Home Sweet Home: the Effect of Sugar Protectionism on Emigration in Italy, 1876-1913*

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Abstract

Protectionist policies have often relevant effects at the regional level. This paper uses the introduction of import duties on sugar in Italy in the late nineteenth century to measure the impact of protectionism on migration outflows at the time of the first globalization. Both for climatic reasons and the nature of the soil, the cultivation and processing of sugar beets was geographically concentrated in a small area, leading *de facto* to a regional protectionist policy. Our theoretical model illustrates how a tariff that favours local producers may affect residents' incentives to migrate abroad. Using a new set of historical data, the predictions of the model are empirically tested through quasi-experimental methods which use the exogenous variation in sugar cultivation across areas to estimate the effect of interest. Our results show that protectionism effectively reduced the relative incentive to migrate away from sugar-producing areas.

Keywords: protectionism, regional economics, migrations, 19th century Italy, difference in difference.

JEL Classification: N93, J4, C23.

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1 Introduction

The use of protectionism as a barrier to increasing globalization is not new in history. The second half of the nineteenth century was characterized by considerable movements of international resources, favoured by the reduction of transport costs induced by the diffusion of railways and steamship navigation. On the one side, to use the words of Hatton and Williamson (1998), it was the age of mass migrations. On the other side, rising transoceanic trade was a central factor for globalization. Most European countries, including Germany and Italy, reacted to the first globalization with protectionist policies. Comparisons between historical and contemporary waves of globalization and protectionism have already been made in the literature (Hatton and Williamson, 2005; Rapaport, 2016; Federico and Tena, 2017). In this perspective, the analysis of historical episodes of the past can contribute to our understanding of current socioeconomic issues (Livi Bacci, 2018), also by providing cases which allow to examine how national policies have different impact on different sub-national areas.

This paper investigates the relation between protectionist policies and migration outflows in Italy during the second half of the 19th century. Much like countries of large emigration today, nineteenth century Italy was a backward country, mainly based on agriculture, with poor infrastructures, and largely illiterate. At the early stage of the demographic transition, with high birth and death rates, the young and fast-growing Italian population took part in the age of mass migrations over the second half of the 19th century, when millions of Italians moved to the New World (Sori, 1979; Gomellini, and Ó Gráda, 2013; Ardeni and Gentili, 2014).[1] According to the historical population censuses, the Italian population increased roughly from 25 to 35 million of individuals during 1861-1911. At the same time, about 14 millions of Italians left the country, at least temporarily, during 1876-1913 (Sori, 1979, p. 21).

In this study we focus on the take-off of the domestic sugar beet industry following the adoption of prohibitive duties on imported cane sugar. Both for geographical reasons (latitude, nature of soil,

[1] The internal composition of Italian emigration in terms of destination countries shifted over time. After an early dominance of European countries, starting with the mid-1880s the Americas represented the preferred destination (Faini and Venturini, 1994a). According to Hatton and Williamson (1998, p. 101) at the eve of WWI about 60% (40%) of Italian emigrants chose to move to the Americas (Europe). European destinations were mostly preferred by Italian emigrants from Northern regions.

and climatic factors) and for the high perishability of the sugar beet, cultivation and processing were concentrated in a few neighbouring provinces of the Center/North-East Italy. The policy on sugar, established in principle at the national level by the Italian government, was *de facto* a regional protectionist policy. Thus, some areas gained from the tariffs on sugar, while the others were excluded from their benefits. Our research design uses the variation in sugar cultivation across areas to estimate the effect of protectionism on migration outflows.

The local effects of protectionist policies - as well as the impact of trade-liberalization - have been recently considered in the literature. Some papers have considered the asymmetric local effects of liberalization on industrial agglomeration (Lu and Tao, 2009), on regional specialization (Bai, Du, Tao, Tong, 2004), on the local formal sector employment (Dix-Carneiro and Kovak, 2017) and on local school attendance and child labour (Edmonds, Pavcnik and Topalova, 2010). In particular, Topalova (2010) considers the impact of trade liberalization on district-level poverty, showing that the Indian districts that were more exposed to tariff cuts exhibited slower progress in poverty reduction. By contrast, the regional consequences of protectionism are largely overlooked in the economic history literature, partly because of the lack of data at the subnational level. Among the few exceptions, Tirado et al. (2013), building on Hanson (1997), consider the effects of protectionism on regional economic activity in Spain during the interwar period (1914-1930). They show that protectionist policies were detrimental to the region of Barcelona and favoured more central locations. Our paper aims at expanding this branch of what we might call "historical regional economics literature".

We first present a theoretical model that relates tariffs protecting local products to residents' welfare, and their incentive to migrate. Standard economic theory predicts that the implementation of tariffs has negligible effects on local living conditions, provided that production factors are perfectly mobile: as long as local protectionism tends to raise local wages, additional workers will flow into the area (see, for example, Topalova, 2010). By contrast, our model explicitly shows that when labour mobility is imperfect,[2] protection of local goods has permanent effects on local wages

[2] The assumption of imperfect workers' mobility seems reasonable for 19th century Italy, characterized by illiterate people often speaking local dialects and a national railway network still under construction. Ciccarelli and Weisdorf (2019) illustrate regional trends in literacy rates during 1821-1911. Ciccarelli and Groote (2018) document the development of the railway network during 1839-1913. Both studies focus on the Italian provinces. Evidence of

and, thus, it reduces the incentive of residents to migrate.

The predictions of the model are tested using annual data on Italian emigration at the provincial level (NUTS 3 units) over the period 1876-1913. The identification strategy is similar to the Difference in Difference (DID) approach in Topalova (2010), where the impact of tariffs is captured by exploiting differences in local industrial composition and, thus, by differences in local exposure to the policy change. Here, to assess the effect of protectionism on provincial migration rates, we start by a standard static DID specification. Our findings provide evidence in favor of the model's predictions: the birth of the local sugar industry reduced emigration in recipient provinces. However, protectionism might have generated important dynamic effects that cannot be captured by the standard DID model. For this reason, we resort to a dynamic specification and find that the negative impact on migration - exerted by the newly-born sugar industry - actually grew in size over time.

To add further support to our findings, we also exploit matching methods and Synthetic Control Method. Both methods build on the idea of comparing the outcomes of the treated with those of the untreated units, where the untreated are matched to the treated following some criteria of similarity, such as similarity of covariates or propensity score. Using these two alternative quasi-experimental methods, we find robust evidence that the introduction of protectionism and the upsurge of the national sugar industry reduced relative migration rates in the beneficiary provinces.

The paper has the following structure. Section 2 describes the sugar trade policy and the development of the Italian sugar industry. Section 3 presents a model relating tariffs to local real wages, which theoretically shows how protectionism may affect the incentive to migrate. Section 4 discusses the empirical methodology adopted. Section 5 describes the data, and Section 6 presents the empirical results. Section 7 concludes.

2 Historical background: the Italian sugar industry

This section reviews the Italian trade policy on sugar, the rapid development of the national sugar industry in the late 19th century, and its geographical location. We also document the dimension

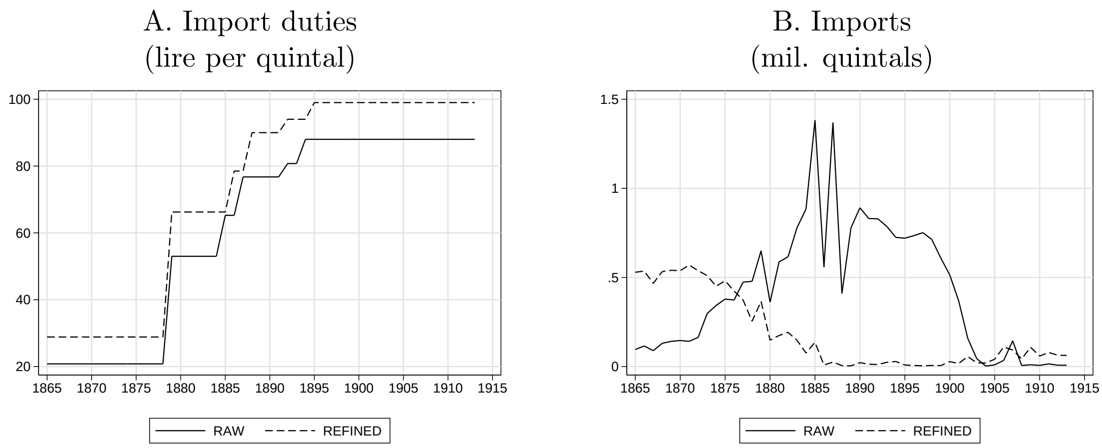
limited internal mobility in 19th century Italy is also provided in Federico et al. (2019).

of the sugar industry and its possible impact on the local economy.

2.1 Sugar trade policy

Figure 1 illustrates the evolution of import duties on sugar (Panel A) and the quantities imported from abroad (Panel B). Panel A reports two step functions with jumps at the time of tariff reforms on sugar (the main ones occurred in 1878, 1887, and at the end of 1894).[3] The continuous line refers to raw sugar, the dashed one to refined sugar. The vertical difference shows the protection guaranteed to sugar refiners. Three regimes emerge rather neatly. A first period of low protection (1865-1878), a second one of transition to protectionism (from 1878 to 1895), and a final period of high protection (1895-1913). From 1895 to the end of our sample period the import duties on sugar were unchanged. We will refer to the “sugar reform of 1895” to denote the last of a series of tariff increases that took place between 1878 and 1895.

