The Economic Effects of Immigration Restriction Policies

Evidence from the Italian Mass Migration to the US*

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Abstract

This paper studies how immigration restriction policies influence the development of emigration countries in the context of the early XX-century Italian mass emigration to the US. We assemble a unique dataset spanning the 1890-1930 period to link Italian emigrants to their district of origin and complement it with newly digitized historical Italian census data. To identify the effects of the 1921-1924 Immigration Acts we compare districts with similar emigration rates but different destinations. Districts that were more exposed to the policy shock display a sizable increase in population. We provide evidence that "missing migrants" whose migration was inhibited by the Acts drive this result. We find that investment in capital goods by manufacturing firms in exposed districts sharply decreases. Moreover, industrial employment in those districts increases. We interpret this as evidence of directed technical adoption: more abundant labor dampened the incentive for firms to adopt productivity-enhancing technologies thereby potentially hampering long-run growth.

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

JEL Classification: N14, N34, O15, O33

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1 Introduction

In recent years, immigration has been in the spotlight of public opinion and policy alike. The resulting heated political debate led to increasingly common immigration restriction policies –henceforth, IRPs– and even fostered the rebirth of nativist anti-immigration sentiments (among others, see Guriev & Papaioannou, 2020). Nativist propaganda describes immigration as a threat to economic and cultural security for receiving countries. Dissenting observers and policymakers on the other hand emphasize the beneficial effects of immigration, especially in rapidly ageing developed countries. A unifying approach of this increasingly polarized public debate is that both arguments rely solely on the alleged effects of *immigration*, either them be positive or negative. In other words, political actors in developed countries focus on the potential gains or losses that migrants can bring to their own –receiving– country. This is a shared perspective across several Western countries, irrespective of whether they had historically been receivers or suppliers of migrants. Even though tens of millions of migrants left the Old World in search of better fortunes in the New during the Mass Migration period (1850-1914), similar anti-immigration sentiments in fact recently gained momentum in the U.S. as well as in several European countries.

The effects of *emigration* –and its hetero-imposed restriction– on sending countries receive considerably less attention both in the public sphere as well as in the scholarly literature. The resulting current shortage of evidence documenting the economic consequences of emigration thus precludes a comprehensive understanding of the effects of migrations on receiving as well as on sending countries. This poses a substantial limitation because the effects of emigration on sending countries are *ex ante* ambiguous and possibly conflicting. Emigration can be conducive to economic growth because it can foster human capital accumulation through remittances, return migration or increased returns to schooling (Fernández-Sánchez, 2020; Dustmann *et al.*, 2011; Beine *et al.*, 2008). However, it can also depress the human capital stock if migrants are positively selected from the human capital distribution, thereby hampering development and modernization (Fontana *et al.*, 2020; Kwok & Leland, 1982). Moreover, long-standing albeit seldom empirically tested theories imply that emigration can foster invention and adoption of labor-saving technologies because it makes labor relatively more expensive than capital (Hicks, 1932; Habakkuk, 1962; Acemoglu, 2002). Acemoglu (2007) describes the general framework of directed technical change.

This paper investigates the economic effects of immigration restriction policies in the context of the Age of Mass Migration, the largest episode of voluntary migration in recorded history (Choate, 2008). More specifically, we focus on the archetypal emigration country during this period, Italy. Over the years 1876-1925 approximately 17 million emigrants left Italy out of an average population of

26 millions, approximately half of them never to return.[1] Italy had thus one of the highest emigration rates and, since the 1890s, it was the leader in sheer emigration numbers (Hatton & Williamson, 1998). Out of ten randomly sampled emigrants, throughout this period on average four headed towards the U.S., while the remaining were split between South America and Western Europe. The United States were therefore the largely prevailing single destination of emigration. The Italian mass migration to the U.S. however abruptly ended in 1921-1924 when the Congress passed a series of restrictive IRPs which we collectively refer to as the "Quota Acts". The Quota Acts envisaged numerical quotas for European countries that depended on how many citizens from that country were recorded living in the U.S. in 1910 (later 1890). Since Italy had been a latecomer to mass migrations, this resulted in a very severe emigration restriction (see section 2 for a more detailed historical context). We leverage the differential exposure to this shock across Italian districts to estimate the economic effects of emigration on economic growth. Comparable empirical exercises face two major limitations. First, emigration is seldom concentrated into few destinations, hence it is difficult to observe large restrictive policy shifts. Second, migration dynamics are likely affected by co-evolving regulations enacted by both receiving as well as sending countries. The unique historical setting we analyse allows to overcome these difficulties.

Our empirical strategy relies on different exposure to the Quota Acts across Italian districts. Consider, for the sake of the argument, two districts A and B. Both A and B had high emigration rates. However, most migrants from district A went to the U.S., 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 the Italian mass migration more than home-destination wage gaps (Gould, 1980b).[2] 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. This identification assumption would be met if variation in quota exposure was random across districts conditional on emigration rates. In figure II we plot emigrants as a fraction of the total population, and show that Northern as well as Southern regions experienced varying emigration intensities. By contrast, the share of emigrants heading to the US is prevailing in the *Mezzogiorno*. The bottom panel figure shows that exposure to the US Quota Acts reflects these heterogeneous patterns once we control for the extensive margin of emigration. In this context, it is therefore natural to think of the model as a simple difference-in-differences regression

[1] In the the Online Appendix we report additional data on total and U.S. emigration across Italian regions in this period.

[2] 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 by Hatton & Williamson (1998) who noted that pull, rather than push, factors explain the bulk of variation of the Italian emigration.

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 are not suitable for this exercise because (i) we do not know the origin of Italian migrants at a relatively 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 arrived at Ellis Island between 1892 and 1930 to their district of origin, and complement it with newly digitized detailed data from industrial and population censuses. These data allow us to document three sets of results.

We first show that industrial firms located in districts which were 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 further distinguish between traditional mechanical engines and cutting-edge electrical ones. The electrical engine was in particular a defining technology of the Second Industrial Revolution which could yield sizable productivity gains (David, 1990; Mokyr, 1998). We show that in more exposed districts the adoption of engines slowed down. This effect is particularly strong in magnitude for electrical engines, either them be measured in sheer 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 is consistent with findings by Andersson *et al.* (2020) who showed that emigration fosters the adoption of labor-saving technologies because it prevents excess supply of labor. The effect of the IRP shock was not homogeneous across industrial sectors. We find that firms in "First Industrial Revolution" sectors, such as the textile and construction industries, sharply reduced their investment in capital goods. On the contrary, firms in modern "Second Industrial Revolution" sectors, such as chemicals and steel-working, did not.

To rationalize these findings we advance and validate the hypothesis that IRPs induce a geographically segmented labor supply shock.[3] 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, hence cheaper, labor dampens the incentive for firms to adopt capital-intensive technologies, as we observe. Under this interpretation, in Italy the Quota Acts effectively induced a highly geographically segmented positive labor supply shock. Districts which experienced more emigration until 1924 were more exposed to the quotas because pull factors were disproportionately more effective there.[4] We document that population in these districts grew comparatively more relative to

[3]This approach mirrors that by Abramitzky *et al.* (2019a) who documented the Quota Acts induced a negative labor supply shock in *US* counties whose intensity depended on the prevailing origin of immigrants across European countries.

[4]Several studies documented that emigration location choices tend to persist over time (*e.g.* Gould, 1980b; Brum, 2019;

districts which were less exposed to the Quota Acts. In the Online Appendix, we provide supportive evidence of this mechanism and show that (i) emigrants did not substitute the U.S. with other arrival destinations, and (ii) emigration outflows towards unrestricted countries did not decrease. Hence, districts which had been supplying relatively more U.S. emigrants ended up having more "missing" migrants, *i.e.* people that would have migrated had the Quota Acts not been enacted. This mechanism generates a geographically segmented positive labor supply shock. If our directed technical adoption interpretation was correct, we would expect to observe increased industrial employment in more treated districts, the more so in sectors which experienced the larger drop in investment in capital goods. It should also be noted that Italian emigrants to the U.S. during this period were not skilled workers. In fact, Sequeira *et al.* (2020) show that second-wave emigration countries, among them Italy, mainly supplied unskilled labor. Hence, in this paper we largely abstract from the human capital channel –the "brain drain"– extensively studied in the literature. Instead, we explore the mechanics of a local labor supply shock on economic development and technology adoption.[5]

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 largely focus on the two biggest sectors at the time, agriculture and manufacture. We find that employment in agriculture did not react to the labor supply shock, whereas the number of workers employed in the manufacture sector considerably grew. This is fully consistent with directed technical adoption: firms in manufacture substituted capital goods with more abundant, therefore cheaper, labor provided by missing migrants. Because industrial employment grew and agricultural employment did not, the overall share of workers engaged in industrial undertakings increased. To further validate our proposed mechanism, we then zoom into specific sectors within manufacture to study whether employment dynamics are consistent with the heterogeneity we find in capital goods investment patterns. Our results suggest that sectors which experienced the largest drop in investment in capital goods, namely textile and construction, were also those that absorbed most of the enlarged labor supply. We similarly find no evidence that more modern sectors, such as steel-working and chemicals, substituted capital for labor. Because the latter did not undergo any reduction in investment in capital goods, this is fully consistent with directed technical adoption. Since during this period manufacture was the driving force of economic growth, our results suggest that the Quota Acts possibly contributed to the modernization of the Italian economy because they pushed workers into manufacture rather than agriculture. This notwithstanding, it is well known that technology adoption is a key driver of long-run

Fontana *et al.*, 2020; Spitzer & Zimran, 2020).

[5] Spitzer & Zimran (2018) provided biometric evidence suggesting that Italian emigrants were nation-wide negatively selected, but positively so within the South. However, we find no evidence that districts with historically higher emigration rates had lower literacy rates, hence it is unlikely that emigrant selection operated through human capital.

growth (*e.g.* Juhász *et al.*, 2020). Therefore, the Quota Acts –as virtually every IRP– could have very well threatened long-run economic development because they altered the incentives for firms to invest in capital-intensive productivity-enhancing technologies.

Identification, and therefore a causal interpretation of our estimates, may fail if, despite dissenting evidence by Spitzer & Zimran (2020), conditional variation in U.S. emigration rates was still systematically correlated with economic performance. While this assumption cannot be tested, we run two main robustness checks that provide supporting evidence. First, we provide additional estimates controlling for the emigration rate. Controlling for the extensive margin of emigration ensures that we do not compare high *vis-à-vis* low emigration districts. Because emigration may correlate with underlying economic outcomes at the district level, this would weaken identification and threaten the consistency of our estimates. Our baseline results are confirmed under this tighter identification scheme. In the second validation exercise, we develop an instrumental variable (IV) along the lines of Tabellini (2020). This allows to fix the cross-sectional variation in emigrants origin to a given –early– point in time, and predict a district's emigration using the time-series variation in aggregate outflows dropping emigrants from that district. We thus wash out all variation due to idiosyncratic economic performance which we cannot control for using time fixed-effects. The results of this exercise confirm our baseline estimates.

This paper complements and expands, but equally benefits from, three streams of literature. We first 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.[6] This literature identifies human capital accumulation as the key driver of economic growth fostered by emigration, either it be fuelled by return migrants or increased returns to schooling (Dinkelman & Mariotti, 2016; Dustmann *et al.*, 2011; Beine *et al.*, 2008). In XIX-century Galicia, emigration in the short-run depressed human capital. However, in the long-run it nonetheless exerted a positive overall effect (Fernández-Sánchez, 2020). This paper informs this literature of a novel mechanism whereby emigration fosters adoption of labor-saving technologies, hence proving unambiguously beneficial for economic development. We emphasize that this channel operates plausibly independently from human capital accumulation. The second stream of literature this paper relates to studies the relationship between technology adoption and the supply of production inputs. Besides path-breaking contributions by Hicks (1932) and Habakkuk (1962), Hornbeck & Naidu (2014) show that the 1927 Great Mississippi Flood induced higher technology adoption in more severely hit counties due to higher unskilled emigration rates depressing labor supply. Clemens *et al.* (2018) document that immigration barriers

[6] Borjas (1995, 2014) produced two influential reviews of this literature. Abramitzky & Boustan (2017) surveyed papers studying the historical and contemporary US immigration. Hatton & Williamson (2005) and Ferrie & Hatton (2014) provided two complementary works studying the role of immigration from a global economic history standpoint. Clemens (2011) instead surveyed the literature studying the effects of emigration, hence on sending countries.

imposed through the ending of the *bracero* program induced adoption of labor-saving innovations in farms and did not benefit natives' employment. Hanlon (2015) studies the Lancaster cotton famine and shows that scarcity of a production input –Confederate cotton– induced directed technical change to make up for imperfectly substitute available inputs. In a modern setting, Lewis (2011) shows that immigration of low and middle skilled workers in the 1980s and 1990s induced U.S. manufacture firms to adopt less machinery per unit of output. The paper ours is closest to is that by Andersson *et al.* (2020) studying the interplay between emigration and technological change in XIX-century Sweden. Relative to their contribution, we emphasize the labor supply shock mechanism and study how emigration restriction policies can hamper economic development of sending countries in a difference-in-differences framework. We do not however cover innovation because Italy performed poorly throughout this period (Vasta, 1999; Nuvolari & Vasta, 2015). By virtue of its setting, the present paper is finally related to the large and growing literature investigating the exceptionally broad social phenomenon represented by the Age of Mass Migrations (for a review, see Abramitzky & Boustan, 2017). Our baseline empirical strategy owes to the approach pioneered by Abramitzky *et al.* (2019a) who leverage differential exposure to the Quota Acts to study how labor scarcity impacted earnings and capital investment. Tabellini (2020) studies the interaction between economic and political consequences of immigration and develops the IV we adapt to our setting, whereas Sequeira *et al.* (2020) especially focus on the long-run effects of immigration. Karadja & Prawitz (2019) document that emigration fostered demand for political change in Sweden, and empowered change too. Coming 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 in shaping Italian emigration dynamics. Brum (2019) further shows that the location choice and origin of pioneers explains a large share of the variation in emigration patterns across Italian municipalities. Spitzer *et al.* (2020) prove that the deadly Messina earthquake had little impact on emigration from even highly disaggregated communities. Our contributions to this literature are several. In terms of methodology, we build the first highly comprehensive geographically disaggregated dataset of Italian emigrants during the years of the bulk of the Italian mass migration (1900-1914). We also digitize new detailed district-level data from population and industrial censuses. Substantially, we show that the spectacular outflow of unskilled labor leaving Europe towards the Americas unlikely hampered economic growth, even at the periphery of the (slowly) industrializing Old world. The opposite impact probably prevailed, and it was actually emigration *restriction* which threatened economic and social modernization in Italy.

We structure the paper as follows. Section 2 describes in some detail the features of the Italian mass migration, the policies which shaped it, and the key economic characteristics of early XX-century Italy. Section 3 presents a simple theoretical framework to think of directed technical change and highlight the empirical implications of the theory which we test. In section 4 we discuss our data collection exercise and our sources. In section 5 we clarify our empirical strategy, and present the three sets of results we mentioned hitherto. Section 6 summarizes the key robustness checks and the IV exercise; section 7 finally wraps the results up and concludes.

2 Historical Context

2.1 The Italian Mass Migration

The Italian mass emigration (1870-1925) was the largest episode of voluntary migration in recorded history (Choate, 2008). In 1900, the population of the Kingdom of Italy amounted to approximately 26 million people. Between 1880 and 1913 half of it left, mostly heading towards continental Europe and the Americas. Along with the Irish, Italians had the highest per capita emigration rates (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). In fact, Gould (1980a) vividly describes late-nineteenth century Italy as the archetypal case of mass migration.

