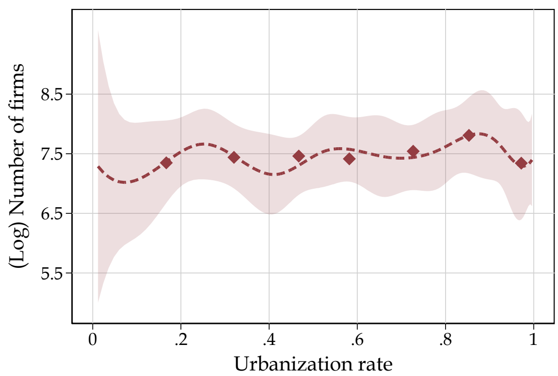
(a) No Control Below Median



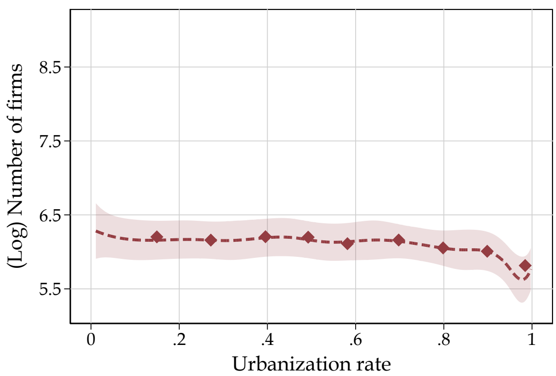
(b) With Controls Below Median



(c) No Control Above Median



(d) With Controls Above Median



Notes. The graphs display binned scatter plots relating the total number of firms (in logarithm) and the urbanization rate at district level in the pre-Quota period (before 1921). The blue lines refer to those district whose Quota Exposure is below the median. Red lines, instead, refer to district with Quota Exposure above the median. The left panels show the results of a binscatter generalized linear regression of the number of firms in a given district to its urbanization rate in the pre-Quota period. For the right panels, we also control for the emigration rate (intensive margin), and for year and province fixed effects. Dashed lines represent the cubic B-spline estimate of the regression function of interest. 95% confidence bands are based on the same spline. The plots show there is no significant difference between the correlation between number of firms and urbanization rate, by exposure to the Quotas.

D A Model of Directed Technical Adoption

In this section we develop a simple framework to rationalize our main findings in the context of labor-saving technical change theory. Proofs and further analytical insights on the baseline environment can be found in section D.3.

D.1 Theoretical Framework

In this section we develop a simple analytical framework inspired to Zeira (1998) and San (2021) to clarify the empirical implications of directed technical change and adoption theory. The core assumption we make is that capital goods—hereafter, machines—substitute labor as a production input. We thus implicitly restrict technological progress to be labor-saving, differently from *e.g.* Acemoglu (2002, 2007). The decision of the firm to adopt productivity-enhancing machines will depend on their price relative to the cost of labor. In the equilibrium a labor supply shock—such as the one induced by IRPs—dampens the incentive to adopt machines because it pushes down the wage, hence prompting firms to substitute capital with labor.

Consider a closed economy with one consumption good, and a representative household supplying labor. The consumption good is produced by a continuum of tasks $j \in [0, 1]$. Each task can be performed with either labor or machines. The amount of machines in task j is denoted by $x(j)$, whereas the amount of labor employed is $e(j)$. Note that each task can be fulfilled with either machines or labor, but not both. This is intended to model in a stylized manner labor-saving machines. To simplify the analysis and following Zeira (1998) we assume that machines fully depreciate at the end of the period, hence the model is essentially static.

The final consumption good is produced by identical perfectly competitive firms with the following production function:

$$Y = A\left[\int_0^{\iota} mx(j)^{\alpha}\, dj + \int_{\iota}^{1} e(j)^{\alpha}\, dj\right] \tag{D.1}$$

where A is a technology parameter, m is the relative productivity of machines and $\alpha \in (0, 1)$ is a production parameter. We assume $m \in (0, 1)$ following San (2021), and restrict machines to be equally productive across tasks j. The choice variable $\iota \in [0, 1]$ denotes *industrialization* defined as the share of automatized tasks, which are those fulfilled by machines. We assume that tasks are ordered by degree of complexity. Because the marginal cost of producing machines—which we define below—is increasing in complexity, the price of machines is non-decreasing in j. It is therefore without loss of generality to assume that the first ι tasks are automatized. This is because the final good producer will first automatize tasks whose machine costs the least, since the relative productivity of machines is constant across tasks. We assume that there is a fixed stock of labor $L > 0$ which is supplied inelastically by the household.