Figure 1: Sugar: import duties and imports, 1865-1913



Source: Data on imports of raw and refined sugar, and import duties have been collected by the annual publication Ministero delle Finanze, *Movimento commerciale del regno d'Italia*, the official source on Italian commercial flows (see Appendix A for details).

Panel B illustrates the evolution of sugar imports over time. After a decade of stability, the imports of refined sugar declined rapidly to reach nearly zero in 1885. This trend was mirrored by

[3]In the reference period 1876-1913, the last law raising import duties on imported sugar was approved by the Royal Decree no. 532 on the 10^{th} of December 1894. The full list of laws on the Italian sugar industry approved during 1846-1945 is reported in Sabbatucci Severini (2004), pp. 264-265.

imports of raw sugar (to be refined in Italy), which increased over the period 1865-1885. The mid-1880s peaks in raw sugar imported by Italian refiners can be related to the expected increases in import duties. Since the early 1890s, the imports of raw sugar declined, with a rapid drop at the end of the century (see also Robertson, 1938; Eridania Zuccherifici Nazionali, 1949). Overall, Figure 1 shows that the tariff on sugar started as a fiscal duty to become more and more a protectionist duty. Around the turn of the century the domestic production of sugar beet became, as we will document, a profitable business.

2.2 The birth of the domestic sugar industry

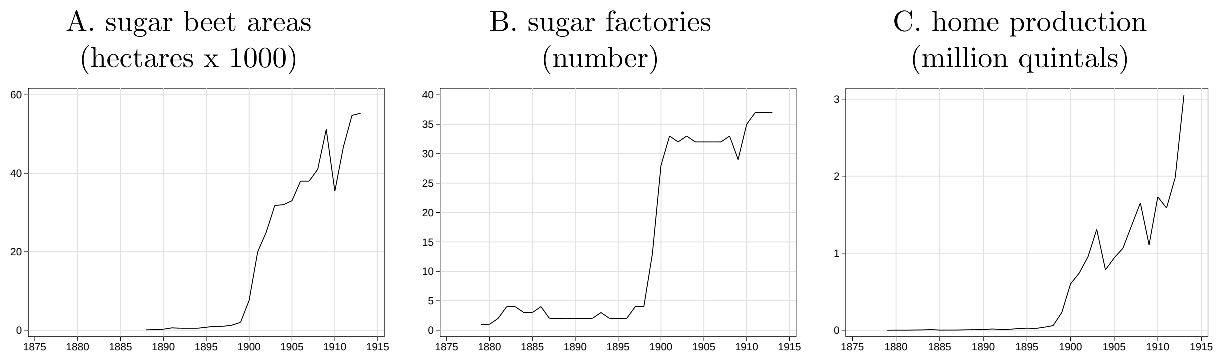
In the session of 17 November 1881, the Italian Chamber of Deputies voted a motion asking the Italian Government "to support, in every possible way, the development of the national cultivation of sugar beet" (Camera Deputati, 1881, p. 7145). As reported by the Ministero di Agricoltura Industria e Commercio (1882): *i)* being sugar beet highly perishable, its cultivation and subsequent processing had to be done in the same area (p. 6); *ii)* the development of the sugar industry was not suitable for the South of Italy (pp. 8-9); *iii)* the Po Valley, especially its lower part, represented an appropriate region for the development of the sugar industry (pp. 37, 48; see also Maccaferri, 1883; Peglion, 1917, pp. 90, 99-104; Cardoza, 1979, p. 178).

Figure 2 documents national trends in the sugar industry. Panel A illustrates the agricultural expansion of sugar production, showing that the areas where sugar beet was cultivated rose from a few hundreds to about 55 thousand hectares over the period 1890-1913. Panel B refers to sugar factories, the industrial plants where sugar beet was processed. With a neat jump in the last years of the century, the number of sugar factories reached about 35 units in 1913. The take-off of the sugar industry followed the consolidation of protectionism after 1895, with a short, yet inevitable, time lag required to build the factories and set up the business.[4] Finally, Panel C illustrates the amount of sugar produced in Italy. Its profile largely reproduces the one illustrated in Panel A for the sugar beet areas. The domestic production of sugar peaked in 1913, when it reached some 3 million quintals. Depending on the conditions of the market, the annual production was sold in

[4]Sabbatucci Severini (2004), in her carefully documented book, has indeed a chapter on the "1898-1914 take-off of the (Italian) sugar industry".

the national market or allotted to stock accumulation, being exports extremely limited in size.

Figure 2: The growth of the Italian sugar industry, 1880-1913



Source: Panel A: Perdisa (1938), p. 16 and Sabbatucci Severini (2004), p. 268; Panels B and C: data for the years 1879-1894 are from Ministero delle Finanze, *Statistica delle fabbriche [...]*; data for the years 1895-1913 are from Ministero delle Finanze, *Statistica delle tasse di fabbricazione*. See Appendix A for additional details.

2.3 The geographical concentration of the sugar industry

Figure 3 illustrates the geographical distribution of the sugar beet areas and the location of the industrial plants at the end of our investigation period. The vast majority of the sugar beet areas lies in the lower part of the Po Valley, as illustrated in Panel A by a dark triangular zone, the "Italian sugar triangle". There are also some scattered areas of sugar beet cultivation, mainly located in the Centre of Italy.[5] Each of the 35 dots reported in Panel B represents a sugar factory. The Po river, flowing eastward across Northern Italy, is also identifiable. The factories are always located in the proximity of the cultivated fields.[6] Since sugar beet is a perishable product, "the beets are hurried from the farms to the factory and at breakneck speed turned into sugar" (Nelson Evening Mail, 24 October 1910). The spatial correlation between the two sides of the picture, representing respectively the agricultural (left-panel) and industrial (right-panel) side of the sugar industry, is crystal-clear.

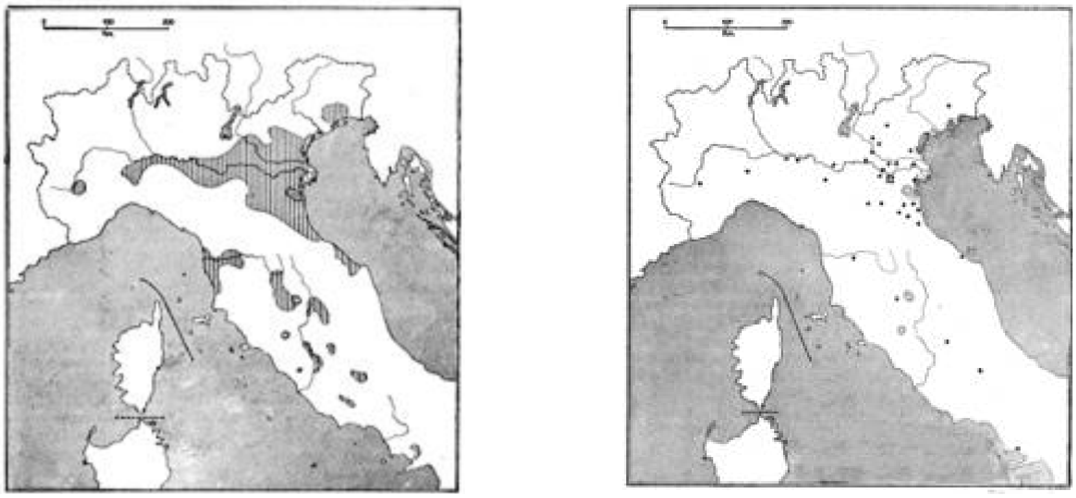
As we will explain in more detail later, the provinces belonging to the "Italian sugar triangle"

[5]Italy's lies between latitudes $36^o N$ and $47^o N$ ca, within the usual lower and upper bound for sugar beet cultivation ($30^o N$ to $60^o N$ ca). The cultivation of sugar cane instead, has a polar upper boundary of $39^o N$ (Grigg 1995, p. 23) and is thus less suitable to Italian latitudes.

[6]For a detailed analysis see Gambi (1955, pp. 85-89) and Robertson (1938).

Figure 3: Sugar beet areas and factories, 1910-12

A. sugar beet areas B. sugar factories



Source: Gambi (1955).

form the "protected area" in the theoretical model, and the "treated group" in the empirical exercise. We deliberately exclude from the protected area/treated group the few provinces characterised by a reduced and discontinuous production of sugar over time.

2.4 The dimension of the sugar industry and its impact on the local economy

The historical literature documents the size of the Italian sugar industry and the economic conditions, including wages, of its employees. According to Montemartini (1912) at least 5,000 workers were employed throughout the year, and more than 10,000 over the August-October period. In September 1910, at its highest peak, employment in sugar factories amounted to some 15,500 individuals, of which 95% were adult men. Exact data on sugar beet growers are not available. However, Montemartini (1912) reports that one hectare of cultivated land required about 125 working days. Therefore at the end of our sample period, when the sugar beet area covered more than 50,000 hectares, about 17,000 individuals were possibly active during the sugar beet season. Using data from each of the 38 sugar factories and refineries existing in Italy in 1910, Borgnino (1910) confirms these figures and notices that sugar factory workers were also recruited among sugar beet growers.[7] Thus, perhaps some 30,000 workers were directly or indirectly involved in the sugar industry throughout the year. If we compare these employment figures to the total male population of age 15 and above in 1901 in the twelve provinces of the sugar triangle (about 1,2 million people), we can see that the dimension of the sugar industry was not particularly large. Nevertheless, the spillovers generated by the birth of the Italian sugar industry were not negligible. For instance, a wide variety of new by-products such as pulp and molasses not transformed into sugar were exploited to feed livestock, especially over the winter season. These effects, however, are hard to quantify for the historical period at hand.