Italy was a latecomer to large-scale mass migration. Northern European countries had been undergoing substantial population outflows since the 1840s. By contrast Italy, along with other Southern and Eastern European countries, started experiencing mass emigration in the 1880s. Migration patterns over the 1870-1925 period display substantial time variation. Until the 1880s, emigration rates remained relatively modest and the bulk of migrants came from northern regions. Prohibitively high transportation costs and prevailing poverty in southern rural areas largely inhibited migrations from the *Mezzogiorno*. During this period, northerners chiefly moved to neighbouring countries on a temporary seasonal basis (Sori, 1979). The widespread adoption of steamships and an agrarian crisis kicked off the southern mass emigration starting in the early 1890s (Keeling, 1999). By the early 1900s the absolute majority of migrants was coming from southern regions, thereby attesting the spectacular rise in emigration rates in those areas. Even though the share of migrants from northern regions declined as that from southern regions grew, emigration rates from both areas steadily *increased* throughout the 1870-1913 period (Hatton & Williamson, 1998, p. 100). By 1890s, Italy had become the global leader in sheer numbers of emigrants and emigration rates. The only country whose emigration rates were comparable with Italy's during the Age of Mass Migration was Ireland. While the Irish emigration rates nonetheless declined over time, the Italian rates sharply grew from 5‰ in 1880, to a peak of 25‰ in 1913 (Hatton & Williamson, 1998, p. 95). Emigration collapsed during World War 1 (WW1) but quickly regained momentum in the early 1920s. The restrictive immigration policies enacted by the U.S. Congress in 1921-1924 (see below) effectively halted mass emigration to the U.S.. Emigration towards 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 as well as economic 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. The lion's share of the remaining minority heading towards transoceanic destinations went to Argentina and Brazil. This pattern is completely reversed in the south, where the largely prevailing destination was the United States. The share of migrants moving to the U.S. substantially increased over time in each region. By the 1910s the U.S. was the preferred transoceanic destination throughout Italy, even though northern migrants still tended to prefer continental European destination over overseas ones. To explain why destinations carrying low relative wage gap 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 places and diffusion accelerated as the volume of emigration increased. This process implied that emigration from different localities followed a "S" curve whereby emigration started low, increased at increasing pace until eventually levelling off at saturation. Gould (1980b) provides convincing evidence suggesting that declining regional emigration rates 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, while featuring high persistence instead (Hatton & Williamson, 1998, p. 99). Gould's hypothesis further strengthens our identification scheme. In fact, we leverage differential exposure of Italian counties to the U.S. Immigration Act 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.[7]

The average Italian migrant during the Age of Mass Migration was young, male and single. Over half migrants were aged 15-34, and only one in four travelled with family. Unlike Scandinavian and German migrants, Italians were predominantly men (80%) (Hatton & Williamson, 1998, p. 102). Italian emigrants were typically unskilled agricultural workers. In the U.S., Italian emigration was part of the "second-wave" of immigration mostly coming from southern and eastern Europe. Compared to first-wave countries such as England and Germany, poorer second-wave ones tended to supply less educated and skilled migrants, who experienced harder living conditions and slower assimilation and economic catch-up with the natives (Abramitzky & Boustan, 2017; Daniels, 2002, p. 121). Since we

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

exploit a migration policy shift to assess the impact of emigration on economic development, potential endogenous selection of migrants may be relevant for our results.[8] 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 during the Age of Mass Migration in Italy is internal migrations. Current data limitations hinder a quantitative study of internal migrations over the years 1870-1925. In the remainder of this study we abstract from explicitly accounting for internal migrations for a number of reasons beyond data availability. First, Gallo (2012) shows that internal migrants were easily outnumbered by international migration flows, the more so during the Age of Mass Migration. Second, internal mobility was by far and large temporary and seasonal, hence inherently different from transoceanic migration (Gallo, 2012, p. 32). Last, internal migrations reflected historically deep-rooted and persistent economic relationships between regions which are unlikely to influence our results on economic modernization in the 1930s (Gallo, 2012, p. 38).

2.2 Migration Policy in Italy and in the U.S.

The newly unified Italy had virtually no emigration policy until 1873. Occasional and largely ineffective provisions were enacted over the 1873-1887 period. These reflected the perceived need to deal with labor agents and recruiters, the so called *padroni*, but did not form a comprehensive corpus of migration law (Gabaccia, 2013, p. 55). The first such attempt was the 1888 Crispi-De Zerbi law which introduced and regulated the contract of emigration between the migrant and the migration agency. This regulatory attempt was nonetheless manifestly inadequate to deal with the mass migration which unfolded starting in the 1890s. The Crispi-De Zerbi law regarded emigration as an artificial phenomenon instigated by migration agencies and consequently attempted to centralize its governance. Apart from a small measure to control ticket fares, it effectively failed (Foerster, 1924, p. 477).

Emigration was not curbed by the 1888 law. Instead, its spectacular increase forcefully persuaded Italian policymakers that it was more likely to make laws, rather than abide them (Foerster, 1924, p. 475). The 1901 emigration law was thus passed under the renewed understanding that emigration was no artificial phenomenon, and that it could bear positive effects for Italy. As such, the law did not restrict emigration but rather sought to protect migrants from exploitation. The law envisaged the establishment of 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,

[8] 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 bias downward our estimated impact of migration on economic development.

whose rates were set 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 the period of mass emigration Italy either failed at enforcing emigration restrictions, or abstained from undertaking such an endeavor (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 imply that emigration featured as a first-order dimension of the Italian economic and social development process.

The United States alike maintained an open border between 1775 and the early 1920s, albeit shortly interrupted by isolated outbreaks of anti-immigration policy interventions. During the Age of Mass Migration, some 30 million migrants entered the U.S.. By 1910, 22 percent 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.[9] In 1895 a bill was introduced by Henry Cabot Lodge requiring a literacy test to be administered to each immigrant upon arrival. Though the Congress voted the bill, 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 was eventually passed in 1917, although it was largely ineffective because of the rising literacy rates in Europe (Goldin, 1994).

In 1907 a joint U.S. Immigration Commission, also known as Dillingham commission after its chairman, was formed to study the economic and social conditions of immigrants. The report produced by the Commission favored 'old' immigration countries such as England and Germany over 'new', mainly Southern and Eastern European ones. The Commission noted that because immigration from second-wave countries displayed higher return rates, migrants were less likely to assimilate (Higham, 1955). The Dillingham report was highly influential and shaped subsequent migration policy interventions. When immigration revamped after WW1, demand for restrictions surged and an Emergency Quota Act was passed in 1921. The 1921 Act was modified by the 1924 Immigration Act which further tightened immigration restrictions for countries of new emigration to the U.S..

The 1921 Emergency Quota Act envisaged a (temporary) annual quota of 360,000 immigrants from Europe.[10] Importantly for our identification, entry quotas were assigned to each country as 3% of that country's nationals living in the U.S. 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 percent and shifted

[9]The Chinese Exclusion Act built on the 1875 Page Act which banned Chinese women immigration. To date, these are the only U.S. laws explicitly targeting one ethnic group.

[10]In 1907 a peak of 1,285,349 migrants entered the U.S.. On average, entrants during the 1910s were around 800,000.

the census baseline year to 1890. The latter provision in particular disadvantaged countries newer to mass migrations and was thus 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 who came from those countries and lived in the U.S. in 1890 was extremely small. By contrast, the quotas assigned to Northern and Western European countries were comparatively generous. For the purpose of this paper it suffices to note that the 1921-1924 Acts (henceforth Quota Acts) effectively halted the Italian mass migration to the U.S.. Since the 1890s the U.S. had been absorbing 30 to 40 percent of the total Italian emigration. Hence, the Quota Acts represented a major policy shock for Italy whose effects this paper seeks to uncover.

2.3 Technology Adoption and Economic Growth in Italy

Italy entered the Age of Mass Migrations in the 1880s, following two decades of stagnation and in the midst of an agrarian crisis (Toniolo, 2014, pp. 60-73). The 1895-1913 years, known to Italian economists as the *boom giolittiano* were the only period until the 1950s "economic miracle" when Italy managed to outperform and narrow the income gap with the leading industrial nations. Still in the the 1920s and 1930s, during the Fascist period, Italy was a mainly agricultural country featuring low income per capita levels and stagnating productivity (Cohen & Federico, 2001, p. 23). During the first half of the *Ventennio* economic policy primarily aimed at fiscal and monetary consolidation. Agricultural policy formed an integral part of the Fascist propaganda, yet sheer numbers attest no "Battle", either it be for the wheat or for the land, resulted in neither substantial intervention nor sizeable progress (Zamagni, 1990, p. 262). All in all, growth slowed down after 1925 and regional disparities further widened (Cohen & Federico, 2001, p. 15). Historical evidence is thus consistent with our finding that following the forced emigration restriction in 1921-1924, Italy underwent a period of economic distress and rising regional inequality.

The second set of results we relate the migration shock to is diminished investment in capital goods, especially technologically advanced ones, and a shift to labor-intensive production routines. Italy was hardly at 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 industrial nations in terms of research and development expenditures and imported substantial amounts of foreign technology, both as 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 moreover made it more profitable for Italian entrepreneurs to adopt labor-intensive technologies relative to the highly capital-intensive German and British ones. Consistently with this narrative, we find that the migration policy shock increased the stock of unskilled workers in regions of 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 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.

3.1 The Model

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:

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

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

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

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

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

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

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

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

The economic intuition behind condition (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 (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.[11] 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

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

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

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

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

[11] 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.

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 the Online Appendix.

Lemma 3.1. *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 3.1. *There exists one and only one $\iota^* \in [0,1]$ which solves the problem of the final good producer* (3a)-(3b)-(4) *as well as the problem of the machine producers* (6) *and verifies labor market clearing. In particular, the equilibrium industrialization ι^* is the solution to the following:*

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

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.

3.2 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 3.1 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 3.1. 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 (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 3.2. 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 5.2– and increased employment – section 5.4. These findings are fully in line with the empirical predictions 3.2 of the model and hence provide evidence in favor of labor-saving directed technical adoption.

Implications 3.1-3.2 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 3.3. Let H 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 3.3.

4 Data

Our analysis spans the years 1881-1936. We collect data from a number of sources, however we stack them at census years and conduct the analysis at the *circondario* (henceforth district) level of aggregation.[12] In 1921 there were 216 districts each consisting of a variable number of municipalities (see the Online Appendix 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 of the variables in our final dataset.

[12]Population censuses were taken in 1881, 1901, 1911, 1921, 1931 and 1936. We do not include data prior to 1901 in our baseline analysis. In the Online Appendix we show that if we interpolate data for the 1891 census, we can validate the parallel trends assumption which is implicit in our difference-in-differences models.

4.1 Emigration

Italian official emigration statistics are of somehow dubious reliability and limited scope. We call for caution on reliability because gross emigration flows were measured based on issued passports. However, passport regulations changed substantially over this period. Passports were not obligatory before 1901 and a fee was applied, but they became free and mandatory afterwards (Hatton & Williamson, 1998, p. 98). Moreover, the most granular observation level is the *provincia* (province). Province-level data are not suited for a quantitative analysis because provinces were relatively large: in 1921 there were only 60 provinces over a population of approximately 20 million. This naturally limits the use of official statistics for an econometric exercise.

To overcome these issues and study the Italian mass migration to the U.S., we collect administrative records of nearly all Italians who entered the U.S. between 1892 and 1930 through the Ellis Island immigration station.[13] This was by far the largest, although not unique, immigration gateway throughout this period.[14] Administrative records report, for the vast majority of migrants, name and surname, year, age, municipality of origin and sailing ship. In this study we concentrate on the migration year and the municipality of origin. Ultimately, we collect approximately 2,7 millions individual observations spanning the years 1890-1930.

Because all data were recorded by U.S. officials, the municipality variable displays frequent coding errors. We adapt the matching procedure by Abramitzky *et al.* (2014) and use a sound-spelling similarity metric to account for orthographic and misspelling errors (see the Online Appendix for a discussion of the methodology). We then set a threshold measure below which we accept the best matched municipality, and above which we do not and drop the observation, and run robustness checks around this threshold. In our preferred specification, we are able to match 1,6 million migrants to their origin municipality. Among those, 800,000 are coded with no error. We map each municipality to the district it belonged to in 1921, and compute 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.[15] In figure I we plot the overall country-level yearly inflow of emigrants landed in Ellis Island over the years 1890-1930. Emigration took off in the mid 1890s and peaked between 1905 and

[13] These records are freely available at `heritage.statueofliberty.org`. We run queries over a comprehensive pool of 20,000 Italian surnames over the 1890-1930 period.

[14] According to U.S. official statistics, between 1892 and 1924 a total of 14,277,144 migrants entered the U.S. 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 the Ellis Island station.

[15] 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).

1913. It collapsed during WW1, quickly regained momentum in 1920 and was definitely 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 (Sequeira *et al.*, 2020; Brum, 2019). In figures 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 US 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.

4.2 Population

We digitize information from the 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. For the 1931-1936 census we digitize them at the municipality level and aggregate at the district level. More detailed data are, unfortunately, only available until 1921 for the manufacture sectors. For the remaining years, we digitize the same variables from the manufacture census, 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 is compiled by the Italian statistical office (ISTAT). We compute 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.

4.3 Economic Activity

To measure shifts in the adoption of capital-intensive technology, we digitize province-level data from the 1911, 1927 and 1937 manufacture censuses. Manufacture censuses gathered information on the universe of firms operating in each province at the time of census completion and 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, as well as (vi) that generated by electrical ones. We distinguish between electrical and mechanical engines because the former were at the forefront of technical 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 only available at the province level. To impute them to districts, we regress (i)-(vi) against the number of workers in each aggregate sector, population and a set of year and province dummies.

From the resulting OLS fit we compute the district-level variables using the same explanatory variables at the district level. In the Online Appendix we show province-level conditional correlations with the number of industrial workers and OLS results.

5 Results

5.1 Empirical Strategy

The 1921-1924 Immigration Acts imposed a severe emigration restriction on Italy. We seek to measure their impact on the Italian economic development. Our identification relies on geographic variation in emigration patterns and intensity across districts in the pre-quota period.[16] Consider for the sake of our argument two ideal districts, call them A and B. Over the 1890-1924 years, many emigrated from both district A and B. However, most emigrants from district A headed towards the U.S., 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 acted as a powerful pull factor that exerted influence on potential emigrants through previous generations emigrants (Spitzer & Zimran, 2020). This induced substantial persistence in emigration patterns by country of destination. Districts which had experienced higher emigration towards the U.S. before the Quotas were therefore comparatively more exposed to the migration restriction shock, relative to those whose emigration outflow headed mainly toward European and South American countries.

Reality was more nuanced than our example. Emigrants came from all districts, and headed to all destinations, hence the intensity of quota exposure varies smoothly with respect to U.S. relative emigrant outflows. Importantly, existing dispersion of U.S. emigrants by district of origin shown in figure II ensures that the intensity of exposure to the quotas stems from plausibly exogenous variation in marginal emigration patterns. 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.[17] Our identification assumption thus relies on the key assumption that districts with similar relative outflow emigration but 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 the Online Appendix we thus report results comparing estimates obtained using all districts, as well as restricting

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

[17]In section 6.2 we present a simple instrumental variable that further addresses the possible residual correlation between intensity of quota exposure and economic performance of districts.

the sample to only Southern ones.

We measure quota exposure of district c in region r as

$$\text{QE}_{cr} = \frac{1}{\text{Population}_{cr,1880}} \sum_{t=1890}^{1924} \text{Emigrants}_{cr,t}^{U.S.} = \frac{\text{Emigrants}_{cr}^{U.S.}}{\text{Population}_{cr,1880}} \tag{7}$$

where Population_{1880} is the population of district c in 1880, and $\text{Emigrants}_{cr,t}^{U.S.}$ is the number of emigrants who headed to the U.S. over the period. Since mass outmigration started in the 1880s, in equation (7) 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 (7) can be further decomposed as

$$\text{QE}_{cr} = \underbrace{\frac{\text{Emigrants}_{cr}^{U.S.}}{\text{Emigrants}_{cr}}}_{\text{Intensive margin} \equiv \text{IM}_{cr}} \times \underbrace{\frac{\text{Emigrants}_{cr}}{\text{Population}_{cr,1880}}}_{\text{Extensive margin} \equiv \text{EM}_{cr}} \tag{8}$$

where Emigrants_{cr} is the total number of emigrants. The intensive margin (IM) of exposure measures the relative importance of U.S. 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 towards the U.S.. By contrast, districts with both little overall and U.S. 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 rate 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. We construct a measure for EM using province-level data of total emigration available in the census, and assume constant emigration rates within each province. 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 (7) serves as our baseline treatment. We stack data at census years. Throughout the remainder of the paper we estimate variations on the following difference-in-differences (DiD) model:

$$y_{cr,t} = \gamma_c + \gamma_t + \boldsymbol{x}_{cr,t}\boldsymbol{\beta} + \delta_1 \left(\text{EM}_{cr} \times \text{Post}_t\right) + \delta_2 \left(\text{QE}_{cr} \times \text{Post}_t\right) + \varepsilon_{cr,t} \tag{9}$$

where y is the log-difference of one of many outcomes, $\boldsymbol{x}$ is a vector of additional –optional– controls, and Post_t is an indicator that is equal to one only if $t > 1924$. 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. In the event-studies we run in the Online Appendix we show that results hold if we condition on the interaction between several indicator of economic performance before the Quotas and time dummies. 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. Since we collect data from the 1901, 1911, 1921, 1931 and 1936 population censuses, we have two pre-treatment and two post-period ones. For the sake of model (9), we follow Abramitzky *et al.* (2019a) and collapse them to single pre- and post-treatment periods. 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 more in detail in section 6 we control for variation due to WW1, measurement errors in the years following the Quotas due to changes in registration procedures at Ellis Island, as well as possible correlation between QE and the error term.