The problem of the representative final good producer is therefore to choose the industrialization level ι, and input quantities $x(j)$ and $e(j)$ for each task, to maximize profits

$$\max_{\iota, \{x(j), e(j)\}_{j\in[0,1]}} Y - \int_0^{\iota} p(j)x(j)\, dj - w\int_{\iota}^{1} e(j)\, dj \tag{D.2}$$

where $p(j)$ is the price of a machine for task j, w is the nominal wage, subject to the technology constraint (D.1). Note that the price of the consumption good is implicitly normalized to one. In section D.3, we formally show that the demand for machines and labor are given by the following demand schedules:

$$x(j) = p(j)^{-\frac{1}{1-\alpha}} (\alpha A m)^{\frac{1}{1-\alpha}} \quad \forall j \in [0, \iota] \tag{D.3a}$$

$$e(j) = w^{-\frac{1}{1-\alpha}} (\alpha A)^{\frac{1}{1-\alpha}} \quad \forall j \in [\iota, 1] \tag{D.3b}$$

Combining (D.3a)-(D.3b) with the first order condition for the industrialization rate, it follows that in the equilibrium ι^* is pinned down by the following:

$$m = \left[\frac{p(\iota^*)}{w}\right]^{\alpha} \tag{D.4}$$

The economic intuition behind condition (D.4) is that at the marginal task, *i.e.* the last automatized task, the price of the machine fulfilling the task must be equal to the cost of labor, adjusted by the technology parameter and the relative productivity of machines.

Each machine is produced by a monopolist, following Zeira (1998). The machine producer will seek to set the monopoly price which maximizes its profits subject to demand for machines (D.3a). We assume that the marginal cost of machines $\psi(\cdot)$ is increasing in the complexity of tasks, *i.e.* $\psi'(\cdot) > 0$. Moreover, we assume that the marginal cost function satisfies basic Inada conditions.[51] This is intended to capture the idea that machines substituting low-skill tasks are not as expensive as those replacing tasks on the right side of the skill distribution of workers. The problem of the machine producer is therefore

$$\max_{p(j)} [p(j) - \psi(j)]\, x(j) \tag{D.5}$$

subject to (D.3a). In section D.3, we show that the first-order conditions imply

$$p(j) = \min\left\{mw, \frac{\psi(j)}{\alpha}\right\} \tag{D.6}$$

where the minimum descends from the observation that because each task can be performed by labor as well as by machines, setting a price greater than the productivity-adjusted wage simply pushes the final goods producer not to automatize the task. We now obtain two technical results to ensure existence and uniqueness of the equilibrium. The formal definition of the competitive equilibrium in this economy as well as the proofs of all lemmas and propositions can be found in section D.3.

Lemma D.1. *In the equilibrium, the marginal task ι^* is such that $p(\iota^*) = \psi(\iota^*)/\alpha = wm^{1/\alpha}$.*

Combining this result with the equilibrium conditions of the final goods producer, we derive the following strong existence result.

Proposition D.1. *There exists one and only one $\iota^* \in [0, 1]$ which solves the problem of the final good producer (D.3a)-(D.3b)-(D.4) as well as the problem of the machine producers (D.6) and verifies labor market clearing. In particular, the equilibrium industrialization ι^* is the solution to the following:*

$$\psi(\iota^*) = L^{\alpha-1}(1-\iota^*)^{1-\alpha}\alpha^2 A m^{1/\alpha}.$$

[51]In this setting, this simply boils down to $\lim_{j\uparrow 1} \psi(j) = +\infty$ and $\lim_{j\downarrow 0} \psi(j) = 0$. The economic intuition behind these is that it is never profitable for the representative firm to automatize all tasks. Similarly, there is always at least one task that is automatized. Note that while these assumptions are sufficient for the existence of an equilibrium, they are not necessary.

This concludes our analytical characterization of the environment. We now exploit the model to deliver a number of testable predictions which will guide our empirical analysis.