Historical sources also show that wages in the sugar industry were particularly high, reaching 7 lire per working day. By comparison, the daily wage of a master builder in 1910 varied between 2 and 5 lire, depending on the province considered (Direzione generale della statistica e del lavoro, 1912, p. 222). Gambi (1955, p. 81) calculates that, despite the seasonal nature of their activity, sugar refineries provided employees with more than 50% of their annual income.

3 A model with tariffs and regional migrations

In this section, we sketch a simple model to analyze the incentive to migrate as a function of local labour market conditions. In particular, we look at the role of tariffs on foreign imports of agricultural commodities, like sugar in our case, to assess how protectionism may have different effects across different national areas on the incentive to migrate abroad. We show that tariffs on imports reduce the incentive to migrate for workers employed in the protected sector. By contrast, tariffs can increase the incentive to migrate for workers that are employed in non-protected sectors. Thus, since territories largely specialize in certain productions, tariffs are a source of variability across space.

[7] This point is also made in Robertson (1938) when referring to the Italian sugar industry. The author noticed that "The erection of a beet-sugar factory affects the agriculture within a certain radius [...] somewhat similarly to the bringing nearer, by transport improvements [...] of a large market. The beet-sugar factory provides a market practically on the spot for a new cash crop that can stand increased capital and labor costs" Robertson (1938, p. 14). Ciccarelli and Fenoaltea (2013) document the rapid industrial growth experienced during the pre-WWI decade by the provinces of the Po Valley. Interestingly, the authors relate it to the boom of the Italian sugar industry.

3.1 The basic model

In the model we outline below individuals decide among three alternatives. They choose whether to stay in the area where they were born, or to move to another province in the same country, or to migrate abroad. Domestic firms are competitive and may, or may not, operate under the protection of tariffs.

The analytical derivation of the main results reported in the text is provided in Appendix B.

3.1.1 Workers

Space is partitioned in three areas, indexed by $C = \{s, ns, f\}$. Areas $\{s, ns\}$ are provinces belonging to the same country, while the area denoted by f is abroad. In what follows, the index s will denote the province which specializes in the production of the protected good, while the index ns denotes the province which produces non-protected goods. The natives in the domestic provinces $\{s, ns\}$ are given, respectively, by N_s and N_{ns}.

The utility of a worker who lives in $c = \{s, ns\}$ has the following Cobb-Douglas form:

$$U_c = \mu \cdot \ln q + (1 - \mu) \cdot \ln Q + \chi_c \tag{1}$$

where $\mu \in (0, 1)$, q denotes the consumption of the protected good (sugar, here), and Q denotes the consumption of other goods, not protected from foreign competition. The idiosyncratic preference shock for location c, denoted by χ_c , is identically, independently distributed as a Type I Extreme Value (see, e.g., Diamond, 2016, and Kline and Moretti, 2014a). Thus, for any given consumption bundle $\{q, Q\}$, individuals derive different levels of utility depending on where they live. As we will emphasize in what follows, heterogeneity in local preferences also implies that the local labour supply is not perfectly elastic to changes in local wages. In other words, we explicitly deviate from models where the labour factor is perfectly mobile across areas.

The workers who decide to live in province $c = \{s, ns\}$ attain a utility level which is given by maximizing (1) subject to the budget constraint:

$$w_c = p \cdot q + p' \cdot Q \tag{2}$$

where w_c denotes the nominal local wage, p and p' the price of the protected and unprotected goods, respectively. Prices (p, p') are assumed to be the same in both the provinces $\{s, ns\}$ of the country considered.

Protectionist policies set tariffs on imported goods. In particular, tariffs are such that

$$p = p^* + \tau \tag{3}$$

where p^* denotes the international price of the good considered (sugar), and $\tau \geq 0$ the per-unit tariff on imports. The domestic aggregate price level is given by

$$P = p^{\mu} \cdot \left(p'\right)^{1-\mu} . \tag{4}$$

Maximization of (1) under (2) yields $q = \mu \cdot \frac{w_c}{P}$ and $Q = (1-\mu) \cdot \frac{w_c}{P}$ which, once substituted back into (1), give the following indirect utility function for $c = \{s, ns\}$:

$$V_c = \ln \theta + \ln \left(\frac{w_c}{P}\right) + \chi_c \equiv \upsilon_c + \chi_c \ , \tag{5}$$

where we define $\upsilon_c \equiv \ln \theta + \ln \left(\frac{w_c}{P}\right)$, and $\theta \equiv \mu^{\mu}(1-\mu)^{1-\mu}$.

On the other hand, the level of utility which can be attained by migrating abroad (area f) is given by:

$$V_f = \ln \theta + \ln \left(W_f - t\right) + \chi_f \equiv \upsilon_f + \chi_f \tag{6}$$

where $W_f - t$ is the (exogenously given) real wage abroad, $\frac{w_f}{P_f}$, net of transport costs that are to be borne to migrate, defined by $t \geq 0$. The preference shock χ_f has the same distribution as χ_c.

As shown in Appendix B, when individual mobility is not perfect and liquidity constraints are not binding, the rate of migration from area s, defined as the number of migrants, m_s, relative to the local labour supply n_s will be given by:

$$\rho_s \equiv \frac{m_s}{n_s^s} = \frac{\exp\left\{\frac{v_f}{\phi}\right\}}{\exp\left\{\frac{v_s}{\phi}\right\}} \cdot \frac{N_s}{N_s + N_{ns}}. \tag{7}$$

An expression analogous to (7) holds for area ns.

3.1.2 Producers

We now characterize the behavior of producers. We postulate that producers are price-takers for what it concerns output. Also, local labour markets are competitive.

Consider first the behavior of producers located in the domestic province s, specialized in the production of a good protected by tariffs, like sugar in our case. Producers maximize profits, $\Pi_s = p \cdot q - w_s \cdot n_s - r_s \cdot L_s$, where p is given by (3), subject to a constant returns to scale Cobb-Douglas technology $q = (L_s)^\alpha \cdot (n_s)^{1-\alpha}$, where $\alpha \in (0, 1)$ and L_s denotes the local land input, inelastically supplied in quantity $\bar{L}$ at price r_s. As shown in Appendix B, residents' utility maximization, together with producers' profit maximization allow to characterize labour market equilibrium in province s.

Next, we analyze what happens in province ns, which produces goods that are not protected by tariffs. Producers located in province ns, the domestic province specialized in the production of goods that are not protected by tariffs, maximize profit $\Pi_{ns} = p' \cdot Q - w_{ns} \cdot n_{ns} - r \cdot L_{ns}$ subject to a Cobb-Douglas technology $Q = (L_{ns})^\alpha \cdot (n_{ns})^{1-\alpha}$, where L_{ns} denotes the local land input, inelastically supplied in quantity $\bar{L}$ at price r_{ns}. Again, Appendix B illustrates labour market equilibrium also for province ns.

3.1.3 Equilibrium

We can now turn to the impact of tariffs on the decision to migrate abroad. By using (7) and the analogous expression for area ns, we can analyze the relative behaviour of migration rates across domestic provinces, the main object of the empirical analysis. Additional detail is provided in Appendix B.

The relative migration rate between area s and area ns is given by:

$$\ln \rho_s - \ln \rho_{ns} = \ln\left(\frac{N_s}{N_{ns}}\right) - \frac{\alpha}{\alpha+\phi}\left[\ln(p^* + \tau) - \ln p'\right] \tag{8}$$

Expression (8) emphasizes the impact of the tariff on the relative migration rate between the two domestic provinces. As shown in Appendix B, the tariff changes relative local wages in favour of the protected areas. As a consequence, the implementation of a tariff which benefits area s will reduce the incentive of its residents to move abroad, relative to provinces which do not benefit from protectionism. These conclusions are summarized in the following:

Result. The application of tariffs on imports will discourage migration abroad from areas specialized in protected products, while encouraging migration from areas specialized in non-protected products.

There is another way to look at the Result above. Tariffs increase the cost of living, as emphasized by the price level (4). Thus, in provinces where non-protected sectors dominate, real wages will fall, raising the incentives to migrate. Notice also that the expenditure share on protected goods, μ, is equal to the elasticity of the cost of living P to the price of the protected good, $p^* + \tau$. As a consequence, the larger μ the larger the adverse impact on local wages. By contrast, in provinces where protected sectors dominate, nominal wages will increase more than the cost of life, so to discourage migration. Also, as shown in Andini et al. (2017), a reduction in local migration will imply an increase in welfare for all the residents.