Causal inference on estimates of model (9) requires that treatment and control groups were on the same trend before the treatment, *i.e.* the Quota Acts, occurred. Because no census in 1891 was taken to test the parallel trends assumption we need to interpolate data points between 1881 and 1891. The results of this limited exercise are shown in the Online Appendix for all regressions and provide convincing evidence in favor of the parallel trends assumption. 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 1901 and 1911. This exercise is not ideal in that we cannot clean for year fixed effects but nonetheless strongly suggests that 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.

5.2 Emigration and Technology Adoption

We first study how technology adoption and investment in capital goods by firms in manufacture responded to the IRP shock. To do that, we collect several proxies for capital investment from the manufacture census, and report estimates of model (9) for these various outcomes. Our baseline measures of investment in capital goods are the number and installed horsepower capacity of engines. 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). We therefore interpret investment in electrical engine as a proxy for advanced technology adoption, a key driver of long-run economic growth (Juhász *et al.*, 2020). At the turn of the XX century, U.S. observers were evocatively describing the period as the "Age of Electricity". In 1900 horsepower produced by electrical engines accounted for a mere 5 percent of the overall consumption for production purposes. Two decades after, this had risen to 50 percent (David, 1990). Even 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 in the 1930s electricity represented a major source of potential productivity growth. Despite the large productivity gains, adoption of electrical engines was slow. Capital stocks in the early phase of adoption were a patchwork of different engine vintages. All these implied that, in the U.S., capital-per-worker increased following the introduction of electrical engines (David, 1990). We document a different pattern in Italy in the aftermath of the emigration restriction shock.

Table III reports the baseline results. We employ six outcome variables to measure investment in capital goods and technology adoption, and estimate the causal impact of the Quota Acts in model (9) controlling for the extensive emigration margin, population, labor market slackness and district and year fixed effects. Moving from left to right in the table columns these are the total number of firms, the number of firms with at least one engine of whatever 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 in model (9). We find that investment in mechanical and electrical engines alike declined substantially in more exposed districts, either it be 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. A 1% increase in QE reduces mechanical horsepower by 0.7%, while the effect on electrical horsepower is two times as much. Results are qualitatively unchanged if we restrict the estimation sample to Southern regions.[18]

To rationalize this finding we build on Andersson *et al.* (2020) 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 and argue that the Quota Acts, and IRPs more broadly, induced a geographically segmented positive labor supply shock. Districts which before the Acts had experienced high U.S. 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 contrary, districts whose emigrants headed towards destinations other than the U.S. 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 incentivized to decrease investment in capital goods and substitute capital with labor, which had become a more abundant production input following the IRP-induced shock. We devote the rest of the paper to validate this hypothesis.

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

An obvious corollary of this hypothesis is that production technologies in more treated districts should become more labor-intensive. We assess this in table IV. To measure labor-intensity in production we calculate the ratio between the number of workers employed in industry and 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 generate horsepower. In terms of magnitude, the effect of the IRP is comparable across vintages as a 1% increase in QE leads to a 0.25% increase in the worker-to-capital ratio for both electrical and mechanical engines.

Finally, we ask whether the effects of the IRP shock are evenly distributed across industrial sectors. We thus repeat the exercise of table III for each sector recorded by the manufacture census.[19] 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 such as textiles and construction reduced investment in capital goods. This effect is stronger for more advanced electrical engines. On the contrary, we find that firms in modern sectors, such as chemicals and metallurgy, did not undergo any such drop in capital investment and adoption of electrical engines. 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 those sectors were apparently eager to substitute capital for newly available labor. We evaluate this prediction in section 5.4.

5.3 Emigration and Population Growth

We now 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 upon the country of origin of emigrants. We thus estimate model (9) setting population growth as the outcome variable and report the resulting estimates in table V. We compare the estimates obtained from the baseline continuous treatment, as well as those with a 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 to Southern districts only, where the

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

exclusion restriction is sharper. We view this result as confirming that our measure of quota exposure is sound. Districts with larger outstanding U.S. emigrants stocks experienced less emigration, which triggered higher population growth in the years following the Quotas. While studying the precise mechanism driving this result is beyond the purpose of the present paper, this is consistent with pull factors, such as social networks through information diffusion, exerting better influence in more exposed districts. Importantly, significance and magnitude of the DiD coefficient δ_2 increase once we control for the extensive margins of U.S. emigration.

Implicitly, table V provides evidence against mechanisms which 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. migrants substituted U.S. emigration with either (i) emigration to unrestricted countries, or (ii) internal emigration. In the Online Appendix we provide anecdotal 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.

Identification in a difference-in-differences model relies on a parallel trends assumption. In the Online Appendix we show the results of an event-study design providing evidence supporting this assumption. We estimate model (9), except that we interact the treatment measure QE with census-decade dummies instead of conflating pre- and post-treatment periods in two. The figures then plot the estimated coefficient of these interactions for each census decade. Under the parallel trends assumption we expect all coefficients before the treatment period not to be statistically significantly different from zero. This ensures pre-treatment comparability across treatment and control districts.

5.4 Emigration and Industrialization

In the previous section we provided 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, at least potentially, take on industrial jobs. First, Italian emigrants in the U.S. were by far and large unskilled workers who took low qualification jobs in manufacture (Abramitzky & Boustan, 2017). Second, Italian firms during this period were reliant 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 demand by firms. To test this, we estimate model (9) taking as outcome variables changes in the number of workers employed in agriculture and manufacture, as well as changes in the share of workers employed in both sectors as a fraction of the overall employed.[20] As an alternative measure for broader modernization we use the urbanization rate, calculated as the share of citizens living in municipalities with more than 10,000 inhabitants.[21]

In table VI we show that while agricultural employment did not significantly react to the Quota Acts, industrial employment increased substantially. This effect is consistent with evidence presented in table III which documents that firms in *manufacture* decreased investment in capital goods following the IRP shock. Taken together, these results suggest that manufacture 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 agriculture workers as the main outcome variable. Because overall employment hardly reacts to the Quota Acts, industrial employment grows and agriculture employment does not, the share of workers employed in manufacture increased. By contrast, 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 for they pushed comparatively more workers into modern industrial sectors. This notwithstanding, in the last column we report that urbanization declined in exposed districts, thereby suggesting an ambiguous effect on modernization.

In figure III we documented that there is sizable heterogeneity in capital investment and technology adoption decisions across sectors. We now ask whether the directed technical adoption mechanism allows to reconcile these dynamics with changes in sector-level industrial employment. We therefore estimate the baseline difference-in-differences model for the six sectors whose employment was collected in the population and manufacture censuses. The outcome variable in each regression is the growth rate in sector employment, and we control for aggregate manufacture 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 contrary, firms in First Industrial Revolution sectors –chiefly textiles, but construction as well– increased their labor stock. We find no evidence that employment in the two distinctively Second Industrial Revolution sectors, namely chemicals and metallurgy, reacted to the IRP shock. These results

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

[21]Urbanization has been widely used as a proxy for economic modernization, among others see Boustan *et al.* (2018) and Sequeira *et al.* (2020).

are entirely consistent with evidence reported in figure III. Our results suggest that, faced with more abundant unskilled labor, firms in textile and construction substituted capital with labor, hence increasing employment and cutting capital goods investment. By contrast, industrial firms in the agriculture sector reduced their overall labor stock and increased investment in capital goods. Higher value-added sectors did not respond to the labor supply shock, hence maintaining their employment stock and capital investment unchanged. All the above are consistent with the baseline directed technical adoption narrative, and therefore provide evidence in favor of our proposed mechanism.

5.5 Discussion and Alternative Mechanisms

We have documented that the Quota Acts, arguably one of the most sudden and restrictive immigration restriction policy 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 more exposed districts experienced a larger positive labor supply shock induced by the IRP. Throughout the paper, we interpreted these evidence through the lenses of directed technical change and adoption theory. This section discusses some alternative mechanisms which could be compatible with our findings, and interesting additions which are unfeasible due to data limitations. 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 (2020) suggests that Italian emigrants to the US were positively selected within Southern regions. This implies 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 we estimate might be driven by substitutability between the upper-tail of the skill distribution of workers and capital goods, rather than 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 consisted in traditionally low-skilled and labor-intensive manufactures, especially in southern regions (A'Hearn, 1998). Hence it is unlikely that high-skilled workers might be comparatively more productive there. Second, we run a battery of robustness analysis (see Online Appendix) where we include the literacy rate as a proxy for average human capital in our regressions and confirm that results hold nevertheless.

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-to-third order effect on economic growth in sending countries if compared to the welfare effects of immigration restriction barriers. This paper builds on this insight and we consequently abstracted from including remittances in our analysis, the more so given that existing data are of at best questionable reliability. Remittances dynamics nonetheless represent a competing mechanism. More exposed districts were receiving most remittances before the Quota Acts, and hence suffered the most from the border closure. Inasmuch intra-household cash transfer 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 human –rather than physical– capital investment (for a review, see Yang, 2011). A more sensible interpretation could be that remittances fostered literacy (*e.g.* Fernández-Sánchez, 2020; Dinkelman & Mariotti, 2016). Exposed districts would have thus suffered from a relative drop in skilled workers following the Acts, hence the labor force would reshuffle towards unskilled sectors. This would thus move in the opposite direction of the reversed brain drain effect. Under this interpretation, however, 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 trigger 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 US with other –internal or international– unrestricted destinations. Our main argument against this interpretation is provided by evidence in table V. If emigrants substituted the US with other destinations we would expect no effect of exposure to the Quota Acts on population growth. Given the persistence of demographic dynamics, we cannot think of an alternative explanation for such a sharp and sizable increase that is correlated with the conditionally exogenous variation we exploit. Disaggregated emigration data towards countries other than the US does not currently exist. However, in the Online Appendix we report aggregate outflows towards the four main emigration destinations, before and after the treatment period(s). We show that the US are the only country where emigration significantly departs from its historical level, except during WW1.[22] Moreover, the sheer numbers of internal migrations cannot account for the drop in US out-migration (Gallo, 2012). In no southern region did the gross outflow to northern regions in the decade 1921-1931 exceed 5% of US emigrants in the decade 1910-1920. Qualitative and quantitative evidence alike therefore call to dismiss the emigration substitution argument.

Data limitations prevent us from studying two potentially interesting additional variables, namely

[22]These four countries are the US, France, Argentina and Brazil. Taken together, emigrants towards 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 US was the only country whose inflow falls relative to the prediction based on historical data after the Quotas.

wages and output, hence productivity. Studying wages would be informative because directed technical adoption hinges on the relatively more abundant labor turning relatively cheaper. Hence, an analysis of wages could reveal this pattern which we currently implicitly assume. Geographically disaggregated data on wages unfortunately do not currently exist. Productivity would in turn be key to investigate the welfare effects of the Quota Acts. However, disaggregated data on output was not recorded until 1936, hence we lack time series covering the period we study.

To conclude, it is not obvious that our results lend themselves to further generalization. Some similarities with contemporary settings nonetheless emerge. In terms of emigrants selection, the average Italian emigrant to the US was slightly positively selected, left rural areas and took on unskilled industrial jobs once in the US (Sequeira *et al.*, 2020). This description is not starkly different from contemporary emigration from poor countries, whereas it is clearly very distant from emigration from rich countries (*e.g.* Grogger & Hanson, 2011). While we do not claim that all our findings generalize to contemporary migration relationships, 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.

6 Summary of Robustness Checks

In this section we summarize our main robustness checks. More detailed analyses can be found in the Online Appendix. We essentially address two empirical problems. First, we provide evidence that results thus shown are robust to alternative measures of treatment exposure across districts. Second, we propose a simple instrumental variable framework to deal with potential endogeneity issues of our estimates.

6.1 Alternative Measures of Treatment Exposure

Our measure of the treatment is admittedly arbitrary. There are two margins along which measured quota exposure may be subject to mis-measurement. First, while the large majority of Ellis Island records after 1900 reports the district of origin, this is not true for the years 1890-1900. Records for these years most often only report "Italy" as the origin of a migrant. Similarly, after the 1921 Emergency Act was enacted, Ellis Island authorities largely stopped recording immigrants' origins at the municipality level. If there were systematic patterns underlying whether a migrant was recorded with her district of origin or was simply assigned with Italian nationality, then our measure would suffer from bias. Second, as discussed in section 2, emigration collapsed during WW1. It did nonetheless not completely dry out. During the War, districts closer to emigration ports are in fact disproportionately represented relative to previous shares.[23] This induces spurious variation in measured quota exposure 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 (7), we measure the exposure of a given districts as the share of people who migrated from that district over the years 1890-1924, relative to that district's 1900 population. To make sure 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 only consider emigrants who left no later than 1921. Then, we further restrict the subsample to the years before the outbreak of WW1. The first alternative measure seeks to control for the fact the Ellis Island database lacks information about municipality of origin for a high number of Italian migrants after 1921. We thus aim to clean for possible measurement error due to non random selection of registered district locations. The second exposure measure drops emigrants who left after the beginning of World War I, as emigration opportunities were possibly affected by the 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. Most likely, this is due to the fact that 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, even 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 WW1 induced a marked collapsed in those districts as well. Finally, emigrants lacking recorded district of origin are the majority for the post-1924 period. Yet, we find no noticeable pattern inducing non-random recording across districts. Hence, measured quota exposure should not be mismeasured either we include those years or not, as confirmed by the estimated coefficients.

6.2 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 developed by Card (2001) and Tabellini (2020) to address a similar –although specular– issue.

Let $\omega_{cr}^{t} \equiv \sum_{\tau=0}^{t} \text{Emigrants}_{c,\tau}^{U.S.} / \text{Emigrants}_{\tau}^{U.S.}$ be the share of emigrants from district c in region -or province- r until time t ($\text{Emigrants}_{c,t}^{U.S.}$) relative to total emigration ($\text{Emigrants}_{t}^{U.S.}$). We predict total

[23] Throughout this period, emigrants could sail overseas only from Naples, Palermo and Genoa.

emigrant outflow from district c from the following "zero-stage" equation:

$$\widehat{\text{Emigrants}}^{U.S.}_{-rc,T} = \omega^t_{cr} \times \sum_{\tau=1890}^{T} \sum_{c' \notin r} \text{Emigrants}^{U.S.}_{c',\tau} = \omega^t_{cr} \times \text{Emigrants}^{U.S.}_{-r,T} \tag{10}$$

In the first stage, we instrument QE_c using $\widehat{\text{Emigrants}}^{U.S.}_{-rc,T}$, and plug the resulting predicted $\widehat{\text{QE}}_c$ in the second-stage regression to estimate the baseline model (9).

The instrumental variable (10) exploits two sources of variation. Cross-sectional variation is embedded in the (ω^t_{cr}) 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 ($\text{Emigrants}^{U.S.}_{-r,T}$), 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 WW1 altered the composition of Italian emigrants to the U.S. in a spatially non-random fashion.

In table IX we summarize the results of the first stage regressions, where we vary measured quota exposure as discussed in section 6.1. We also control for different baseline years τ 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 as the instrument has high explanatory power, as we would expect for emigration –and immigration– patterns exhibit substantial persistence. Minor changes arise in the first stage comparing results for the two different baseline years considered, 1895 and 1900. An advantage of picking τ less than 1906 is that we wash out variation induced by the Messina-Reggio Calabria earthquake (Spitzer *et al.*, 2020). Tables XI 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 be determined by geographical clustering within South of the conditional identifying variation. This however does not affect neither the sign nor the significance of the results.

7 Conclusion

In recent years, immigration has become an increasingly focal and polarizing theme of the public debate. Policymakers exhibit very distant opinions of the effects of increased immigration on economic, social and cultural security of natives. Despite that, a common perspective can be disentangled. Proponents as well as dissenters 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 worrying, the more so given that sending nations often experience economic hardship and social distress.

In this study we explored how restrictive immigration policies shape economic development in sending countries. This poses two empirical challenges. First, emigration is seldom directed towards 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 pre-existing regulations enacted by both receiving as well as sending countries. To avoid both issues, we study the Italian emigration to the U.S. during the Age of Mass Migration (1850-1914). Through the 1921-1924 Immigration Acts, the U.S. adopted a very restrictive immigration policy which starkly contrasted the open border 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 manufacture 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.* Acemoglu, 2002, 2007). We document that population growth increased in comparatively more treated districts, consistently with the idea that the Quotas prevented individuals that would have migrated from doing so. Our empirical results endorse the directed technical adoption mechanism as we observe that in highly exposed districts industrial employment increased, while agricultural employment did not. Shifting the level of the analysis to manufacture sectors, we find that sectors where capital investment decreased the most were also the ones which 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, hence cheaper.