D.2 Empirical Testable Implications

Having established the existence of the equilibrium, we can now derive two key empirical implications of this directed technical adoption setting. First, note that Lemma D.1 conveys the basic intuition of the model. In particular, we have $\psi(\iota^*) = \alpha m^{1/\alpha} w$, hence an increase in the nominal wage induces industrialization to rise because $\psi'(\cdot) > 0$ by assumption. The economic intuition behind this result is that if the cost of labor increases, then the final good producer will seek to automatize more tasks in order to avoid paying the increase in the wage. This is summarized in the following implication statement.

Implication D.1. Following an exogenous increase (resp. decrease) in the nominal wage w, the share of tasks performed by machines ι^* increases (resp. decreases).

A similar comparative static result follows considering an increase in the labor stock. To see it, notice that because the nominal wage is invariant across tasks, from (D.3b) and labor market clearing the total labor stock L is evenly allocated across the $(1 - \iota^*)$ non-automated tasks. Using this insight, we obtain the following empirical prediction.

Implication D.2. Following an exogenous increase (resp. decrease) in the labor supply stock L, the share of tasks performed by machines ι^* decreases (resp. increases).

This is the key implication of the model that we test in the paper. In our setting, we provide evidence that immigration restriction policies induce positive labor supply shocks, hence increasing the labor stock. We show that firms operating in districts which were more exposed to the Quota Acts decreased investment in machinery—section 4.2—and increased employment—-section 4.4. These findings are fully in line with the empirical predictions D.2 of the model and hence provide evidence in favor of labor-saving directed technical adoption.

Implications D.1-D.2 are tested using aggregate data on manufacture employment and investment in physical capital. We provide some results at a more disaggregated sector-level. We refer to relatively backward and modern sectors as respectively "First" and "Second Industrial Revolution" sectors. For concreteness, the former comprise textiles and construction whereas the latter mainly refer to the chemical and metalworking industries. To capture this difference in the model, we assume that machines in the relatively modern sector are more productive than in the relatively backward one. The following result holds.

Implication D.3. Let M and L respectively denote a modern and a backward sector which differ by the productivity of machines $1 > m_M > m_B > 0$. Then, following a positive (resp. negative) labor supply shock, the share of industrialized tasks if $m = m_B$ decreases (resp. increases) more than if $m = m_M$.

We test this prediction using data on employment and technology adoption at the sector level of aggregation. We find that in First Industrial Revolution sectors investment in capital goods and employment respectively decreased and increased considerably more than in Second Industrial Revolution industries. This finding is fully consistent with prediction D.3.

D.3 Proofs of Analytical Results

Solution of the problem of the final good producer. Plugging the technology constraint into problem (D.2), the problem of the final good producer reads out as follows:

$$\max_{\iota,\{x(j),e(j)\}_{j\in[0,1]}} A\left[\int_0^\iota mx(j)^\alpha\,dj+\int_\iota^1 e(j)^\alpha\,dj\right]-\int_0^\iota p(j)x(j)\,dj-w\int_\iota^1 e(j)\,dj$$

The—necessary and sufficient—first-order conditions with respect to labor and capital in the generic task j are

$$x(j)=p(j)^{-\frac{1}{1-\alpha}}(\alpha Am)^{\frac{1}{\alpha}}\quad\forall j\in[0,\iota]$$
$$e(j)=w^{-\frac{1}{1-\alpha}}(\alpha A)^{\frac{1}{\alpha}}\qquad\forall j\in[\iota,1]$$

To obtain the first-order condition for the optimal industrialization rate, apply the Leibniz integral rule with respect to ι to get:

$$x(\iota^*)\left[mx(\iota^*)^{\alpha-1}-p(\iota^*)\right]=e(\iota^*)\left[e(\iota^*)^{\alpha-1}-w\right]$$

Plugging (D.3a)-(D.3b) into the expression above we get $m=(p(\iota^*)/w)^\alpha$. □

Solution of the problem of the monopolist. The solution is trivial upon plugging (D.3a) into the objective function (D.5). □

Proof of Lemma D.1. From (D.6) and (D.4), it is

$$p(\iota^*)=\min\left\{\frac{\psi(\iota^*)}{\alpha},mw\right\}$$
$$p(\iota^*)=m^{1/\alpha}w$$