How important was the adverse effect induced by the higher price of sugar for workers outside the sugar industry? The answer crucially depends on the expenditure share on sugar, μ. Table 1 documents the average per capita consumption of sugar in selected countries during 1881-1913. The table shows that the average per-capita consumption of sugar in Italy was almost ten times below the one in the UK and USA, and between four and five times below the corresponding levels in France, Germany and Norway.[8]

Table 1: Percapita sugar consumption in selected countries, 1881-1913 (kg/person).

Period	Italy	France	Germany	Great Britain	Norway	USA
1881-1885	3.2	10.7	9.1	27.0	5.5	20.9
1886-1890	2.3	11.4	7.9	28.8	6.5	23.0
1891-1895	2.4	11.8	9.9	31.9	9.2	29.1
1896-1900	2.4	11.0	11.7	34.3	13.6	28.1
1901-1905	3.0	13.0	13.6	33.2	16.1	32.2
1906-1910	3.9	15.3	17.1	35.5	17.8	35.5
1911-1913	4.8	17.5	18.4	36.7	19.3	37.0

Source: Authors' elaboration on Sabbatucci Severini (2004), p. 267.

We can thus argue that the expenditure share on sugar for Italian wage earners was extremely low. This is quite reassuring, since it allows us to consider workers outside the sugar producing areas as non-treated individuals, that is, as individuals who are largely unaffected by the implementation of sugar tariffs.

We can now summarize the main features of the model to address the empirical analysis that follows. First, the model suggests that the impact of protection can be gauged by simply looking at migrations rates, rather than looking at local wages as, for example, in Hanson (1997) and Tirado et al. (2013). Secondly, in the absence of liquidity constraints,[9] protection is such that workers in sugar producing areas will have lower incentives to migrate away, relative to workers in non-protected areas. The size of this effect is higher the smaller the expenditure share on sugar. Thirdly, since workers were likely to have a negligible expenditure on sugar, the impact of protectionism on workers' cost-of-living in non-sugar producing provinces was nil. To this regard, non-sugar producing provinces can be considered as suitable controls in the empirical analysis.

[8]Historical per-capita consumption of sugar in a comparative perspective is also discussed in Tonizzi (2001).

[9]Abramitzky et al. (2013) argue that liquidity constraints played a minor role during the age of mass migration, "an era when migration costs were relatively low" (p.2). What mattered most were "opportunities in the source country" (p.3), as our model suggests. By contrast, Armstrong and Lewis (2017, p.172) suggest that Italians used to face relevant liquidity problems, relative to migrants of other nationalities. Such issue, however, appears to be less relevant for the individuals located in the sugar-producing areas, where indirect measures of well-being such as "height" and "literacy" were, as documented later, above the national average. The case for binding liquidity constraints is sketched in Ciccarelli et al. (2018) and briefly discussed in Appendix B.

4 Empirical strategy

Our empirical goal is to assess the central implication of the model, that is, whether the implementation of protectionism in the sugar industry negatively affected the migration decisions of individuals living in sugar-producing provinces (in technical jargon, we want to estimate the "average treatment on the treated"). The starting point is to define the set of provinces exposed to the treatment. As mentioned in Section 2.3, both for geographical reasons and for its high perishability, sugar beet cultivation and processing were concentrated in a few neighbouring provinces of the Center/North-East, forming the "Italian sugar triangle". Our treated group, as illustrated in Figure 4, is therefore made of 12 provinces. They include the eight provinces forming the region of Emilia (Bologna, Ferrara, Forlì, Modena, Parma, Piacenza, Ravenna, Reggio Emilia), the two provinces of Cremona and Mantua in Lombardy, and the two provinces of Rovigo and Verona in Veneto.

Figure 4: Treated provinces

The treated provinces are Bologna, Cremona, Ferrara, Forlì, Mantua, Modena, Parma, Piacenza, Padova, Ravenna, Reggio Emilia, Rovigo, and Verona. Source: see text.

A simple comparison of migration patterns between provinces affected by sugar protectionism (the treated provinces) and the remaining provinces might be, however, potentially biased.[10] To overcome this identification issue and produce unbiased estimates of the effect of import duties on migration, we employ a DID approach which exploits the quasi-experimental framework provided by the change in the tariff regime on sugar which took place in 1895.[11] In particular, we compare the changes in migration behavior of individuals living in treated provinces (which benefitted from an increase in protectionism) to changes in migration behavior of individuals living in control provinces (those not affected by the sugar protectionist policy). The causal effect of protectionism on migration choices is identified by the least squares estimation of the following DID specification:

$$Y_{jt} = \alpha + \beta D_j + \gamma Post_t + \delta D_j \times Post_t + \mu X_{jt} + \lambda_j + u_{jt} \qquad (9)$$

where Y_{jt} represents the migration rate in province j at time t, D_j is a dummy that takes the value 1 if province j belongs to the treated group, the variable $Post$ is a dummy that takes the value one from 1895 on and zero otherwise. The coefficient β captures the permanent differences between the treated and controls. The coefficient γ captures the effect of time on Y, i.e. general changes in the economic and social context before and after the import duties reform. The interaction coefficient δ is the effect of interest. It captures the differential impact of the implementation of protectionism in treated provinces after the policy, compared with the remaining ones. We also control for a set of province dummies λ_j and various socioeconomic variables at provincial level X_{jt}, including census-based variables (population size and literacy rate), birth rate, height of Italian conscripts, railways endowment, and the share of industrial labor force (over total labor force in agriculture, industry, and services).

The key identifying assumption in the DID approach is that δ would be zero in the absence of the reform, meaning that, on average and conditional on X_{jt}, migration in the treated and the control

[10] If, for example, the economic boom of the pre-WWI decade had been, for whatever reason, more vigorous in the provinces that benefited from protection on sugar, they might have experienced a decrease in migration rates even in the absence of protectionism. More formally, unobserved differences correlated with migration behavior between provinces exposed to protectionism and the other provinces might bias the effect of the reform.

[11] Topalova (2010) employs a similar approach to measure the impact of the 1991 Indian trade liberalization on poverty, using the variation in sectoral composition across districts and differences in liberalization intensity across sectors.

provinces would have followed parallel trends in absence of the reform (parallel trends assumption). This implies that the linear DID can handle treatment endogeneity as long as the resulting bias has the same magnitude before and after the reform and, therefore, it can be differenced away (Heckman *et al.*, 1998; Heckman and Smith, 1999; Heckman *et al.*, 2006).

However, the assumption of a common trend might not hold in our context. For instance, all the provinces of the South belong to the control group but they were arguably very different from the treated provinces, as confirmed by a wide historical literature on the Italian regional divide (see for instance, Federico et al. 2019). The presence of a sizeable regional heterogeneity might make the common trend assumption questionable. For this reason, to further investigate this issue, we follow two additional strategies.

First, we use a dynamic specification as an indirect test of the common trend assumption (Autor, 2003; Angrist and Pischke, 2008); second, we employ compelling estimation methods to assess the robustness of our results, namely matching techniques and Synthetic Control Method (SCM, henceforth). In particular, we employ three matching estimators based on the idea of matching treated and controls according to some criteria of proximity: the propensity-score matching, the nearest matching method and, finally, the inverse probability weighting. As an alternative strategy, we also exploit the SCM, which relies on more general identifying assumptions than the standard difference-in-differences model. The basic idea is to compare the dynamics of migration in the treated provinces between the 19th and 20th centuries with the dynamics of a weighted combination of other Italian provinces, namely the synthetic controls, chosen to resemble the characteristics of the treated provinces before the implementation of sugar protectionism. In other words, the SCM is used to estimate the outcome trend of the counterfactual of treated provinces in the absence of the regime change.[12] The synthetic control approach minimises the chances of selection into treatment based on unobservables by allowing the effects of unobserved variables on the outcome to vary with

[12]The advantages of the SCM with respect to traditional regression analysis (transparency, flexibility and weakness of the identification assumptions) have led to a rapid application of this methodology in many different fields: the economics of terrorism (Abadie and Gardeazabal, 2003), political science (Abadie *et al.*, 2014), the effects of liberalizations (Billmeier and Nannicini, 2013), the effects of natural disasters on growth (Barone and Mocetti, 2014), and the economics of fertility (Machado and Sanz de Galdeano, 2015), among others. It is also worth noting that, although the synthetic control approach represents a natural extension of the standard DID estimator, it shares some features with the matching estimators as both approaches attempt to minimise observable differences between treatment and control units.

time. In this respect, it relies on more general identifying assumptions than the standard DID model. Although the SCM allows us to deal with the endogeneity problem due to time-varying omitted bias, it might still suffer from reverse causation if the timing of the sugar protectionism were linked to expectations on future migration patterns. However, we can confidently exclude this possibility since the regime change was directly aimed at boosting the local sugar industry, rather than reducing emigration.

5 The data

Annual data on emigration flows during 1876-1913 for the 69 Italian provinces (NUTS 3 units) are obtained from *Commissariato Generale dell'Emigrazione* (1926, pp. 45-65). Data from the same publication have been used by Faini and Venturini (1994a), Hatton and Williamson (1998), Del Boca and Venturini (2005), Gray et al. (2019), in their studies on the determinants of Italian migration in the late 19th- early 20th-century.