Taken together our results indicate that, even abstracting from the brain drain channel, IRPs still retain substantial effects on the economic development of sending countries. These effects are not one-way. On the one hand, IRPs in our setting pushed workers into the modern industrial sector. However, the bulk of the enlarged labor supply was absorbed by comparatively backward manufacture firms. On the other hand, increased employment was not complementary to investment in productivity-enhancing capital goods. Instead, firms substituted capital with labor. Because technology adoption is recognized as a key driver of long-run growth, this second effect threatens the long-run development trajectory of the Italian economy. The external validity of these findings is not obvious. This notwithstanding, we argued 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	11.496	0.686	10.735	11.496	12.391
Altitude	1070	5.426	1.159	4.212	5.748	6.444
Population	1066	11.732	0.725	10.885	11.715	12.675
5-Urbanization	1066	0.598	0.259	0.251	0.593	0.949
10-Urbanization	1066	0.370	0.270	0.000	0.311	0.796
15-Urbanization	1066	0.277	0.255	0.000	0.235	0.634
Panel B: Emigration						
Emigration (1890-1930)	1080	12.237	0.834	10.965	12.382	13.115
Emigration (1890-1921)	1080	12.145	0.836	10.870	12.268	13.026
Emigration (1890-1914)	1080	12.009	0.856	10.658	12.132	12.872
US Emigration (1890-1930)	1080	8.342	1.168	6.608	8.378	9.709
US Emigration (1890-1921)	1080	8.254	1.170	6.534	8.304	9.631
US Emigration (1890-1914)	1080	8.090	1.180	6.339	8.191	9.480
Panel C: Employment						
Agriculture Workers	1062	10.464	0.676	9.695	10.531	11.227
Manufacture Workers	1069	9.439	0.985	8.288	9.371	10.729
Trade Workers	1070	8.100	0.957	6.998	7.988	9.295
Liberal Professions	1062	7.189	1.056	5.937	7.152	8.447
Public Administration	1062	7.613	1.038	6.381	7.515	8.901
Panel D: Capital and Technology						
Firms	1061	2.115	0.163	1.846	2.147	2.296
Firms with Engine	1061	1.841	0.194	1.570	1.857	2.076
Mechanical Engines	1061	1.854	0.118	1.706	1.842	2.018
Electrical Engines	1061	1.875	0.314	1.459	1.893	2.280
Mechanical Horsepower	1061	2.329	0.141	2.155	2.309	2.541
Electrical Horsepower	1061	2.191	0.236	1.869	2.195	2.493

Notes. This table reports summary statistics for the variables in our dataset except sector-specific capital and employment ones. All variables except the literacy rate are in log. All variables are district level, except the literacy rate which is at province level. All variables are at district level. Section 4 explains how we impute province-level data to districts.

TABLE II: BALANCE TABLE

	1911	1921
All Firms	0.029	-0.022
	(0.034)	(0.036)
Firms with Engine	0.048	-0.012
	(0.096)	(0.124)
Mechanical Engines	0.089	-0.168
	(0.189)	(0.218)
Electrical Engines	0.005	-0.001
	(0.017)	(0.021)
Mechanical Horsepower	-0.095	0.056
	(0.093)	(0.113)
Electrical Horsepower	-0.004	-0.026
	(0.058)	(0.078)
Population	-0.037	0.106
	(0.165)	(0.218)
Manufacture Workers	-0.028	0.017
	(0.090)	(0.079)
Agriculture Workers	-0.144	-0.048
	(0.157)	(0.136)
Trade Workers	-0.151	0.099
	(0.121)	(0.078)
Liberal Professions	0.005	0.120
	(0.116)	(0.221)
Public Administration	0.027	0.036
	(0.113)	(0.139)

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 1890-1914 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 by province. Under validity of the parallel trends assumption, we would require all coefficients not to be statistically different from zero.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table III: Investment in capital goods and emigration

	Firm		Engine		Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Quota Exposure × Post	0.025	0.057	-0.185***	-0.515***	-0.105***	-0.317***
	(0.046)	(0.098)	(0.031)	(0.108)	(0.026)	(0.050)
Extensive Margin × Post	-0.001	0.046	0.032	0.083	-0.006	0.040
	(0.018)	(0.043)	(0.020)	(0.054)	(0.010)	(0.025)
Population	-0.008	-0.018	-0.012	-0.067*	-0.013	-0.029*
	(0.011)	(0.023)	(0.007)	(0.032)	(0.008)	(0.013)
Slackness × Post	-0.000	0.004	-0.001	0.004	-0.003***	-0.000
	(0.002)	(0.003)	(0.001)	(0.004)	(0.001)	(0.001)
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	786	784	783	785	784
R2	0.954	0.826	0.430	0.782	0.870	0.881
F-stat	0.258	0.792	14.442	7.047	8.740	14.827
Mean Dep. Var.	0.101	0.100	0.004	0.131	0.029	0.107

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 in growth rate. Standard errors are always clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE IV: LABOR INTENSITY AND EMIGRATION

	Worker/Firm		Worker/Engine		Worker/Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Quota Exposure × Post	0.209	0.354*	0.551***	0.632***	0.431***	0.442***
	(0.140)	(0.164)	(0.133)	(0.123)	(0.111)	(0.122)
Extensive Margin × Post	-0.088	-0.154*	-0.144**	-0.129*	-0.096*	-0.097
	(0.049)	(0.066)	(0.045)	(0.062)	(0.040)	(0.050)
Population	0.283***	0.301***	0.213***	0.240***	0.149***	0.232***
	(0.045)	(0.052)	(0.034)	(0.037)	(0.028)	(0.036)
Slackness × Post	0.007	0.002	0.006	-0.010	0.007	0.002
	(0.011)	(0.013)	(0.009)	(0.009)	(0.007)	(0.010)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	209	209	208	208	209
Observations	784	785	785	785	784	785
R2	0.668	0.559	0.453	0.739	0.517	0.588
F-stat	14.909	13.423	20.431	17.215	14.597	18.146
Mean Dep. Var.	-0.058	-0.054	0.033	-0.109	0.001	-0.077

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 in growth rate. District fixed effects refer to 1921-*circondari*. Standard errors are always robust and clustered at the district level.
*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

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 × Post			0.021***	0.023***
			(0.006)	(0.007)
Extensive Margin × Post		-0.068		-0.051
		(0.055)		(0.053)
Population	0.146***	0.141***	0.139***	0.135***
	(0.030)	(0.030)	(0.030)	(0.031)
Slackness 1901 × Post	0.010	0.008	0.012*	0.010
	(0.006)	(0.006)	(0.005)	(0.005)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Districts	204	204	204	204
Observations	751	751	751	751
R2	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

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 (7); 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. Standard errors are always clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

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)
Population	0.206	0.242*	0.141	0.169
	(0.123)	(0.123)	(0.082)	(0.073)
Slackness × Post	-0.008	0.012	-0.008	-0.003
	(0.012)	(0.021)	(0.005)	(0.006)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Districts	205	205	206	206
Observations	742	742	750	750
R2	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

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. Standard errors are robust and clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

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***	-0.419***	-0.425***
	(0.356)	(0.410)	(0.145)	(0.156)	(0.110)	(0.110)
Extensive Margin × Post		0.598		0.066		0.016
		(0.350)		(0.085)		(0.035)
Population	0.075	0.105	0.054	0.062	0.171***	0.173***
	(0.090)	(0.089)	(0.042)	(0.043)	(0.027)	(0.028)
Slackness × Post	-0.004	0.016	-0.003	-0.001	-0.004	-0.003
	(0.007)	(0.016)	(0.005)	(0.005)	(0.006)	(0.007)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	205	205	204	204	202	202
Observations	729	729	743	743	992	992
R2	0.476	0.478	0.510	0.510	0.955	0.955
F-stat	6.085	6.494	5.470	4.049	17.959	13.850
Mean Dep. Var.	0.051	0.051	-0.022	-0.022	0.278	0.278

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. 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. Standard errors are robust and clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE VIII: CHANGES IN INDUSTRY EMPLOYMENT BY SECTOR

	Mining	Agriculture	Steel	Construction	Textile	Chemical
Quota Exposure × Post	0.351	-2.452	1.384	6.103***	5.651***	0.019
	(0.307)	(1.259)	(1.574)	(1.626)	(1.398)	(0.308)
Extensive Margin × Post	-0.000	1.029	-1.124	-2.693*	-0.715	0.182
	(0.228)	(1.254)	(1.578)	(1.293)	(0.991)	(0.277)
Population	0.093	0.449	0.752	-0.095	-0.550	-0.038
	(0.077)	(0.305)	(0.454)	(0.359)	(0.343)	(0.078)
Slackness × Post	0.011	0.038	-0.019	0.153**	-0.027	-0.002
	(0.012)	(0.054)	(0.071)	(0.051)	(0.060)	(0.011)
Industry Employment Growth	0.086***	0.345***	0.457***	0.345**	-0.055	0.039*
	(0.015)	(0.069)	(0.105)	(0.116)	(0.057)	(0.016)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	193	200	198	200	200	195
Observations	683	776	775	778	774	681
R2	0.072	0.424	0.106	0.317	0.449	0.451
F-stat	8.136	5.598	5.024	16.663	4.554	1.844
Mean Dep. Var.	0.724	0.423	0.250	0.553	0.291	0.753

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. Standard errors are clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE IX: FIRST STAGE: ACTUAL AND INSTRUMENTED QUOTA EXPOSURE

	Dep. Var.: Quota Exposure		
	(1890-1930)	(1890-1914)	(1890-1924)
IV Quota Exposure	0.778***	0.833***	0.791***
	(0.038)	(0.038)	(0.039)
Extensive Margin × Post	0.012	-0.001	0.011
	(0.015)	(0.012)	(0.015)
Slackness × Post	0.005	0.003	0.005
	(0.004)	(0.003)	(0.004)
Population	-0.032*	-0.019	-0.026*
	(0.014)	(0.010)	(0.012)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Districts	207	207	207
Observations	754	754	754
K-P F-stat	414.366	483.861	422.069

Notes. This table reports the result of the first stage instrumental variable estimation. The instrument (IV Quota Exposure) is defined in (10). 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). 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table X: Investment in capital goods and emigration - 2SLS

	Firm		Engine		Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Panel A: OLS						
Quota Exposure × Post	0.025	0.057	-0.185***	-0.515***	-0.105***	-0.317***
	(0.046)	(0.098)	(0.031)	(0.108)	(0.026)	(0.050)
Panel B: 2SLS						
Quota Exposure × Post	0.054	0.094	-0.157***	-0.503***	-0.101***	-0.297***
	(0.043)	(0.095)	(0.032)	(0.115)	(0.027)	(0.048)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	209	209	209	209
Observations	786	787	786	785	785	785
F-stat	0.391	0.711	8.515	4.540	6.288	10.241
Mean Dep. Var.	0.101	0.100	0.004	0.131	0.029	0.107

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 (10). 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 in growth rate. Standard errors are always clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table XI: Population Growth, Employment in Industry and Agriculture

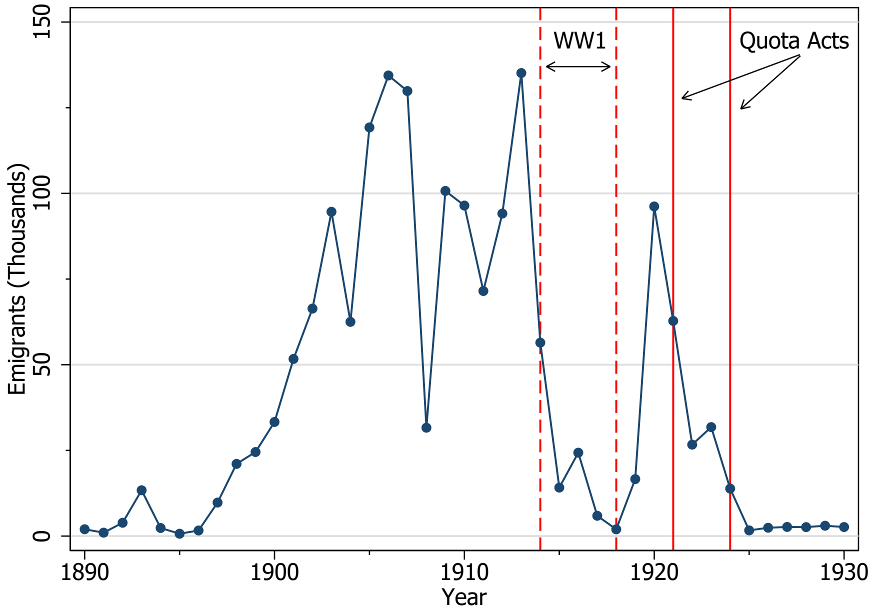
	Population Growth	Industry Growth	Agriculture Growth
Panel A: OLS			
Quota Exposure × Post	0.449***	1.510**	-0.483
	(0.124)	(0.475)	(0.176)
Panel B: 2SLS			
Quota Exposure × Post	0.668***	1.673**	-0.138
	(0.138)	(0.544)	(0.222)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Districts	207	205	209
Observations	754	742	753
F-stat	14.137	6.743	0.274
Mean Dep. Var.	0.042	0.060	-0.041

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 (10). 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 in growth rate. Standard errors are always clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Figures

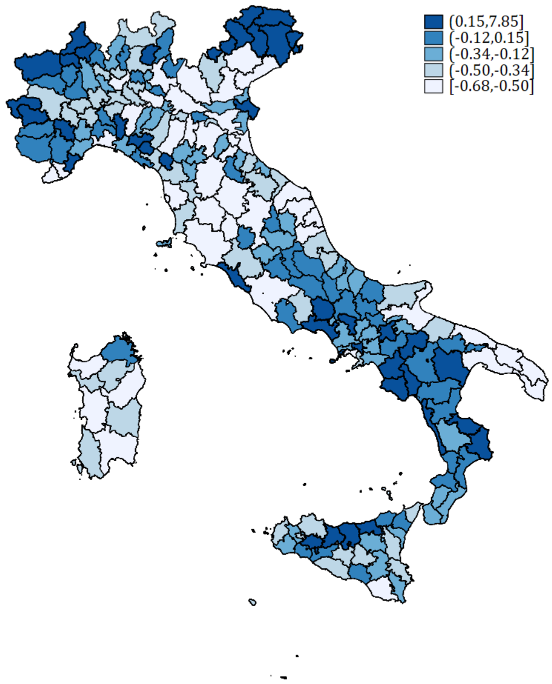
Figure I: Total Inflow of Italian Immigrants at Ellis Island



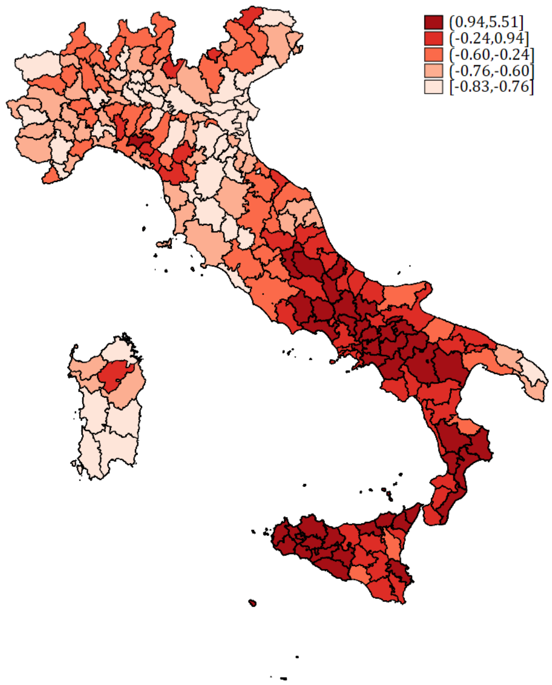
Notes. This figure displays the aggregate number of Italians who registered at the Ellis Island immigration station between 1890-1930. Dashed red lines indicate the period of WW1; solid red lines indicate the 1921 Emergency Quota Act and the 1924 (Johnson-Reed) Immigration Act. Only migrants whose origin 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, 1890-1930