Hence, we have

$$m=\left[\frac{\min\left\{\frac{\psi(\iota^*)}{\alpha},mw\right\}}{w}\right]^\alpha$$

We can distinguish two cases. Assume $mw\leq\psi(\iota^*)/\alpha$. This implies that $m=m^\alpha$, which is only verified if $m=1$ or $m=0$. Since by assumption $m\in(0,1)$, this can never hold. We are left with the case $mw>\psi(\iota^*)/\alpha$. We show that this is consistent with all the parameter restrictions. Note first that since $m\in(0,1)$, it must be $\psi(\iota^*)/\alpha<w$, since otherwise it would be $m\geq1$. We therefore have $\psi(\iota^*)/\alpha<w$ and $\psi(\iota^*)/\alpha<mw$. Because $m<1$, the only binding constraint is $\psi(\iota^*)/\alpha<mw$. It is

$$m=\left[\frac{\psi(\iota^*)}{\alpha}\cdot\frac{1}{w}\right]^\alpha$$

which implies $\psi(\iota^*)/\alpha=wm^{1/\alpha}$. Because $m\in(0,1)$, $m^{1/\alpha}<m$ since $\alpha\in(0,1)$, and therefore $\psi(\iota^*)/\alpha=wm^{1/\alpha}<wm$. This implies that the solution is acceptable. Hence, $p(\iota^*)=\psi(\iota^*)/\alpha$ and this concludes the proof. □

Proof of Proposition D.1. Because $w(j) = w$ for all $j \in [0, 1]$, from (D.3b) we get that $e(j)$ does not depend on j and:

$$e(j) = e = w^{-\frac{1}{1-\alpha}}(\alpha A)^{\frac{1}{1-\alpha}} = \frac{L}{1-\iota^*}$$

where the last equality holds by labor market clearing, which requires $(1-\iota^*)e = L$. From Lemma D.1, it is $w = \psi(\iota^*)/(\alpha m^{1/\alpha})$. Plugging this into the previous equation we get

$$\left(\frac{\psi(\iota^*)}{\alpha m^{1/\alpha}}\right)^{-\frac{1}{1-\alpha}}(\alpha\beta)^{\frac{1}{1-\alpha}} = \frac{L}{1-\iota^*}$$

$$\frac{\psi(\iota^*)}{\alpha m^{1/\alpha}}(\alpha\beta)^{-1} = \left(\frac{L}{1-\iota^*}\right)^{-1+\alpha}$$

$$\psi(\iota^*)L^{1-\beta} = (1-\iota^*)^{1-\alpha}\alpha^2 A m^{1/\alpha}$$

Because $\psi'(\cdot) > 0$, the left hand side is strictly increasing in ι^*. Moreover, because $\alpha \in (0, 1)$, the right hand side is strictly decreasing in ι^*. By the Inada conditions, $\lim_{z\uparrow 1}\psi(z) = +\infty$ and $\lim_{z\downarrow 0}\psi(z) = 0$. If $\iota^* = 0$, the right hand side is strictly positive, whereas it is zero if $\iota^* = 1$. Hence, because both are trivially continuous, by the intermediate value theorem there exists at least one ι^* which verifies the equation. Since both are strictly monotone, ι^* is unique. □