Figure 5: Migration rate, 1876-1913

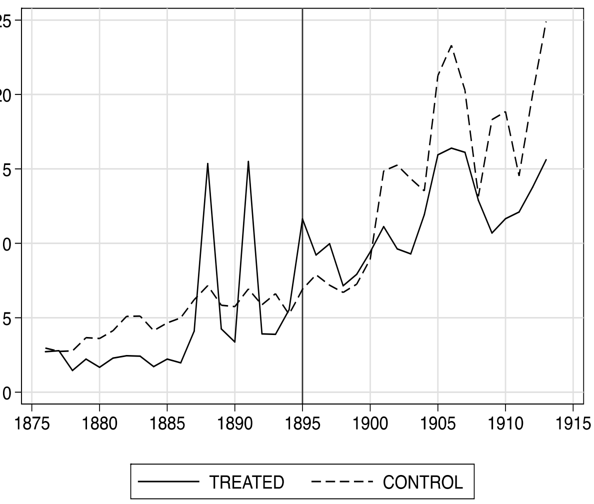
Note: Migration rates are per thousand population
Source: Authors' elaborations on the data reported in Commissariato Generale dell'Emigrazione (1926), pp. 45-65.

Figure 5 illustrates the temporal evolution of migration rates.[13] As a result of migration outflows

[13]Ideally one would like to separate out gross from net migrations. Unfortunately, reliable data on return migration for the whole sample period are not available (Hatton and Williamson, 1998).

increasing from about 100 thousands individuals in 1876 to more than 800 thousands at the end of our sample period in 1913, the migration rate exhibits a five-fold increase over 1876-1913. It passes from slightly less than 5 to almost 25 per thousand individuals in four decades. As evident from Figure 5, there is a rapid upsurge of the emigration rate soon after the turn of the century.[14]

Figure 6: Migration rates by treatment groups, 1876-1913



Note: Migration rates are per thousand population
Source: Authors' elaborations on the data reported in Commissariato Generale dell'Emigrazione (1926), pp. 45-65.

Figure 6 splits the aggregate 1876-1913 migration series into provinces affected by sugar protectionism (the treated group) and provinces not affected (the control group), in the spirit of the DID approach. With the exception of two peaks in the treated provinces, the figure shows that the emigration rate is higher in the control provinces than in the treated ones.[15] While the gap

[14]Italian emigration data of the late 19th century are based on the number of persons granted *nulla-osta* by the local mayor, a first step in the application procedure for an Italian passport. The method of collecting emigration statistics changed after the turn of the century and was based on passport records kept by police authorities. Further details are in Fenoaltea (2011, pp. 77-78) and the literature quoted therein. Hatton and Williamson (1998, p. 98) argue that various statistics on Italian emigration outflows after the turn of the century are likely to be exaggerated but "we simply do not know yet how much". Additional details on the regulation of Italian migration can be found in Appendix A5.

[15]The local peaks in emigration rates of treated provinces in 1887-1888 and 1891 deserve some clarification. As documented in Sori (1979, p.60) these peaks are mostly due to the migration from the region of Veneto, where some (four out of twelve) of our treated provinces are located, to Brazil. Together with the official abolition of slavery in 1888, the Brazilian government provided financial incentives covering travel expenses to promote immigration, so to increase the labor force. The data on the distribution of immigrants to Brazil by nationality of origin for the years

between the two series is initially roughly constant in the treated provinces (except for the two spikes), it starts to widen after the turn of the century.

In the empirical application we use literacy rate, heights, birth rates (all collected from historical sources or available in the literature) at the provincial level as control variables. Data on literacy rates are from the population censuses of 1871, 1881, 1901, and 1911, while those for inter-censual years were obtained by linear interpolation of provincial figures. Data on height are often used in economic history studies since they correlate with nutrition, physical activity, health and diseases. Here we use the annual historical provincial data on the height of Italian soldiers collected by A'Hearn et al. (2009) from military records. Due to the lack of a more direct measure of per capita income, this variable is meant to capture differences in living standards across provinces. We collected annual data on birth rates from 1862 to 1899 from official historical demographic sources (see Appendix A). Under the assumption that individuals are at risk of migration only once they turn 14, the lagged birth rate accounts for the impact of regional demographic pressure on regional migration (as in Faini and Venturini, 1994a; Hatton and Williamson, 1998) so to capture what in the literature is called "push factor" (Ardeni and Gentili, 2014). To account for infrastructural developments we use the annual provincial data on railway endowment provided in Ciccarelli and Groote (2018). Finally, to capture the changing composition of regional economic activity due to the industrialization process, we collect provincial data on the share of (male) industrial labor force from the population censuses. As in the case of literacy rates, the data for inter-censual years were obtained by linear interpolation of provincial figures.[16]

Summary statistics for the migration variables and the covariates in treated and control provinces are reported in Appendix Table C2-Panel A. The t-statistics of the difference in means shows that for a high number of socioeconomic dimensions the treated and controls differ considerably, which imposes some cautions upon the use of the DID method. To further increase comparability of treated provinces and control provinces we use the propensity score to trim the sample. In par-

1820-1907 reported in Ferenczi (1929) confirm that the peaks in total immigration to Brazil in 1887-1888 and 1891 were largely due to immigration from Italy. As already remarked at the end of Section 3, our theoretical model is consistent with this observation. Furthermore, the provinces of Belluno and Udine (in the upper North-East of Italy) were characterized by extremely sizeable seasonal migrations (Sori, 1979) and have been excluded from the analysis.

[16]The figures on industrial female labor force reported in the historical population census are considered party unreliable by Italy's economic historians and, therefore, not used here (Fenoaltea, 2011, pp. 231-232).

ticular, we drop control provinces which, based on their pre-reform characteristics, appear to be substantially different from treated provinces (see Angrist and Pischke, 2010 for a similar exercise, and Kline and Moretti, 2014b for an application). In practice, we estimate a logit model of the probability of being included in the treated area based on the pre-reform variables. We then drop from the analysis all control provinces with a predicted probability of treatment in the bottom 25%. This criterion leads us to drop 513 control province-year observations (25% of the total, by construction). Summary statistics for the covariates in treated and control provinces in the pre-reform period on the whole sample (columns 1 and 2) and on the trimmed sample (columns 4 and 5) are reported in Table C2-Panel B. Indeed, with the only exception of the share industry variable (representing the share of industrial labor force) in the remaining cases (height, birth rate, literacy rate, and railways) the similarity between the treated and the controls increases.

6 Results

6.1 Standard DID analysis

Table 2 reports the estimates of equation (9) using the emigration rate (migrants over population) as dependent variable. All columns include a set of province fixed effects to account for time invariant unobserved heterogeneity at provincial level. Columns 6-7 also include province-specific linear trends. The table exploits an expanding set of covariates.

Column 1 displays estimation results when excluding controls (X_{it} in equation 9). In line with the prediction of the theoretical model, we find a negative and significant effect of the sugar reform in the treated provinces, relative to control provinces. The impact amounts to an average of about -0.4 percentage points (from a baseline of 10.02) - a reduction in mean emigration of 4 per thousand.

A rich literature on historical Italian migration has provided evidence that migration choices depend on various socioeconomic indicators. For this reason, in columns 2 and 3 we control for birth rates at provincial level to account for demographic pressure on migration rates (see Hatton and Williamson 1998). As in Spitzer and Zimran (2018), we also include standard indicators of economic well-being such as height and literacy rate. As long as these variables have a positive

Table 2: DID Estimates of the Effect of Sugar Duty on Emigration Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated Province	3.145*	21.31***	19.66***	18.69***	20.39***	7.236**	7.799*
	(1.763)	(1.945)	(1.904)	(1.878)	(2.096)	(2.900)	(4.049)
Post_1895	11.260***	2.263	1.816***	2.289***	1.905***	-14.03***	15.65***
	(0.332)	(0.516)	(0.524)	(0.518)	(0.623)	(1.788)	(2.137)
Treated Province*Post_1895	-4.004***	-5.920***	-6.948***	-6.031***	-6.144***	1.433	1.808
	(0.753)	(0.699)	(0.721)	(0.715)	(0.77)	(1.01)	(1.11)
Birth Rate		0.721***	0.627***	0.727***	0.596***	0.172***	0.219***
		(0.064)	(0.0644)	(0.063)	(0.075)	(0.066)	(0.081)
Height		-0.294	-0.0545	0.130	-0.647**	-0.648***	-1.037***
		(0.232)	(0.231)	(0.226)	(0.302)	(0.198)	(0.266)
Literacy Rate		82.84***	89.15***	88.70***	85.20***	-0.465	-6.656
		(4.050)	(4.302)	(4.148)	(5.068)	(16.39)	(19.51)
Railways			-0.006*	-0.009***	0.004	0.004	0.012*
			(0.003)	(0.003)	(0.005)	(0.004)	(0.007)
Share Industry			-75.51***	-73.51***	-65.05***	-61.25***	-23.98
			(9.651)	(9.291)	(11.05)	(15.41)	(20.62)
Constant	0.536	-21.87	-46.05	-78.95**	49.12	110.7***	167.6***
	(1.229)	(37.60)	(37.30)	(36.39)	(48.79)	(33.61)	(44.96)
Province Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Province Linear Trends						✓	✓
Observations	2,546	2,546	2,546	2,345	2,033	2,546	2,033
R-squared	0.522	0.600	0.610	0.647	0.596	0.798	0.784

Note: Emigration rates are per thousand population
Standard errors in parentheses: $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$.

impact on migration, the DID estimate in column 1 is upward biased (in absolute term). Net of the influence of these variables, the effect of the reform gets larger in size and remains significant. This implies that - in absolute terms - the impact of the tariff on the migration rate increases considerably (by about 50%) relative to column 1.