(A) Emigration Rate

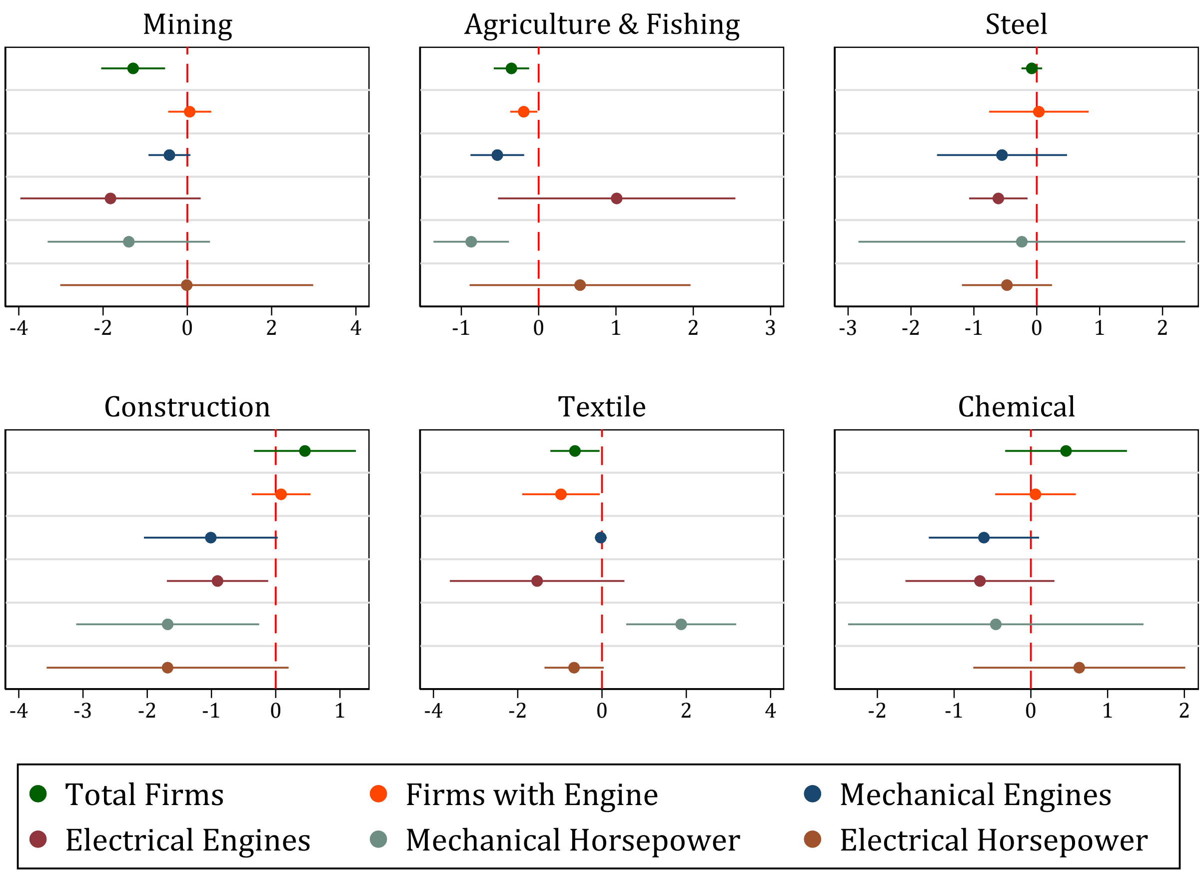


(B) Quota Exposure



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 vairables. 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 (9) 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

May, 2021

A Data Description

A.1 Emigration Matching Procedure

This appendix 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}:

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

where

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

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.[24]

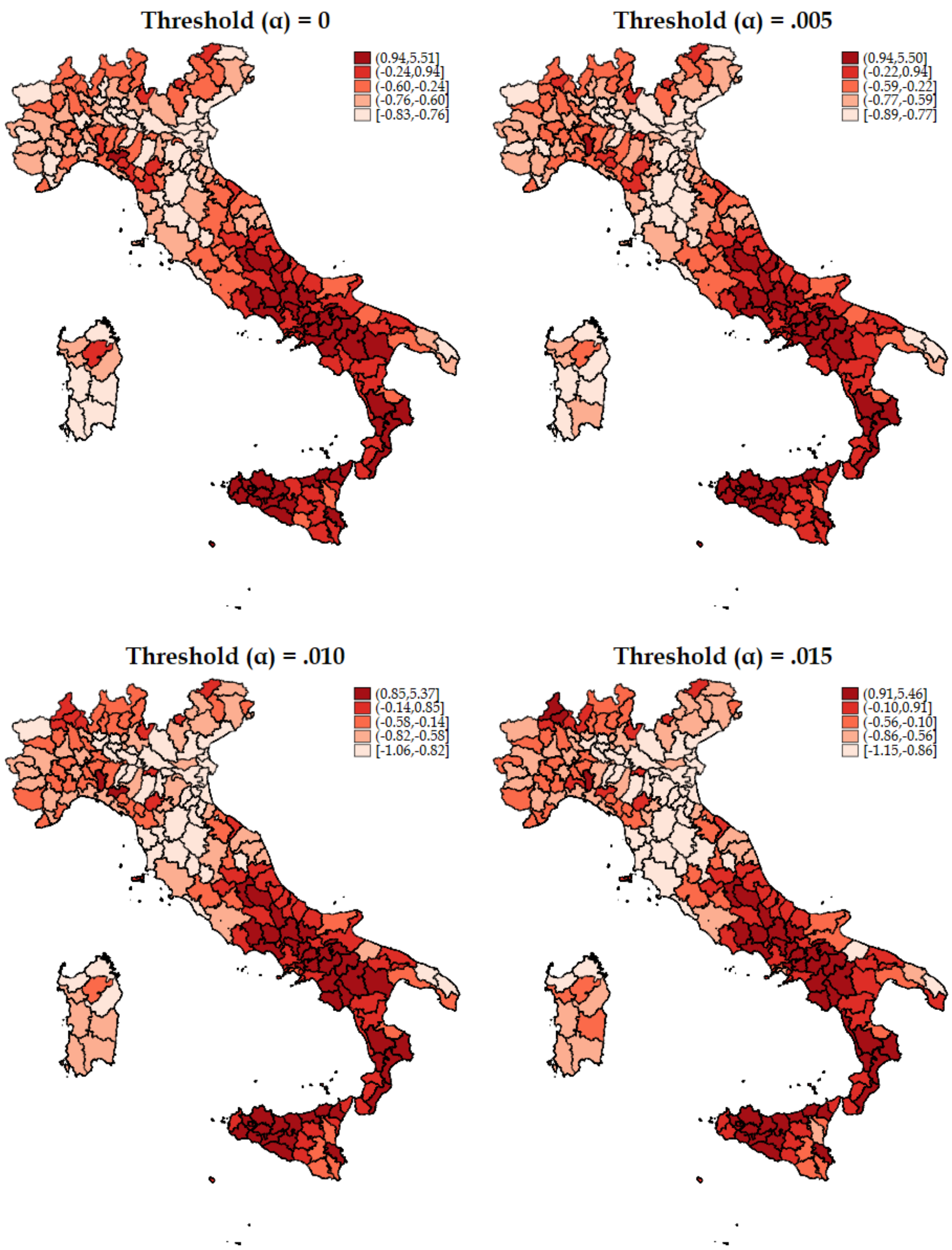
The Jaro-Winker distance has been shown to perform relatively well in linking routines (Abramitzky *et al.*, 2019). In our particular case however, this metric outperforms more standard string dissimilarity

[24] 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).

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 for a complete description of the procedure and the meaning of α.

A.2 Data Sources

We here describe the sources from which we gathered the data needed for our analysis. Analyses are mainly conducted at district level, where districts can be considered as commuting zones, 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 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 resulting in our sample of years for the population’s occupations to be: 1901, 1911, 1921, 1927, 1936. As far as it concerns data on number of firms, engines and horsepower, they are available in the Industrial Censuses: information were available for the years 1911, 1927, 1937.

Data on migration flows are gathered at municipality level from the Ellis Island database, starting from the year 1881. Population at municipality level was instead collected for all Population Censuses starting from 1861. For the year 1901, 1911, 1921 data on population by occupation were available at 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 meaning that data on occupations for the 1936 had to be collected at municipality level, for a total of about 8000 municipalities.

As far as it concerns data on industries (information on number of firms, engines, type of engine, horsepower), they were available only for those municipalities with a population of 15’000 inhabitants or more and with industrial activity: this is not of great concern, in fact districts usually took the name of the biggest municipality, which was in the vast majority of cases the most important (if not the only) industrial site among all the municipalities in that same district. We hence managed to match those observations with district data on migration flows, for a total of about 150 units per year (the total number of districts is instead about 200). Industrial data at (big) municipality- and province-level are available in the Industrial Censuses for the years 1911, 1927, 1936.

B Additional Tables & Results

Table B.1: Regional Emigration

Region	Immigrants to US					Immigrants 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	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 displayed are in thousands. The 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: 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)
Quota Exposure × Post	-0.195***	-0.207***	-0.206***	-0.200***	-0.175***	-0.168***	-0.438***	-0.471***	-0.496***	-0.469***	-0.450***	-0.362**
	(0.032)	(0.032)	(0.033)	(0.032)	(0.038)	(0.042)	(0.107)	(0.110)	(0.105)	(0.105)	(0.110)	(0.113)
Population	-0.016*	-0.012	-0.013	-0.013	-0.010	-0.009	-0.085**	-0.074*	-0.053	-0.053	-0.051	-0.048
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.032)	(0.032)	(0.031)	(0.031)	(0.032)	(0.031)
Extensive Margin × Post		0.027	0.028	0.025	0.028	0.026		0.079	0.055	0.037	0.039	0.015
		(0.015)	(0.015)	(0.014)	(0.014)	(0.015)		(0.055)	(0.055)	(0.059)	(0.059)	(0.064)
Agriculture × Post			-0.004	-0.009	-0.003	-0.004			0.091***	0.065*	0.069*	0.064*
			(0.007)	(0.009)	(0.011)	(0.011)			(0.022)	(0.029)	(0.032)	(0.032)
Urbanization × Post				-0.005	-0.003	-0.003				-0.027	-0.025	-0.025
				(0.005)	(0.006)	(0.006)				(0.017)	(0.017)	(0.017)
Literacy × Post					0.008	0.006					0.006	-0.017
					(0.007)	(0.008)					(0.020)	(0.025)
South × Post						-0.001						-0.018
						(0.003)						(0.011)
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	208	208	208	208	208	208	207	207	207	207	207	207
Observations	801	801	801	801	801	801	800	800	800	800	800	800
R2	0.355	0.359	0.359	0.360	0.361	0.361	0.790	0.790	0.795	0.796	0.796	0.797
F-stat	17.673	13.930	11.298	9.474	8.296	7.375	7.471	5.954	8.356	7.679	6.663	6.007
Mean Dep. Var.	0.004	0.004	0.004	0.004	0.004	0.004	0.132	0.132	0.132	0.132	0.132	0.132

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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.3: 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)
Quota Exposure × Post	-0.113***	-0.121***	-0.121***	-0.123***	-0.103**	-0.114**	-0.286***	-0.305***	-0.305***	-0.298***	-0.259***	-0.255***
	(0.026)	(0.029)	(0.029)	(0.029)	(0.032)	(0.037)	(0.048)	(0.051)	(0.051)	(0.052)	(0.061)	(0.067)
Population	-0.019*	-0.015	-0.016	-0.016	-0.013	-0.013	-0.044**	-0.037**	-0.037*	-0.037*	-0.031*	-0.031*
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Extensive Margin × Post		0.019	0.019	0.020	0.022	0.025		0.048	0.048	0.044	0.049	0.048
		(0.020)	(0.020)	(0.020)	(0.020)	(0.020)		(0.034)	(0.034)	(0.035)	(0.034)	(0.034)
Agriculture × Post			-0.001	0.001	0.006	0.006			-0.001	-0.008	0.002	0.002
			(0.008)	(0.009)	(0.010)	(0.010)			(0.010)	(0.013)	(0.015)	(0.015)
Urbanization × Post				0.002	0.004	0.004				-0.007	-0.003	-0.003
				(0.005)	(0.005)	(0.005)				(0.008)	(0.008)	(0.008)
Literacy × Post					0.006	0.009					0.013	0.012
					(0.006)	(0.007)					(0.010)	(0.011)
South × Post						0.002						-0.001
						(0.003)						(0.005)
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	209	209	209	209	208	208	208	208	208	208
Observations	802	802	802	802	802	802	801	801	801	801	801	801
R2	0.855	0.855	0.855	0.855	0.855	0.855	0.875	0.876	0.875	0.875	0.876	0.875
F-stat	10.007	7.832	6.265	5.626	4.376	3.828	19.138	15.005	12.179	10.076	9.044	7.910
Mean Dep. Var.	0.030	0.030	0.030	0.030	0.030	0.030	0.107	0.107	0.107	0.107	0.107	0.107

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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.4: Robustness Regressions - Labor intensity of Technology: Mechanical and Electrical Engines

	Dep. Var.: Changes in Workers per Mechanical Engines						Dep. Var.: Changes in Workers per Electrical Engines					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Quota Exposure × Post	0.515***	0.586***	0.555***	0.595***	0.710***	0.581***	0.636***	0.693***	0.682***	0.705***	0.928***	0.701***
	(0.134)	(0.141)	(0.135)	(0.131)	(0.153)	(0.164)	(0.152)	(0.158)	(0.158)	(0.155)	(0.168)	(0.174)
Population	0.247***	0.227***	0.257***	0.262***	0.277***	0.274***	0.384***	0.367***	0.380***	0.380***	0.409***	0.402***
	(0.035)	(0.035)	(0.035)	(0.033)	(0.034)	(0.035)	(0.051)	(0.053)	(0.053)	(0.052)	(0.052)	(0.052)
Extensive Margin × Post		-0.155**	-0.208***	-0.242***	-0.228***	-0.195***		-0.130	-0.155*	-0.173*	-0.147	-0.087
		(0.048)	(0.046)	(0.046)	(0.044)	(0.046)		(0.076)	(0.074)	(0.071)	(0.075)	(0.086)
Agriculture × Post			0.146***	0.090**	0.116***	0.126***			0.066	0.039	0.090	0.105*
			(0.028)	(0.028)	(0.033)	(0.032)			(0.035)	(0.039)	(0.046)	(0.044)
Urbanization × Post				-0.062***	-0.050**	-0.050**				-0.029	-0.007	-0.006
				(0.015)	(0.016)	(0.016)				(0.023)	(0.023)	(0.023)
Literacy × Post					0.036	0.072**					0.070*	0.134***
					(0.020)	(0.026)					(0.029)	(0.036)
South × Post						0.027*						0.047**
						(0.012)						(0.015)
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	209	209	209	209	208	208	208	208	208	208
Observations	785	785	785	785	785	785	785	785	785	785	785	785
R2	0.447	0.455	0.491	0.508	0.510	0.517	0.672	0.673	0.674	0.675	0.678	0.683
F-stat	22.393	20.630	22.694	25.365	24.792	23.541	22.089	18.539	16.259	14.551	14.725	13.420
Mean Dep. Var.	0.035	0.035	0.035	0.035	0.035	0.035	-0.095	-0.095	-0.095	-0.095	-0.095	-0.095

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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

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

	Dep. Var.: Changes in Workers per Mechanical Horsepower						Dep. Var.: Changes in Workers per Electrical Horsepower					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Quota Exposure × Post	0.460***	0.518***	0.499***	0.537***	0.649***	0.519**	0.463**	0.517***	0.493***	0.542***	0.644***	0.485**
	(0.134)	(0.142)	(0.132)	(0.126)	(0.150)	(0.162)	(0.146)	(0.155)	(0.142)	(0.130)	(0.156)	(0.163)
Population	0.211***	0.194***	0.229***	0.232***	0.249***	0.249***	0.287***	0.272***	0.309***	0.310***	0.325***	0.324***
	(0.037)	(0.037)	(0.038)	(0.036)	(0.038)	(0.039)	(0.043)	(0.044)	(0.044)	(0.041)	(0.043)	(0.044)
Extensive Margin × Post		-0.123*	-0.180***	-0.211***	-0.198***	-0.163***		-0.121*	-0.184***	-0.224***	-0.212***	-0.171**
		(0.049)	(0.048)	(0.047)	(0.044)	(0.046)		(0.057)	(0.053)	(0.050)	(0.049)	(0.053)
Agriculture × Post			0.151***	0.099***	0.125***	0.134***			0.169***	0.103**	0.126**	0.137***
			(0.027)	(0.028)	(0.032)	(0.031)			(0.032)	(0.034)	(0.041)	(0.040)
Urbanization × Post				-0.057***	-0.046**	-0.045**				-0.071***	-0.061**	-0.060**
				(0.015)	(0.016)	(0.016)				(0.018)	(0.020)	(0.021)
Literacy × Post					0.035	0.072**					0.031	0.077*
					(0.020)	(0.026)					(0.023)	(0.030)
South × Post						0.027*						0.033*
						(0.012)						(0.013)
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	209	209	209	209	209	209	209	209	209	209
Observations	785	785	785	785	785	785	785	785	785	785	785	785
R2	0.466	0.470	0.508	0.523	0.525	0.531	0.532	0.535	0.566	0.582	0.582	0.589
F-stat	16.444	14.448	16.477	19.247	18.451	18.298	20.076	17.162	19.768	21.611	19.366	18.652
Mean Dep. Var.	0.011	0.011	0.011	0.011	0.011	0.011	-0.068	-0.068	-0.068	-0.068	-0.068	-0.068

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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.6: Robustness Regressions - Population Growth