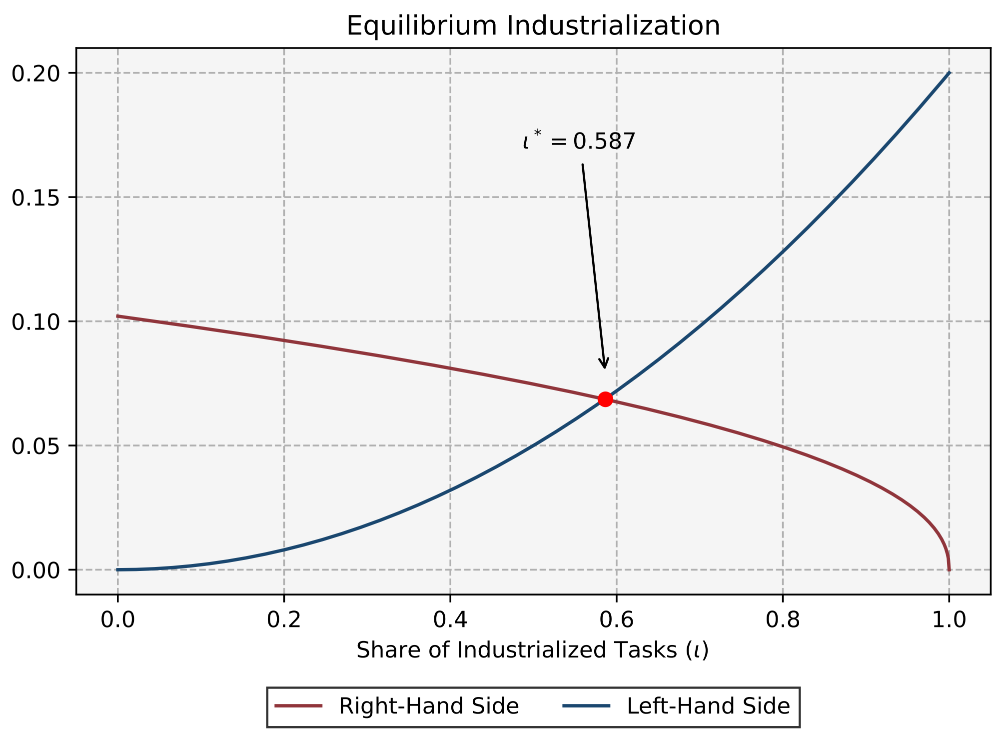


FIGURE D.1: This figure plots the equilibrium of the model. The blue and red lines respectively display the left and right-hand side of the final equation of the proof of Proposition D.1. We assume $\psi(j) = \gamma j^2$ even though quadratic costs do not verify the Inada conditions. Parametrization: $\alpha = .55$, $\beta = .45$, $\gamma = .2$, $A = .5$, $L = 1$, $m = .5$.

Proof of Implication D.1. From Lemma D.1, it is $m^{1/\alpha} = \psi(\iota^*)/(\alpha w)$, or

$$\alpha w m^{1/\alpha} = \psi(\iota^*)$$

Because $\psi'(\cdot) > 0$, an increase in w in the equilibrium implies an increase in $\psi(\iota^*)$, hence in ι^*. $\square$

Proof of Implication D.2. First note that because w is invariant across tasks, then by (D.3b) $e(j) = e$ for all j. Moreover, since the productivity of labor is constant across tasks, it is optimal to divide evenly L across the $(1 - \iota^*)$ non-automatized tasks. Therefore, by labor market clearing $e = L/(1 - \iota^*)$. Plug this in the left-hand side of (D.3b), yielding

$$w^{-\frac{1}{1-\alpha}}(\alpha A)^{\frac{1}{1-\alpha}} = \frac{L}{1-\iota^*}$$

Using Lemma D.1 into the previous equation we get

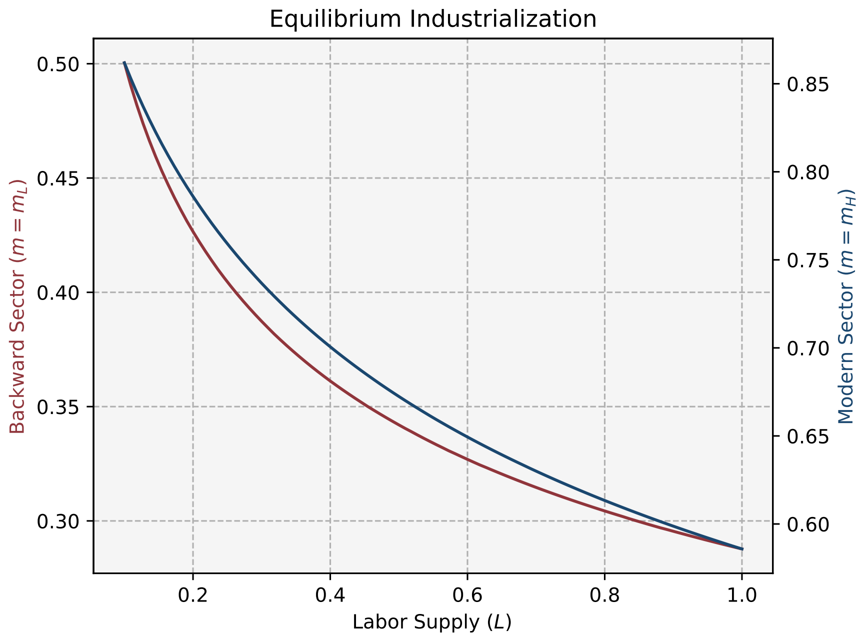
$$\frac{\psi(\iota^*)}{\alpha m^{1/\alpha}} = \left(\frac{L}{1-\iota^*}\right)^{\alpha-1}\alpha A$$
$$L^{1-\alpha} = \frac{(1-\iota^*)^{1-\alpha}}{\psi(\iota^*)}\alpha^2 A m^{1/\alpha}$$