In column 3 we also control for two additional variables computed at provincial level, namely the share of the industrial labor force and railway endowment which are meant to capture the differences in the stage of industrial development. The inclusion of these variables increases further the effect of interest.

In column 4 we exclude the years 1887, 1888 and 1891 from the analysis. As mentioned in Section 5, these years exhibited sizable peaks in migration due to the provision of financial incentives to move to Brazil (Sori 1979, p.60). This led to massive migrations from Veneto and, thus, it affected treated provinces. Indeed, the effect shown in column 4 is slightly reduced relatively to the one in column 3.[17] We take the estimates reported in column 3 as a benchmark for future comparisons.

In column 5, we use the trimmed sample to test the robustness of our results to a change in sample selection. As discussed in section 5, this sample drops the 25 percent of controls that are less similar to the treated in terms of pre-reform characteristics. The impact of the reform is slightly reduced when compared to column (3) of Table 2, based on the full sample.[18]

An additional way to test for the robustness of our results is to include province-specific linear time trends. As suggested, for example, by Autor(2003), and Angrist and Pischke (2008), the inclusion of these trends should rule out the possibility that treated and control provinces were already on different growth trajectories in their outcome variables before treatment occurred. Column (6) of Table 2 reports the results for the entire sample. In contrast with what we get from columns (1)-(5), the DID coefficient is now positive and not significant. A similar result also emerges in column (7), when we consider the trimmed sample. Since one would have expected that the coefficient of interest remained significantly negative even after the inclusion of province-specific trends, this findings seem to suggest that the treated provinces exhibited a declining trend in migration before the implementation of sugar tariffs. In other words, such province-trends reflect omitted variables and, thus, their inclusion would remedy an omitted variable bias. This conclusion is, however, unwarranted in our context, as we argue below. As remarked by Wolfers (2006), omitted variables should only bias the coefficients of interest if there is a systematic relationship between the trend in migration rates and the adoption of protectionism. This seems unlikely, given the exogeneity of the timing of the adoption of tariffs. Also, when controlling for province time-trends, the coeffi-

[17]Looking briefly at the effect of the other covariates we see that, among the measures of well-being, literacy rate has a significant impact on emigration rate, while height has not significant effect. This finding supports the traditional view that literacy played a key role in boosting migration. Demographic pressure, as measured by local birth rates, had a strong and positive effect on the emigration rate. This is not surprising, since nineteenth century Italy experienced a rapid expansion of its population. The coefficients of our proxies for industrial development are both negative and significant. We finally note that the result on literacy rate is at odds with Hatton and Williamson (1998), who find a negligible impact of literacy rate on emigration. Such discrepancies in the results might be due to differences in the sample period and in the estimation procedure. However, the empirical evidence on the relation between literacy rate and migration is mixed. For instance, Giffoni and Gomellini (2015) find that primary school attendance rate at the beginning of the twentieth century is correlated with emigration and return migration.

[18]To further increase the comparability between the treated and control provinces, Appendix C1.1 reports DID results using alternative definitions of neighboring provinces.

cient on *post*sugar* rises. This finding can be reconciled with an omitted variables interpretation only if the factors correlated with a relative fall in migration propensities led the government to adopt protectionist laws. Again, this seems unlikely. If anything, one might expect that the factors associated with a rising migration rate increased the pressure for protectionism.

Another central reason why the empirical results from the inclusion of province-trends may be troublesome is related to the presence of relevant dynamic effects generated by the policy shock. As Wolfers (2006) argues, the standard difference-in-difference approach may produce misleading results if panel-specific trends are included as controls, since the estimates might confound pre-existing trends with the response of the migration rate to tariffs. As argued in Meer and West (2016), since the standard model postulates that tariffs reduce migration in a discrete manner, the inclusion of province-specific trends tends to attenuate estimates of how local protection affects migration (On this point, see also Baum-Snow and Lutz 2011, p. 3026). In our case, we can reasonably expect that protectionism did not immediately generate a negative impact on migration because of "time-to-build" lags. In other words, the growing diffusion of a new agricultural product (sugar beet), the construction of new industrial plants and, more generally, the organization of the new sugar industry took time. Indeed, the specifications in columns 6-7 of Table 2 include only the single *post*sugar* dummy to capture the full adjustment process. As long as the dynamics is not well captured by this variable, province-specific trends may pick up not only different pre-existing trends across provinces, but also differences in the evolution of the migration rate between treated and control areas following the introduction of protectionism. This problem arises in any context where panel-specific trends are included while the response to the policy shock yields interesting dynamics which is left un-modeled. For this reason, in the next section, we will present two dynamic specifications. The first one will only consider the outcome dynamics over the post-treatment period, as in Wolfers (2006) and Halla (2013). The second one, instead, will model the dynamics both on the pre-reform and post-reform period (as, for example, in Autor, 2003; Meer and West, 2016). As a clear advantage, the latter strategy provides an alternative way to test for the common trend assumption.

6.2 Dynamic DID analysis

The standard discrete specification does not account for the dynamics of tariffs and migration rate, that is, how fast migration reacts after the implementation of protectionism, and whether the effect grows or fades away over time. This issue is particularly relevant in our context, where the sugar industry required time-to-build lags and, on the other hand, the local advantages brought in by the new industry could be partially compensated by immigration from other Italian provinces.

To analyze the effects of the policy shock over time, we adopt a specification that imposes very little structure on the response dynamics. Initially, we include six lags or post-treatment effects, that is, indicator variables for the years 1-3, years 4-6, years 7-9, years 10-12, years 13-15 and years 16-18 since 1895, when the tariff was implemented. As argued by Wolfers (2006, p. 1808), these variables should identify the entire response function allowing the estimated province-specific time trends to identify preexisting trends. Further, similarly to Autor (2003), we augment the dynamic specification by including six leads, or pre-treatment effects, for the years 1-3, years 4-6, years 7-9, years 10-12, years 13-15 and years 16-19 before 1895. The pattern of such leads is of substantive interest here, since it is a check on whether -conditional on province and year effects- the treatment causes the outcome and not vice versa. If causality runs from treatment to outcome, then the leads (which are indicators for future policy changes) should not matter.

Table 3 presents the estimated dynamic effects of the sugar reform. Columns 1-2 illustrate the case of the dynamic effect over the post-reform period. Columns 3-4 (full sample) refer, instead, to the full dynamic specification, where the dynamics is considered both before and after the 1895 reform. In columns 1 and 3 we include province fixed effect, while in columns 2 and 4 we include province-specific linear time trends. The post-reform lags are significant, negative and increasing in size over time. The size of the effects gets even larger when province-specific linear time trends are included. Such findings suggest that the implementation of sugar production took some time to display its impact on the welfare of residents. At the same time, the local advantages brought by the new industry were not offset by immigration. By contrast, the pre-reform coefficients are almost never significant, with the exception of the coefficient on the years 1-3 *preceeding* the 1895 policy change. This finding might suggest an anticipation of the reform. For example, some residents

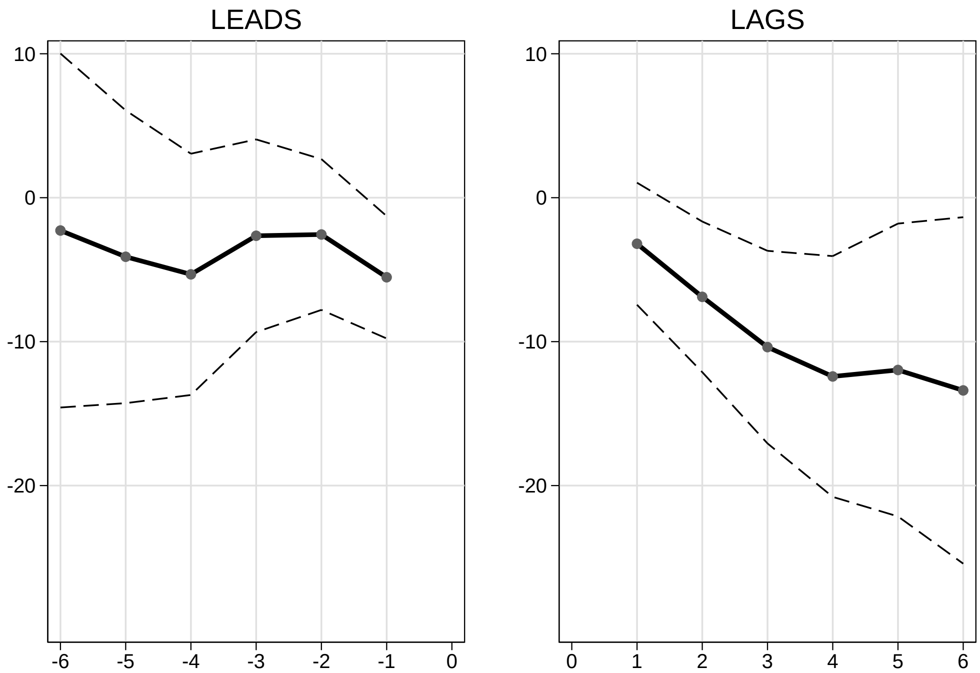
Table 3: Dynamic Effect of Sugar Duty on Emigration Rate