	Dep. Var.: Population Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Quota Exposure × Post	0.408***	0.446***	0.422***	0.443***	0.515***	0.366**
	(0.113)	(0.124)	(0.120)	(0.120)	(0.134)	(0.134)
Population	0.146***	0.142***	0.165***	0.166***	0.180***	0.175***
	(0.030)	(0.030)	(0.031)	(0.030)	(0.032)	(0.033)
Extensive Margin × Post		-0.065	-0.091	-0.109	-0.101	-0.061
		(0.055)	(0.057)	(0.059)	(0.055)	(0.051)
Agriculture × Post			0.095***	0.072**	0.090**	0.098**
			(0.024)	(0.026)	(0.031)	(0.030)
Urbanization × Post				-0.026*	-0.020	-0.019
				(0.013)	(0.014)	(0.014)
Literacy × Post					0.024	0.065***
					(0.017)	(0.019)
South × Post						0.031***
						(0.008)
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	0.453	0.474	0.478	0.479	0.493
F-stat	13.726	10.139	10.400	12.096	14.920	17.552
Mean Dep. Var.	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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.7: Robustness Regressions - Changes in Industrial Employment

	Dep. Var.: Industry Workers Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Quota Exposure × Post	1.825***	1.497**	1.471**	1.469**	1.457**	1.191*
	(0.427)	(0.476)	(0.477)	(0.488)	(0.552)	(0.581)
Population	0.206	0.243*	0.262*	0.261*	0.259	0.251
	(0.123)	(0.123)	(0.126)	(0.127)	(0.137)	(0.137)
Extensive Margin × Post		0.652	0.619	0.621	0.616	0.703
		(0.403)	(0.404)	(0.409)	(0.420)	(0.417)
Agriculture × Post			0.077	0.079	0.075	0.087
			(0.082)	(0.094)	(0.108)	(0.109)
Urbanization × Post				0.001	0.000	0.001
				(0.058)	(0.061)	(0.061)
Literacy × Post					-0.004	0.056
					(0.072)	(0.081)
South × Post						0.048
						(0.036)
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	0.540	0.542	0.542	0.541	0.540	0.540
F-stat	6.777	6.951	6.664	5.616	5.194	5.130
Mean Dep. Var.	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. 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. *: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.8: Robustness Regressions - Changes in the Share of Industrial Workers

	Dep. Var.: Changes in Share of Industrial Workers					
	(1)	(2)	(3)	(4)	(5)	(6)
Quota Exposure × Post	1.455***	1.139**	1.118**	1.237**	1.204*	1.181*
	(0.356)	(0.411)	(0.412)	(0.425)	(0.465)	(0.516)
Population	0.074	0.105	0.124	0.134	0.129	0.128
	(0.090)	(0.088)	(0.092)	(0.093)	(0.096)	(0.097)
Extensive Margin × Post		0.613	0.579	0.509	0.497	0.505
		(0.353)	(0.351)	(0.360)	(0.372)	(0.383)
Agriculture × Post			0.072	0.004	-0.005	-0.003
			(0.059)	(0.075)	(0.096)	(0.099)
Urbanization × Post				-0.077	-0.081	-0.081
				(0.053)	(0.061)	(0.061)
Literacy × Post					-0.012	-0.006
					(0.064)	(0.081)
South × Post						0.004
						(0.036)
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	729	729	729	729	729	729
R2	0.476	0.478	0.478	0.479	0.478	0.477
F-stat	6.068	6.487	5.568	5.131	4.430	3.926
Mean Dep. Var.	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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.9: 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)
Quota Exposure × Post	-1.267**	-1.184**	-1.207**	-1.187**	-1.094**	-2.323***	-2.346**	-2.216**	-2.329**	-2.252**
	(0.385)	(0.396)	(0.393)	(0.383)	(0.399)	(0.692)	(0.720)	(0.705)	(0.715)	(0.739)
Population	0.316*	0.297*	0.316*	0.317*	0.282	0.316	0.322	0.199	0.200	0.172
	(0.133)	(0.136)	(0.138)	(0.138)	(0.146)	(0.207)	(0.208)	(0.220)	(0.221)	(0.223)
Extensive Margin × Post		-0.181	-0.210	-0.225	-0.251		0.050	0.205	0.287	0.267
		(0.159)	(0.161)	(0.167)	(0.163)		(0.439)	(0.427)	(0.458)	(0.459)
Agriculture × Post			0.094	0.076	0.159			-0.530***	-0.421*	-0.326
			(0.097)	(0.126)	(0.162)			(0.157)	(0.212)	(0.284)
Urbanization × Post				-0.020	-0.032				0.115	0.124
				(0.071)	(0.069)				(0.131)	(0.134)
Construction Employment × Post					0.000					
					(0.000)					
Textile Employment × Post										0.000
										(0.000)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	208	208	208	208	208	209	209	209	209	209
Observations	786	786	786	786	786	791	791	791	791	791
R2	0.808	0.807	0.807	0.807	0.807	0.873	0.873	0.876	0.876	0.876
F-stat	5.352	4.724	4.134	3.747	3.928	21.263	17.038	16.080	13.346	11.941
Mean Dep. Var.	0.062	0.062	0.062	0.062	0.062	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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.10: Robustness Regressions - Employment Growth in Selected Manufacture Sectors

	Dep. Var.: Employment Changes in Construction Firms					Dep. Var.: Employment Changes in Textile Firms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Quota Exposure × Post	5.298**	7.013***	7.013***	7.307***	6.624***	7.664***	8.254***	8.254***	8.729***	7.630***
	(1.621)	(1.868)	(1.869)	(1.878)	(1.904)	(1.850)	(2.042)	(2.043)	(1.951)	(1.898)
Population	0.031	-0.109	-0.105	-0.118	0.086	-0.757	-0.804	-0.801	-0.816	-0.550
	(0.390)	(0.413)	(0.433)	(0.430)	(0.448)	(0.497)	(0.501)	(0.533)	(0.526)	(0.511)
Extensive Margin × Post		-3.094*	-3.106*	-3.318*	-2.862		-1.045	-1.051	-1.408	-0.542
		(1.485)	(1.468)	(1.526)	(1.479)		(1.447)	(1.469)	(1.497)	(1.428)
Agriculture × Post			0.018	-0.147	-0.583			0.010	-0.248	-1.160*
			(0.295)	(0.315)	(0.419)			(0.442)	(0.518)	(0.572)
Urbanization × Post				-0.180	-0.090				-0.281	-0.341
				(0.192)	(0.187)				(0.264)	(0.255)
Construction Employment × Post					-0.002*					
					(0.001)					
Textile Employment × Post										-0.002***
										(0.001)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	200	200	200	200	200	200	200	200	200	200
Observations	778	778	778	778	778	774	774	774	774	774
R2	0.313	0.317	0.315	0.315	0.317	0.450	0.449	0.448	0.448	0.453
F-stat	20.251	16.663	14.119	10.408	10.504	5.664	4.554	3.925	4.338	5.601
Mean Dep. Var.	0.553	0.553	0.553	0.553	0.553	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. 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.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE B.11: URBANIZATION AND SHARE OF WORKERS EMPLOYED IN INDUSTRYAND 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			
Quota Exposure × Post	-0.332**	1.382**	-0.603***
	(0.124)	(0.474)	(0.177)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Number of Districts	205	207	208
Observations	995	731	746
F-stat	2.355	5.340	2.912
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 (10). 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. Standard errors are robust and clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

TABLE B.12: LABOR INTENSITY AND EMIGRATION - 2SLS

	Worker/Firm		Worker/Engine		Worker/Horsepower	
	All	Engine	Mechanic	Electric	Mechanic	Electric
Panel A: OLS						
Quota Exposure × Post	0.209	0.354*	0.551***	0.632***	0.431***	0.442***
	(0.140)	(0.164)	(0.133)	(0.123)	(0.111)	(0.122)
Panel B: 2SLS						
Quota Exposure × Post	0.649***	0.796***	0.887***	0.836***	0.729***	0.800***
	(0.149)	(0.184)	(0.126)	(0.129)	(0.101)	(0.129)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	209	209	209	209	209	209
Observations	785	785	785	786	785	785
F-stat	14.856	12.932	19.772	15.233	16.565	17.922
Mean Dep. Var.	-0.058	-0.054	0.033	-0.109	0.001	-0.077

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 (10). 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. Standard errors are robust and clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.13: 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	-1.541***	-0.831**	-0.374**	-0.180	-0.003	0.107	0.630	0.726	-0.855***	-0.543**	0.170	-0.163
	(0.299)	(0.314)	(0.127)	(0.117)	(0.117)	(0.126)	(0.345)	(0.373)	(0.179)	(0.205)	(0.454)	(0.417)
Panel B: Firms with Engine												
Quota Exposure × Post	-0.124	0.025	-0.257*	-0.061	0.037	0.373	0.016	0.198	-1.228***	-0.814*	-0.166	-0.270
	(0.398)	(0.381)	(0.113)	(0.115)	(0.462)	(0.432)	(0.206)	(0.217)	(0.293)	(0.337)	(0.296)	(0.272)
Panel C: Mechanical Engines												
Quota Exposure × Post	-0.518**	-0.385*	-0.538*	-0.172	-0.731	-0.751	-1.184**	-0.670	0.102	0.137	-0.583	-0.500
	(0.172)	(0.195)	(0.223)	(0.219)	(0.445)	(0.432)	(0.396)	(0.447)	(0.098)	(0.102)	(0.312)	(0.321)
Panel D: Electrical Engines												
Quota Exposure × Post	-2.031*	-1.468	1.342*	1.332*	-0.934**	-0.629*	-1.010*	-0.239	-2.346**	-1.751*	-0.886	-0.256
	(1.001)	(0.929)	(0.517)	(0.572)	(0.311)	(0.292)	(0.481)	(0.423)	(0.720)	(0.821)	(0.584)	(0.557)
Panel E: Mechanical Horsepower												
Quota Exposure × Post	-1.714**	-1.395	-1.047***	-0.390	-1.018	-1.399	-2.056***	-1.139	2.747***	1.858**	0.171	0.565
	(0.656)	(0.720)	(0.280)	(0.295)	(1.218)	(1.166)	(0.487)	(0.588)	(0.661)	(0.707)	(1.011)	(1.020)
Panel F: Electrical Horsepower												
Quota Exposure × Post	-1.311	-0.813	1.006	1.319	-0.927**	-0.816*	-2.088	-0.535	-0.944*	-0.508	0.425	0.634
	(1.533)	(1.629)	(0.688)	(0.739)	(0.353)	(0.398)	(1.151)	(1.006)	(0.416)	(0.437)	(0.695)	(0.777)
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	208	208	209	209	209	209	209	209	209	209
Observations	788	788	787	787	791	791	786	786	789	789	788	788

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

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

Table B.14: Changes in Industry Employment by Sector - 2SLS

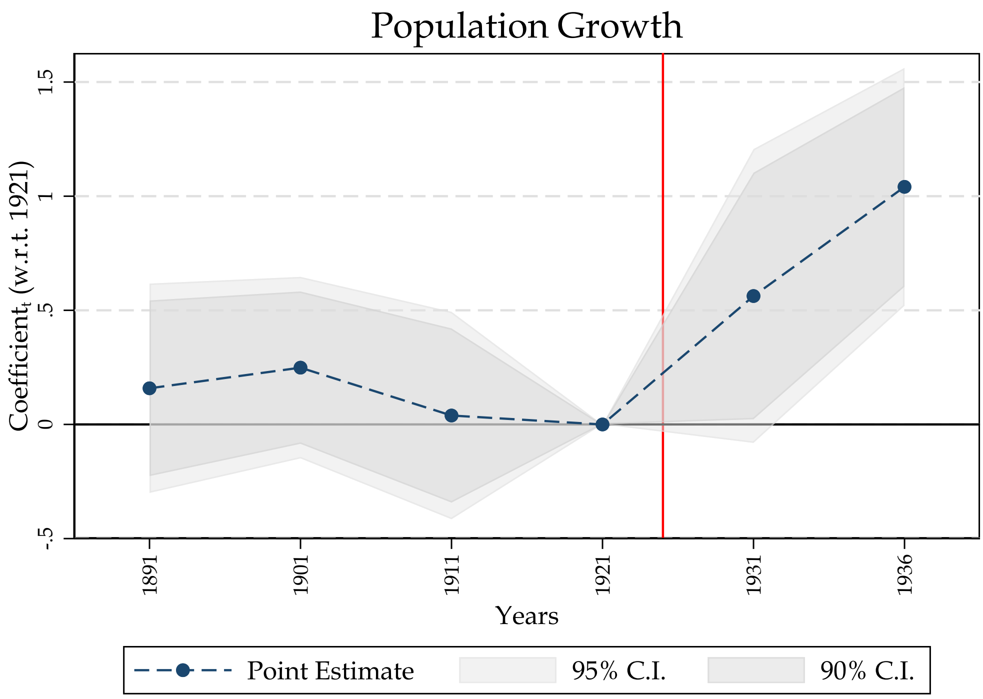
	Mining	Agriculture	Steel	Construction	Textile	Chemical
Panel A: OLS						
Quota Exposure × Post	0.351	-2.452	1.384	6.103***	5.651***	0.019
	(0.307)	(1.259)	(1.574)	(1.626)	(1.398)	(0.308)
Panel B: 2SLS						
Quota Exposure × Post	0.332	-2.269	2.763	5.912**	7.078***	0.159
	(0.391)	(1.581)	(1.577)	(2.183)	(1.327)	(0.361)
Observations	683	776	775	778	774	681
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Districts	193	200	198	200	200	195
Observations	683	776	775	778	774	681
F-stat	8.095	5.275	5.974	15.310	8.373	1.823
Mean Dep. Var.	0.724	0.423	0.250	0.553	0.291	0.753

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 (10). 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. Standard errors are clustered at the district level.

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$.