Because $\alpha \in (0, 1)$ and $\psi'(\cdot) > 0$, the right-hand side is decreasing in ι^*. Therefore, an exogenous increase in L leads to an increase in the right-hand side, hence a decrease in ι^*. Following an increase in the labor supply, the share of automatized tasks decreases. $\square$

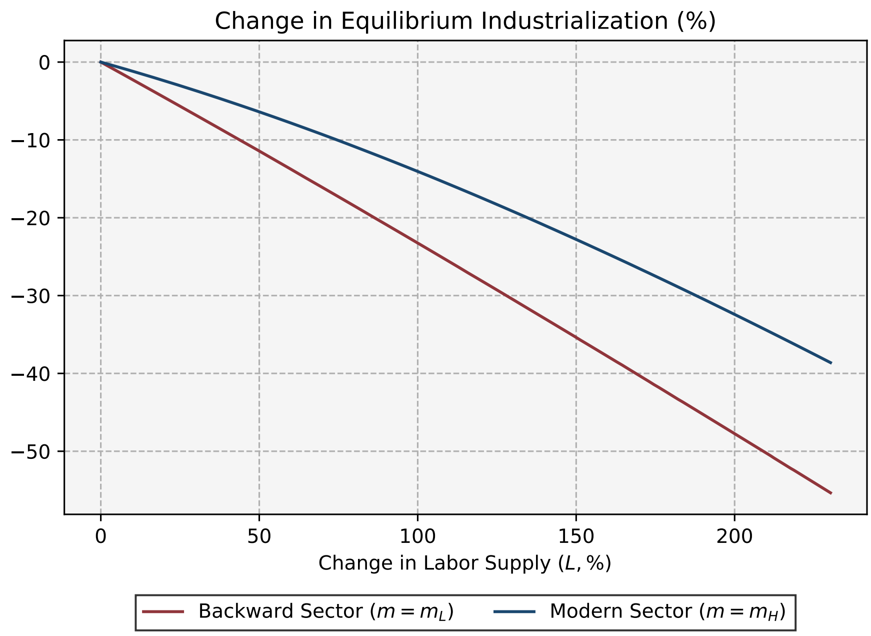
Proof of Implication D.3. Let $m_M > m_B$. From the previous proof, we have

$$\frac{L^{1-\alpha}}{\alpha^2 A m_i^{1/\alpha}} = \frac{(1-\iota^*)^{1-\alpha}}{\psi(\iota^*)}$$

for $i = M, B$. Holding everything else constant, an increase in L translates into an increase in the left-hand side which is smaller if $m = m_M$ than under $m = m_B$ because $m_B, m_M \in (0, 1)$. Therefore, the right-hand side shall increase more under m_B. Hence, the compensating change in ι^* is larger if $m = m_B$, *i.e.* in the relatively backward sector, than if $m = m_M$, *i.e.* in the relatively modern sector. $\square$



(A) Equilibrium industrialization and the labor supply.



(B) Industrialization response to changes in labor supply.

FIGURE D.2: Figures plot the relationship between industrialization and the labor supply. The red and blue lines respectively display the backward and modern sectors. We assume $\psi(j) = \gamma j^2$ even though quadratic costs do not verify the Inada conditions. Parametrization: $\alpha = .55$, $\beta = .45$, $\gamma = .2$, $A = .5$, $L = 1$, $m_H = .5$, $m_L = .2$.

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