	Post-dynamics		Full-dynamics		Full-dynamics (Trimmed sample)	
	(1)	(2)	(3)	(4)	(5)	(6)
*Treated*years before 1895:*						
Years 16-19			-4.479**	-0.928	-4.569**	-2.279
			(2.135)	(5.658)	(2.296)	(6.148)
Years 13-15			-6.279***	-3.091	-6.346***	-4.102
			(2.188)	(4.672)	(2.357)	(5.085)
Years 10-12			-6.829***	-4.403	-7.077***	-5.325
			(2.182)	(3.844)	(2.361)	(4.191)
Years 7-9			-3.216	-1.897	-3.725	-2.643
			(2.176)	(3.068)	(2.352)	(3.347)
Years 4-6			-3.132	-2.186	-3.745	-2.557
			(2.174)	(2.399)	(2.346)	(2.619)
Years 1-3			-5.724***	-5.605***	-5.960**	-5.528***
			(2.170)	(1.940)	(2.346)	(2.127)
*Treated*years after 1895:*						
Years 1-3	0.216	-0.014	-3.514	-3.961**	-3.278	-3.198
	(1.206)	(1.235)	(2.170)	(1.939)	(2.340)	(2.120)
Years 4-6	-3.540***	-3.249**	-6.950***	-7.546***	-7.111***	-6.888***
	(1.213)	(1.354)	(2.172)	(2.396)	(2.346)	(2.616)
Years 7-9	-6.765***	-7.125***	-9.960***	-11.63***	-9.888***	-10.38***
	(1.215)	(1.488)	(2.173)	(3.067)	(2.349)	(3.345)
Years 10-12	-7.983***	-9.219***	-11.01***	-13.88***	-10.82***	-12.42***
	(1.218)	(1.642)	(2.175)	(3.844)	(2.344)	(4.182)
Years 13-15	-6.993***	-8.679***	-9.814***	-13.52***	-9.795***	-11.97**
	(1.221)	(1.808)	(2.177)	(4.675)	(2.344)	(5.086)
Years 16-18	-7.791***	-10.27***	-10.40***	-15.27***	-10.31***	-13.39**
	(1.226)	(1.984)	(2.181)	(5.536)	(2.347)	(6.018)
Constant	48.62	88.59**	70.18**	82.87**	172.8***	131.5***
	(35.20)	(34.38)	(35.45)	(34.17)	(46.51)	(45.49)
Controls	✓	✓	✓	✓	✓	✓
Province Fixed Effects	✓	✓	✓	✓	✓	✓
Province Linear Trends		✓		✓		✓
Observations	2,546	2,546	2,546	2,546	2,033	2,033
R-squared	0.675	0.775	0.690	0.783	0.675	0.769

Note: Emigration rates are per thousand population. Standard errors in parentheses: $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$. Controls are the same as in Table 2.

might have given up the idea of migrating abroad expecting to get well-paid jobs in the new industry. However, estimates in column 6 from the trimmed sample show that the coefficient on years 1-3 *after* the reform is not significant. It is thus unlikely that the reform had an anticipated effect on migration, but it had basically no impact immediately after its implementation. Figure 7 illustrates the pattern of the estimated lead and lag coefficients from Table 3, column 6.

Figure 7: Dynamic effects of the sugar reform



Note: The graph illustrates the temporal pattern of the coefficients reported in Table 3, column (6). Upper and lower bounds represent ± 1.96 time the standard error of each point estimate.

6.3 Evidence from alternative estimation methods: matching and SCM

As anticipated in Section 4, to make sure that our results are not driven by differences in observables, we can use alternative estimation methods, namely the matching methods and the SCM. As for the matching methods, we use three different strategies: the propensity-score matching, the nearest matching and the inverse probability weighting. The first two methods rely on comparing the outcome of each treated unit with an average of the outcomes of similar subjects that do not receive the treatment. For the propensity-score matching method, similarity between subjects is based on estimated treatment probabilities, known as propensity scores. Observations are matched according to the estimated propensity score computed as the probability of a province being included in the treated area, given some observable characteristics. For the nearest matching method, similarity builds on a weighted function of the covariates for each observation. Finally, under "inverse probability of treatment weighting" observations that receive the treatment are given weight of $1/p$ and those that did not receive the treatment are given weight of $1/(1-p)$, where p is the probability of getting the treatment (Imbens, 2000). Thus, we reweight the sample so that the treated and the controls are not so dissimilar and compute the effect of interest with a simple weighted regression. We have used literacy rate, height, birth rate, share of the industrial labor force and railway endowment to compute the propensity score, using only years before the reform. The results are reported in Table 4. Overall, the sign and significance level of coefficients are in line with those presented in Table 2, although the point estimates are smaller in size.

Table 4: Estimates of the Effect of Sugar Duty on Emigration Rate in the Treated Provinces using Different Matching Estimators

	Propensity score matching method (1)	Nearest matching method (2)	Inverse prob. weighting (3)
Treated provinces	-3.024***	-2.023***	-4.447***
	(0.764)	(0.539)	(0.355)
Observations	2,546	2,546	2,546

Note: Standard errors in parentheses: $***p < 0.01, **p < 0.05, *p < 0.1$.

Next, we exploit the SCM – initially developed by Abadie and Gardezabal (2003) and Abadie *et al.* (2010) – which relies on more general identifying assumptions than those of the standard difference-in-differences model. The synthetic control algorithm estimates the missing counterfactuals as a weighted average of potential controls' outcomes. The weights assigned to each control unit are chosen to minimise the differences between the treated units and the synthetic control group in the pre-treatment period. Notice that, in order to apply the SCM, the data of the treated provinces need to be aggregated at the year level, as if they belonged to one single province (the "treated" area, henceforth).

Moreover, we need to identify the predictors of the emigration rate. These predictors include literacy rate, birth rate, height, the share of the industrial labor force and the railways, already used in the DID approach. These variables are averaged over the pre-treatment period (1876-1895). We also add variables, all calculated for the year 1871, which capture the pre-existing differences among provinces, such as total number of births, infant mortality, and, as in Ciccarelli and Fachin (2017), number of postal offices, number of schools, and railway kms. As suggested by Abadie et al. (2010), each annual observation of the pre-treatment outcome variable (migration rate) is used as a separate control variable to maximize the pre-treatment fit. We construct the synthetic control as the convex combination of provinces in the donor pool such that it is sufficiently similar to the "treated" area in terms of pre-reform values of emigration rate predictors.

Table 5 presents the pre-treatment (i.e., pre-1895) sample averages of all predictors for the "treated" area (column 1), for the synthetic control group (column 2), as well as for the population-weighted average of the 55 provinces in the donor pool (column 3).[19] If we compare column 1 to column 3, we see that, on average, the provinces that did not benefit from the sugar reform in 1895 appear to have some limitations as suitable controls for the "treated" area. In particular, before protectionism, the 55 control provinces exhibited, on average, a lower literacy rate than the "treated" area. Moreover, the total number of births, infant mortality, number of postal offices, number of schools, and railways, as measured in 1871, and the existing railways were higher in the 55 provinces of the donor pool. By contrast, the pre-treatment values of the synthetic control group (column 2) are in line with the pre-treatment values for the "treated" province (column 1). This suggests that the synthetic control group acts as a better counterfactual for the treated group.

Table 5 also reports the emigration rate at three points in time, 1 year, 5 years and 15 years after 1895 for the treated area and its synthetic control. For the first 5 years after the reform, the emigration rates in the treated and control provinces are very close (the difference being -1.18 in

[19]The list and relative weights of the provinces used to build the synthetic controls are presented in Appendix Table C3.

Table 5: Covariates and Outcome Means for Migration Rate.

	Treated Area (1)	Synthetic control (2)	Average of 55 control provinces (3)
Pre-treatment migration rate	4.21	4.22	4.89
Literacy Rate	0.37	0.37	0.32
Birth Rate	34.35	33.99	34.14
Height	164.86	164.52	163.11
Railways	109.57	104.14	170.62
Share Industry	0.23	0.22	0.23
No. of births in 1871	10083.77	10406.35	15451.46
Infant mortality in 1871	2672.39	2578.77	3425.63
No. of postal offices in 1871	21.23	21.56	43.65
No. of schools in 1871	433.38	421.06	612.26
Railways in 1871	59.03	61.50	100.82
Migration rate at T_0+1	9.22	10.40	
Migration rate at T_0+5	9.43	10.18	
Migration rate at T_0+15	11.66	18.34	
Average post-treatment effect for migration rate	-3.23		
RMSPE for migration rate	2.64		