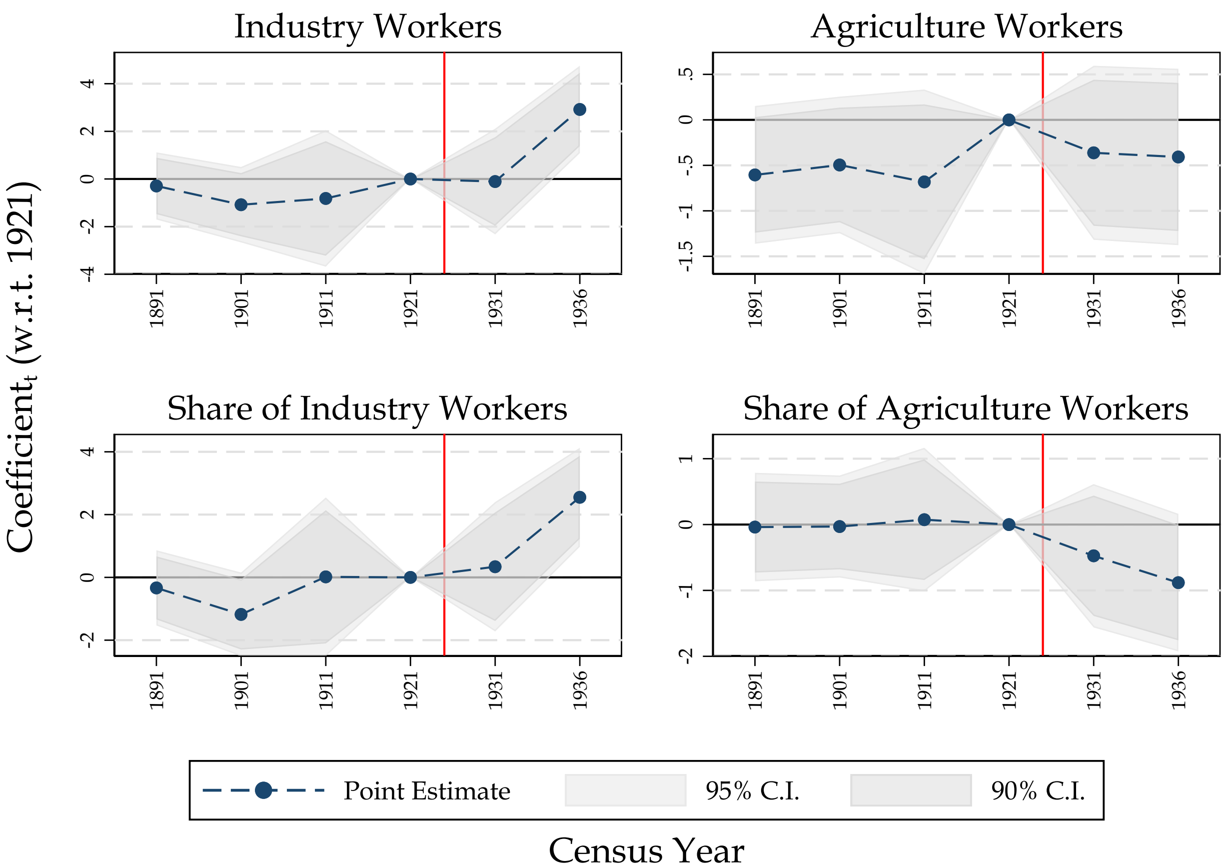
C Additional Figures

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



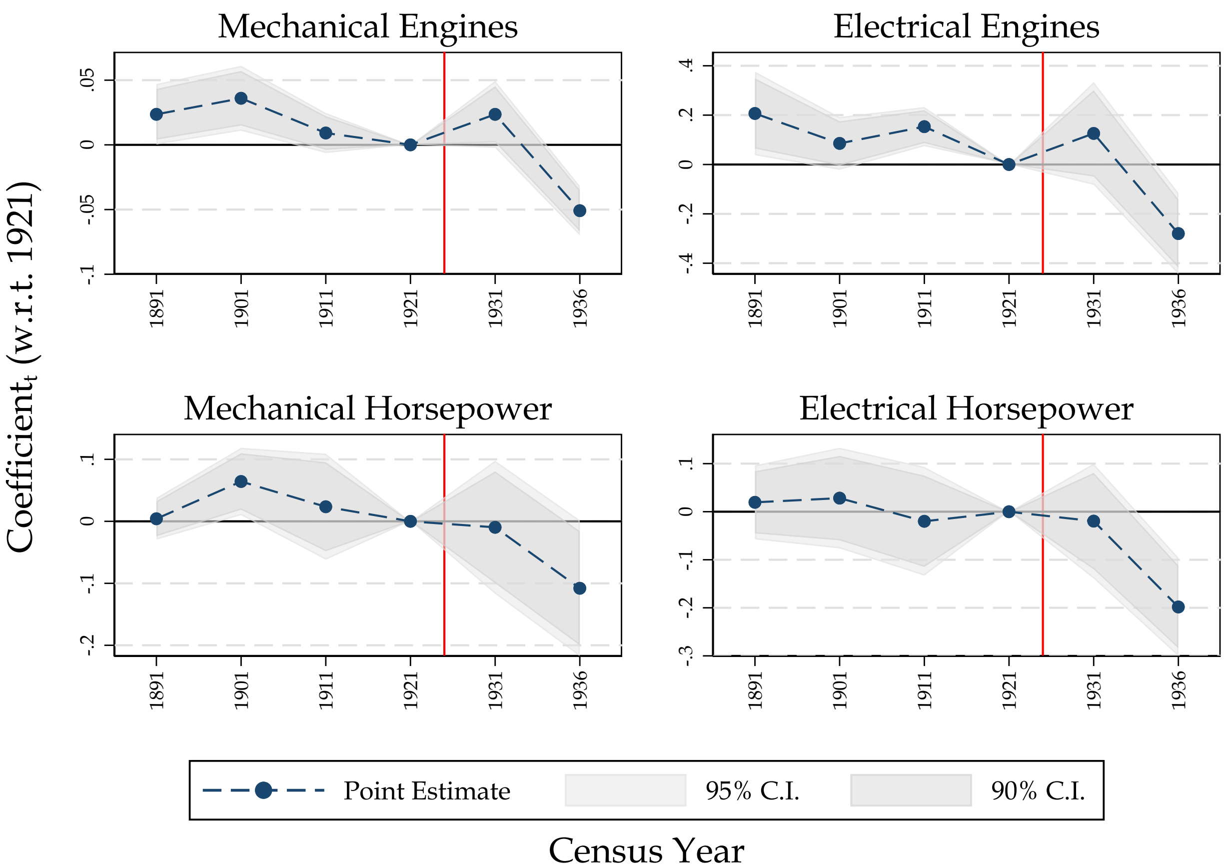
Notes. This figure plots the coefficient of the treatment measure (QE) interacted with census-decade time dummies. Regressions include district and year fixed effects, and region-by-year fixed effects. Further controls are the population in level, and 1901 labor market slackness interacted with census-decade 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.

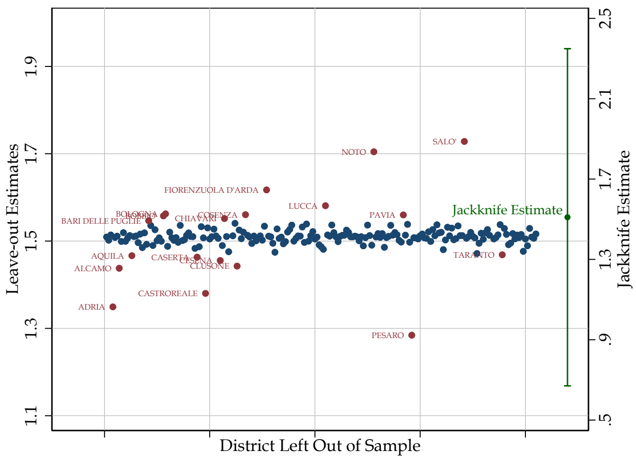
Figure C.3: Event-Study of Technology Adoption and Capital Investment



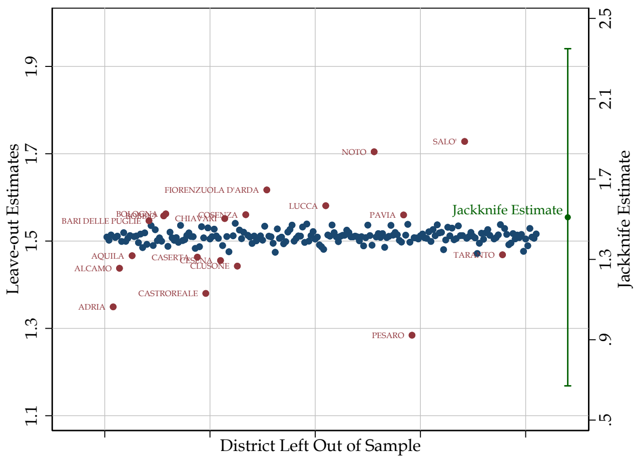
Notes. This figure plots the coefficients of the treatment measure (QE) interacted with census-decade time dummies. Regressions include district and year fixed effects, and region-by-year fixed effects. Further controls are the population in level, and 1901 labor market slackness interacted with census-decade 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.

Figure C.4: Jackknife Estimation Routine

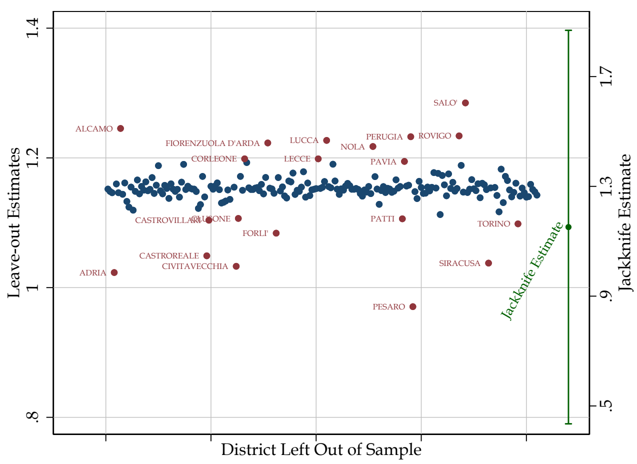
(a) Population Growth



(b) Industrial Employment

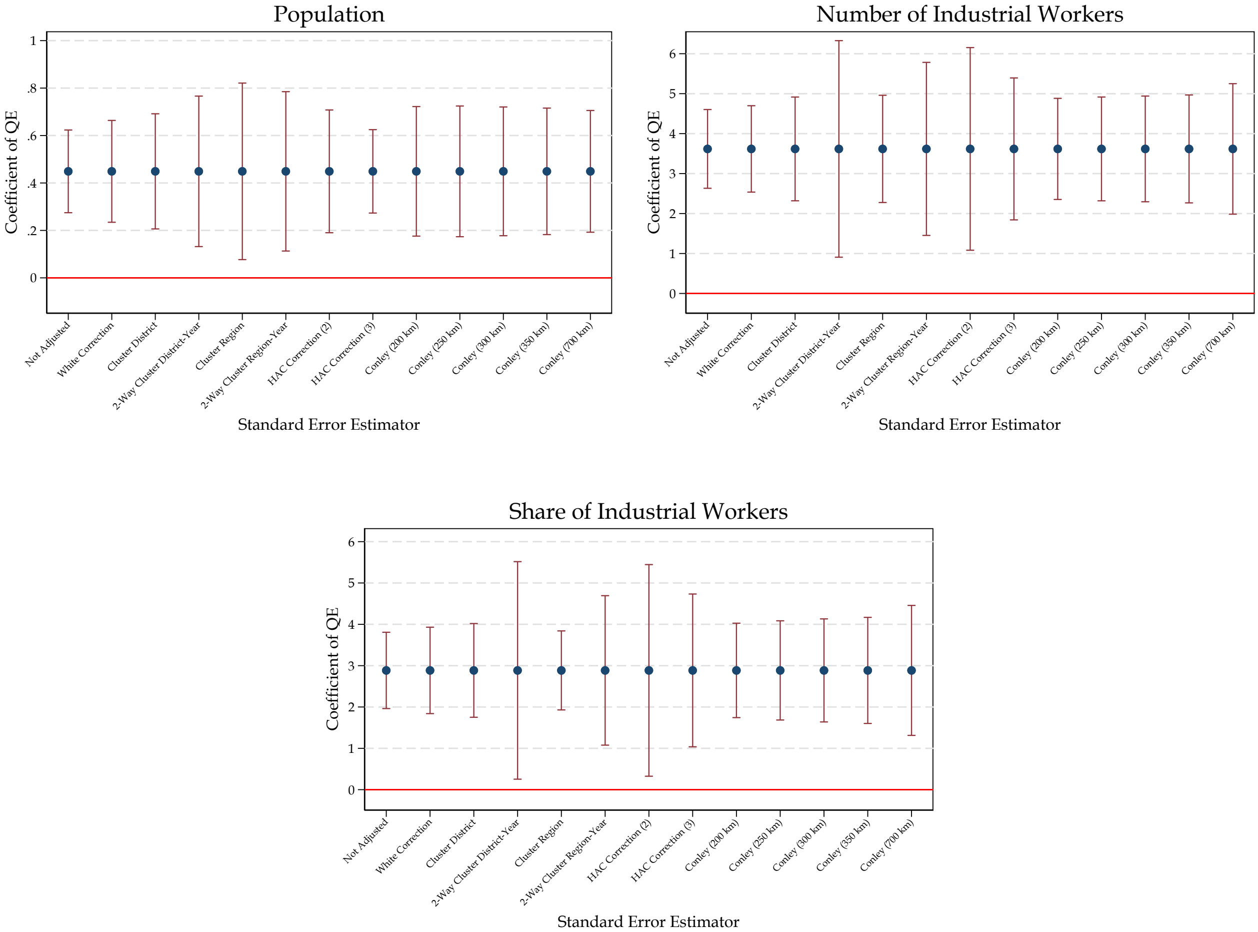


(c) Share of Industrial Workers



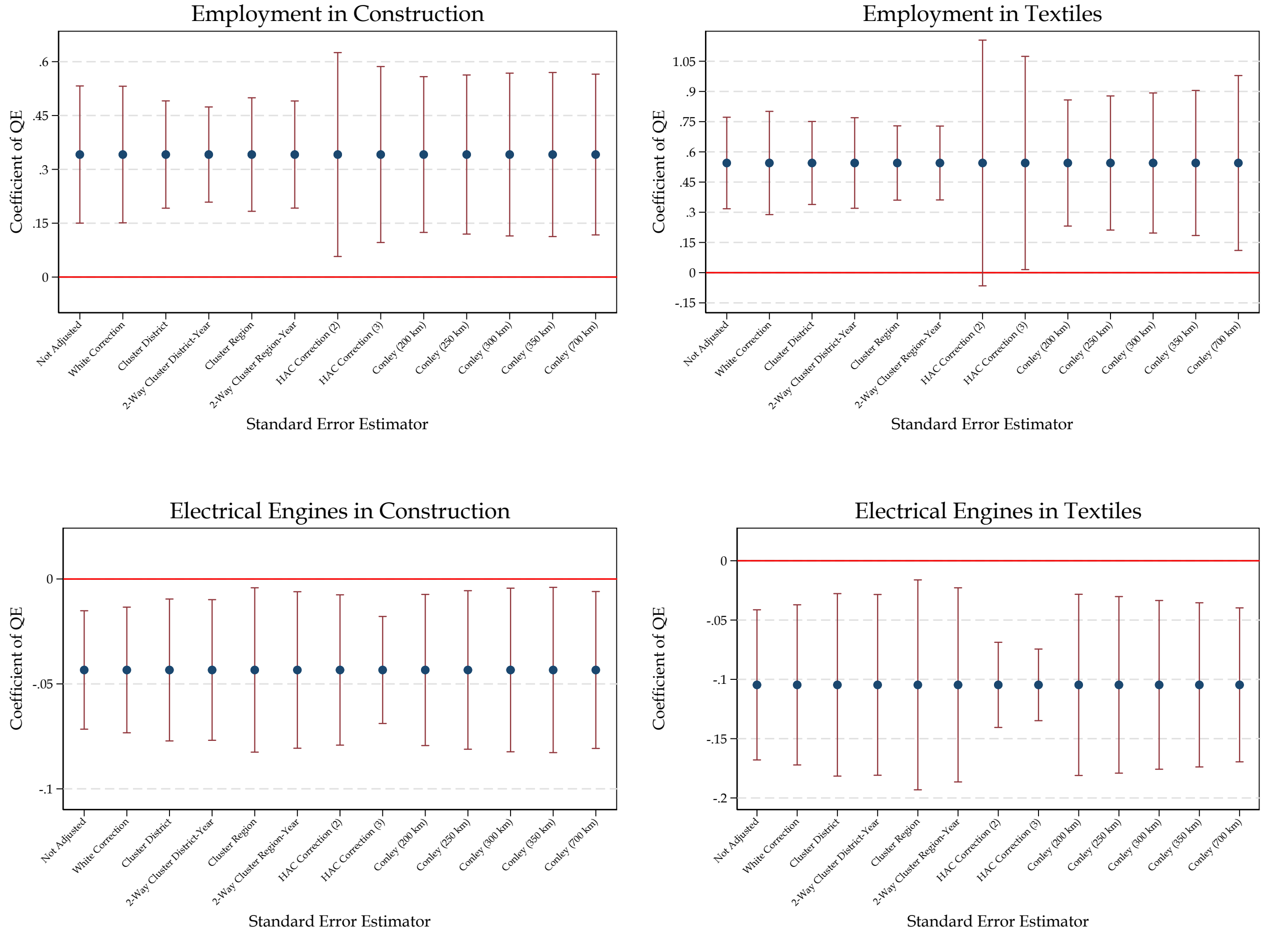
Notes. For each dependent variable shown in the header, each blue dot (on the left y-axis) reports the coefficient of Quota Exposure in the baseline difference-in-differences model dropping one district at a time. Red dots (on the left y-axis) are coefficients above and below respectively the 95th and 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



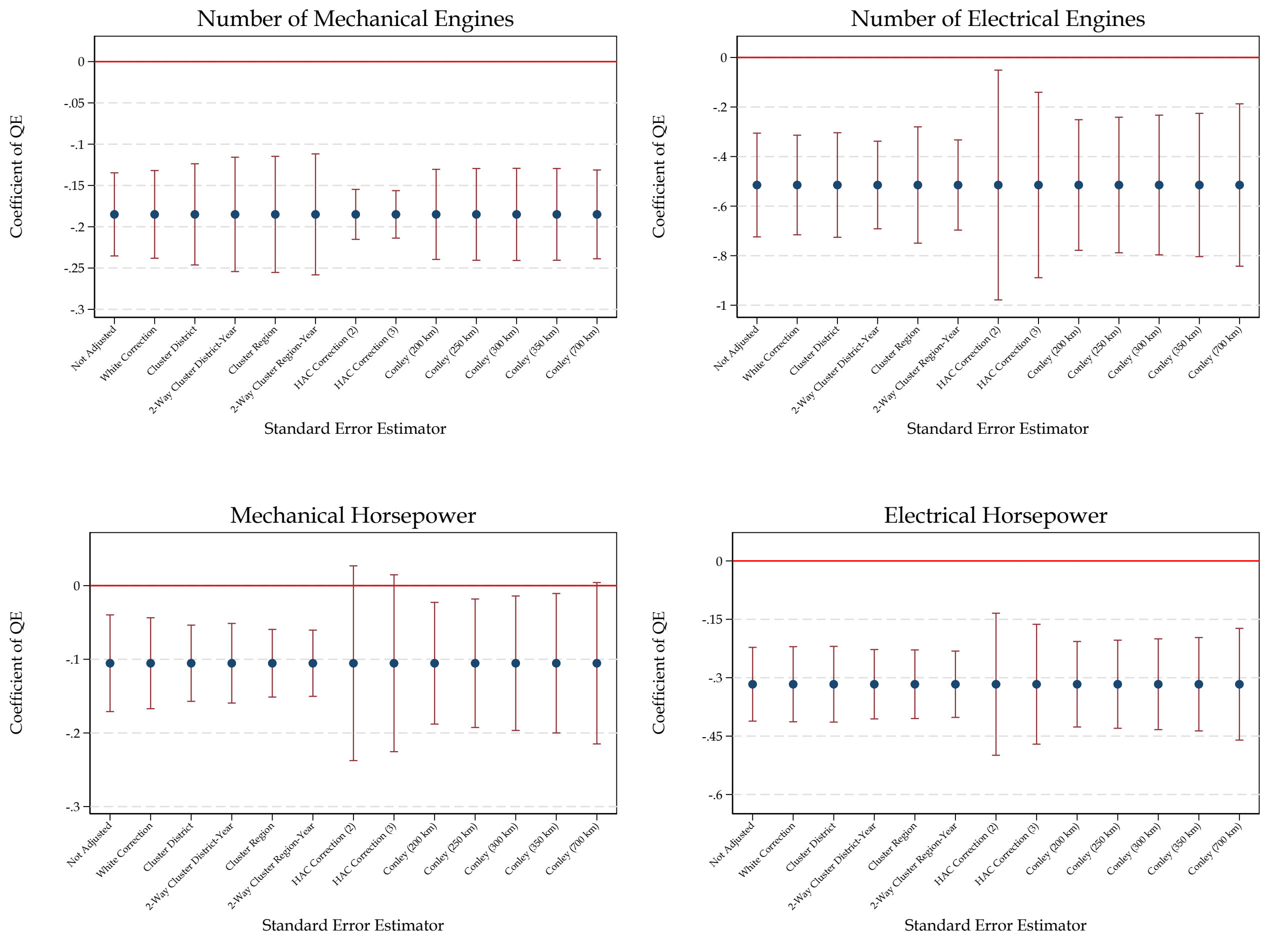
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard 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



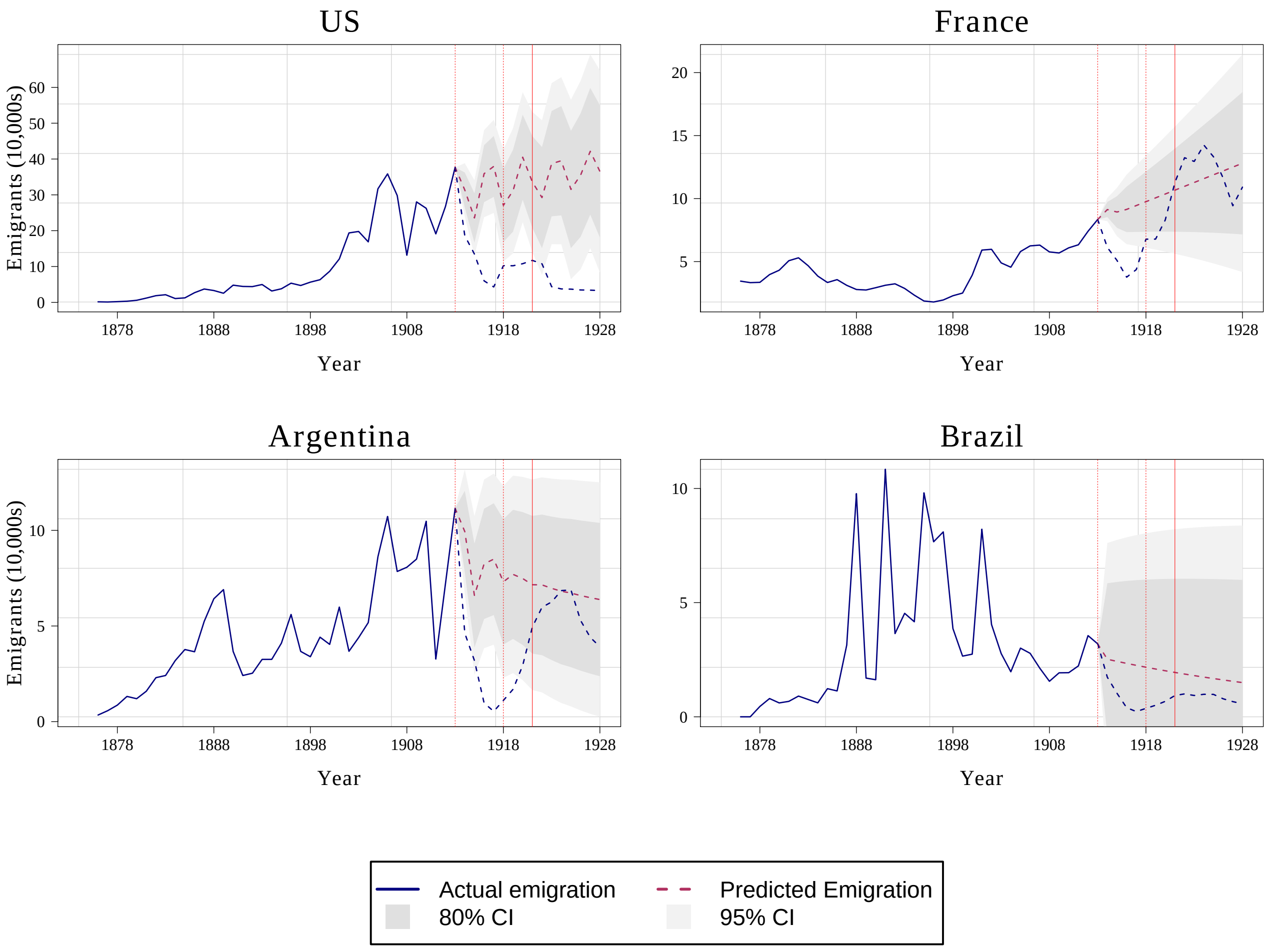
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard 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



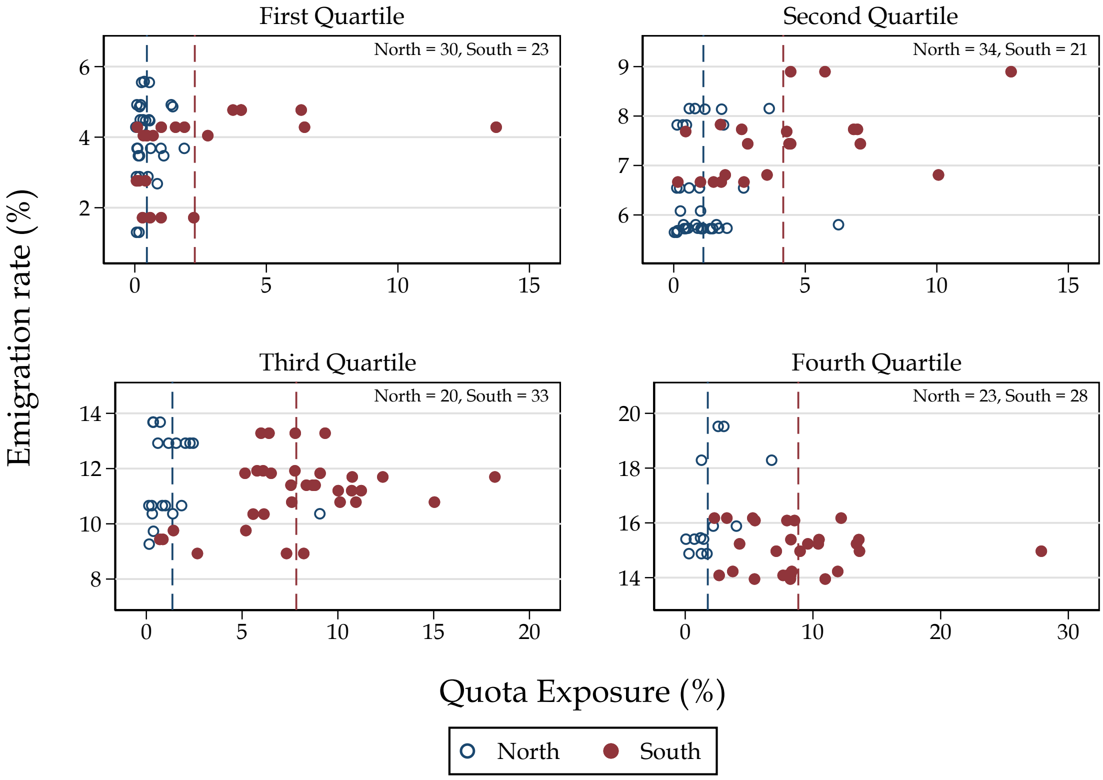
Notes. For a given outcome variable, the blue dots report the estimate of the coefficient of the treatment (QE) in the baseline difference-in-differences specification. The red bands report the 95% confidence intervals for a set of estimators for the coefficient's standard error. We include White standard 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.

D Model: Proofs of main results

In this section we provide the proof of the analytical results we stated in section 3. We refer to the main text for the statement of each result.

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

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

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

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

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

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

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

Plugging (3a)-(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 (3a) into the objective function (5). □

Proof of Lemma 3.1. From (6) and (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 $mw \le \psi(\iota^*)/\alpha$. 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 $m \ge 1$. 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 $\psi(\iota^*)/\alpha = w m^{1/\alpha}$. Because $m \in (0,1)$, $m^{1/\alpha} < m$ since $\alpha \in (0,1)$, and therefore $\psi(\iota^*)/\alpha = w m^{1/\alpha} < wm$. This implies that the solution is acceptable. Hence, $p(\iota^*) = \psi(\iota^*)/\alpha$ and this concludes the proof. □

Proof of Proposition 3.1. Because $w(j) = w$ for all $j \in [0,1]$, from (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 3.1, it is $w = \psi(\iota^*)/(\alpha m^{1/\alpha})$. Plugging this into the orevious 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. □

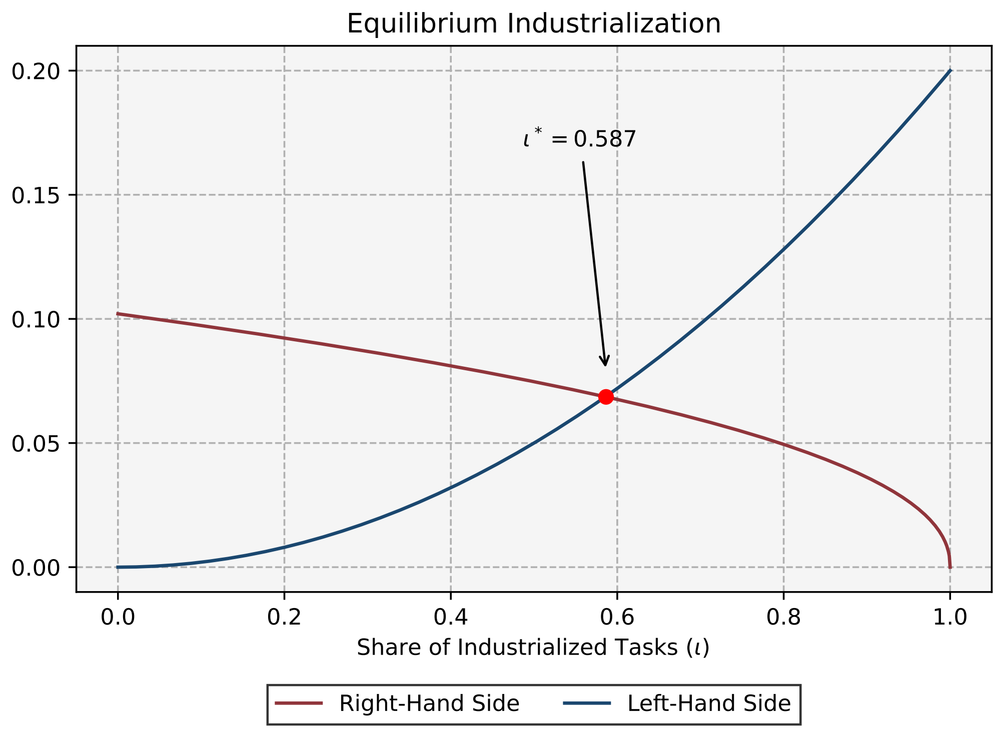


FIGURE D.1: This figure plots the equilibrium of the model. The blue and red lines respectively display the left and right-hand side of the final equation of the proof of Proposition 3.1. We assume $\psi(j) = \gamma j^2$ even though quadratic costs do not verify the Inada conditions. Parametrization: $\alpha = .55$, $\beta = .45$, $\gamma = .2$, $A = .5$, $L = 1$, $m = .5$.

Proof of Implication 3.1. From Lemma 3.1, 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^*. □

Proof of Implication 3.2. First note that because w is invariant across tasks, then by (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 (3b), yielding

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

Using Lemma 3.1 into the previous equation we get

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

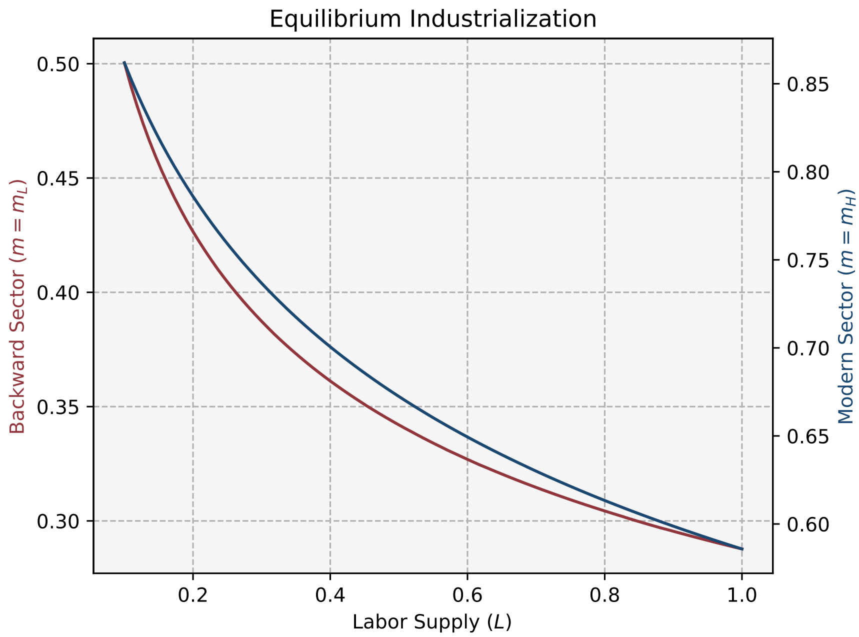
$$L^{1-\alpha} = \frac{(1-\iota^*)^{1-\alpha}}{\psi(\iota^*)} \alpha^2 A m^{1/\alpha}$$

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. □

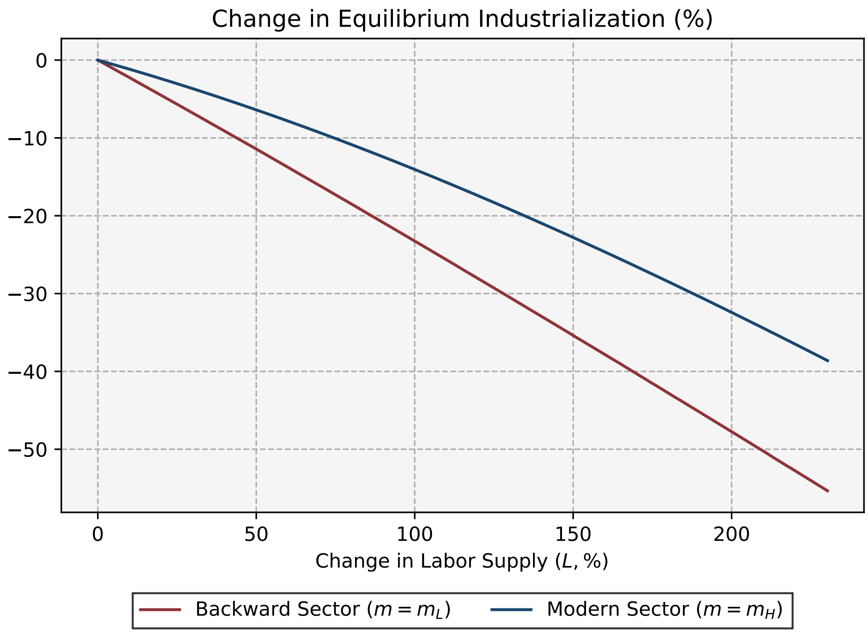
Proof of Implication 3.3. Let $m_M > m_B$. From the previous proof, we have

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

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. □



(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 blue and red 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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