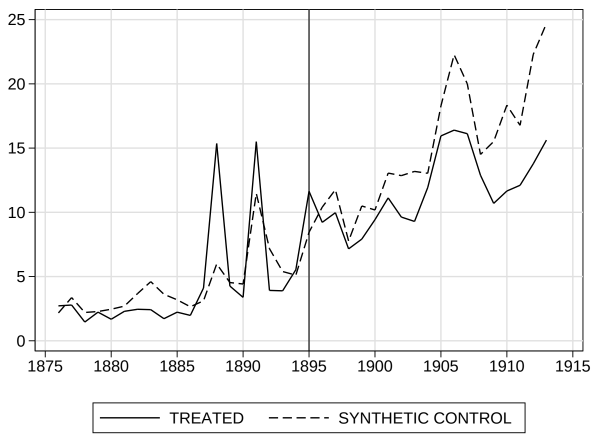
Note: Outcome: migration rate (per thousand population). Covariates are averaged over the pre-treatment period. As a benchmark, in the table the "pre-treatment migration rate" is averaged over the entire pre-treatment period, but the method minimizes the distance between each yearly outcome value for the treated province and its synthetic control. The values of the outcome refer to one period (T_0+1), five years (T_0+5) and fifteen years (T_0+15) after the treatment period (T_0=1895). RMSPE stands for Root Mean Squared Prediction Error. The list and relative weights of the provinces used to build the synthetic controls are presented in Appendix Table C3.

the first year and -0.75 after 5 years). The slow change in migration behaviour might be explained, again, by "time-to-build" lags, i.e. the time required for the diffusion of a new agricultural product (sugar beet), and the construction of new industrial plants processing the sugar beet itself. Fifteen years after 1895, the difference is -6.68. This is equivalent to say that, after 15 years, the emigration rate in the treated province was 36 percent lower than the estimated counterfactual. Over the entire post-treatment period the average difference between the treated province and the synthetic control is -3.23. This number is lower (in absolute term) than the DID estimate reported in column 3 of Table 2 but very close to the matching estimates from Table 4. The last row of Table 5 shows the

Root Mean Square Prediction Error (RMSPE), a measure of the difference in the emigration rate between the treated and the synthetic control groups during the pre-treatment period. The value of the RMSPE is very low (2.64), demonstrating the good fit of the model.

The main empirical result obtained from the SCM is illustrated in Figure 8. This figure represents the time-profile of the emigration rate for the "treated" area (solid line) and its synthetic counterpart (dashed line), in the entire pre-treatment period and for 18 years after the treatment ($T_0 = 1895$). The comparison between the solid and the dashed lines before 1895 provides a measure of the quality of the pre-treatment fit obtained through the SMC algorithm. After the reform, the same comparison illustrates the dynamic treatment effects.

Figure 8: Italian emigrations rates by group (synthetic control method), 1876-1913



The graphical evidence, together with the good balance of covariates shown in Table 5, suggests that the synthetic control is a suitable counterfactual for the migration pattern that would have occurred in the "treated" area between 1896 and 1913 in the absence of the sugar reform. Immediately after the reform, the two lines still remain quite close, with an emigration rate in the treated area slightly higher than the one in the synthetic control. However, after three years, the two patterns begin to diverge. While the emigration rate in the synthetic control takes a sharp upward trend, the treated area follows a positive, but more moderate trend. The discrepancy between the two lines suggests a negative and increasing impact of protectionism on emigration.

Finally, we recall that a possible drawback of the SCM is that inference is often not standard because the number of observations in the control pool is typically small. As suggested by Abadie et al. (2010, 2014), alternative methods of inference can be obtained using "placebo studies" (or falsification exercises), based on permutation techniques. The idea is that the SCM estimates of the impact of the intervention under scrutiny are less credible if similar or even larger (in magnitude) estimates of the effect of interest are obtained when the intervention did not take place. The application of the placebo test to our case is presented in Appendix C2.

7 Conclusion

A growing literature considers the effect of protectionist policies on various socioeconomic outcomes at the regional level. The underlying idea is that, depending on the case considered, an analysis carried out at the country level might miss an important part of the story. Within nations, regions often differ in terms of specialization patterns, local institutions, geographic and climatic factors as well.

The paper considers the link between protectionist policies and migrations in 19th century Italy. We assess the effect of the prohibitive import duties on sugar of the mid-1890s and the take-off of the national sugar industry on relative regional emigration rates. This historical episode represents an almost ideal framework to conduct such an exercise. On the one hand, production was, for the very nature of the industry considered, necessarily concentrated in a well-defined geographic area. On the other hand, the workers of the sugar industry benefitted from the development of a new industry without suffering much from the high price of sugar induced by the tariff, being sugar itself a luxury good. To formally investigate the topic, we develop a simple model that distinctly exploits the spatial dimension. In particular, the model shows how protection of local products raises local wages, thus reducing the incentive to migrate. This implication is tested empirically by exploiting the difference-in-differences method, matching techniques, and the synthetic control method. The results show that the adoption and consolidation of protectionism on sugar contributed to reduce emigration from sugar-producing provinces. Thus, protectionism increased welfare of the working class in the treated areas.

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Appendix

A Historical sources

The empirical analysis presented in this paper is based on a new set of annual historical data at the provincial level. We assembled the dataset partly resorting to primary historical sources, and partly borrowing from data presented in the literature. Our task was surely facilitated by the rising number of contributions concerning historical reconstructions of data disaggregated at the regional level.

A1 Data on migrations

Annual data on emigration flows during 1876-1913 for the 69 Italian provinces (NUTS 3 units) are from *Commissariato Generale dell'Emigrazione* (1926, pp. 45-65). We also consulted Ferenczi (1929).

A2 Data on tariffs and international trade

Data on imports and duties on sugar illustrated in Figure 1 are from the *Movimento commerciale del regno d'Italia*, the official source on Italian commercial flows. The data are reported separately for refined sugar ("zucchero di prima classe") and raw sugar ("zucchero di seconda classe"). Duties, as is customary, are specific (lire per unit of weight, usually quintal).

Data for the period 1862-1873 are from the retrospective figures reported (handwritten) in Movimento Commerciale del Regno 1862-1873, unnumbered pages, items "Importazione, commercio speciale, zucchero raffinato sia in pane sia in polvere" and "Importazione, commercio speciale, zucchero non raffinato".

Data for the period 1874-1906 are from the annual issue of the same historical source. So, for instance, data for the year 1889 are from page 18 (Tavola I, Importazione, Commercio speciale, Categoria II, Generi coloniali, droghe e tabacchi, items "zucchero di prima classe" and "zucchero di seconda classe") of Ministero delle finanze (1890) *Movimento commerciale del regno d'Italia nell'anno 1889*, Rome: Tipografia Eredi Botta. Similarly, data for the year 1897 are from from page 31 (imported quantity of "zucchero di prima classe"), page 32 (imported quantity of "zucchero di seconda classe"), and page 338 (import duties) of Ministero delle finanze (1898), *Movimento commerciale del regno d'Italia nell'anno 1897*, Rome: Tipografia Elzeviriana.

Finally, annual data for the period 1907-1913 are the retrospective figures reported at pp. 66-67 of Ministero delle finanze (1914), *Movimento commerciale del regno d'Italia nell'anno 1897*, Rome: Tipografia nazionale di G. Bertero e C.

The above data have been checked against those reported for the years 1877-1898 in Stringher (1899), p. 63 and against the more recent data base provided by Federico *et al* (2011), and available at `https://www.bancaditalia.it/statistiche/tematiche/stat-storiche/stat-storiche-economia/Serie_1862_1950_v2015.zip`, last accessed, 13 January 2018.

A3 Data on the domestic production of sugar

The data illustrated in Figure 2, Panel A are from the following secondary sources. The annual series referring to the cultivated sugar beet areas (thousand hectares) during 1889-1913 are based on two secondary sources. The first is Perdisa (1938), p. 16. The author bases his statistical reconstruction elaborating on the *Nuova enciclopedia di chimica*, by I. Guareschi and the *Annuario dell'industria saccarafera italiana*. See Perdisa (1938) for full references and additional details. The second source is Sabbatucci Severini (2004), pp. 268-269 reporting a longer series, covering the years 1887-1951 and based, in turn, on Parliamentary retrospective figures published in the mid-1960s. See Sabbatucci Severini (2004) for full references and further details.

The annual 1877-1913 production data illustrated in Figure 2, Panels B-C are from two historical sources, of fiscal nature. The data are available after the introduction of a tax (lire per quintal) on the production of sugar in 1877. In addition, both historical sources report the place of production and the name of the producer. The data for the period 1879-1895 are from the annual publication Ministero delle finanze *Statistica delle fabbriche di spirito, birra, acque gassose, zucchero, olio di semi di cotone, cicoria preparata e polveri piriche, e delle tasse relative*. The data for the year 1882 are, for instance, from Ministero delle finanze (1883), *Statistica delle fabbriche [...]*, p.30. The data for the period 1895-1913 are from the annual publication Ministero delle finanze, *Statistica delle tasse di fabbricazione*. The data for the period July 1899-June 1900 are, for instance, from Ministero delle finanze (1900), *Statistica delle tasse di fabbricazione*, pp. 42-43. The annual source Ministero delle finanze, *Relazione dell'amministrazione delle gabelle*, was also consulted as a complementary source of information.

A4 Additional socioeconomic data

In the empirical application we control for birth rate, height, literacy rate, share of industrial labor force, and railway endowment. The data are at the provincial level (roughly NUTS 3 units). The annual data on (lagged) birth rate are from the historical source Ministero di Agricoltura Industria e Commercio, *Popolazione. Movimento dello Stato Civile*, *ad annum*. They cover the period 1862-1899, while migration data cover the period 1876-1913. The underlying assumption is that individuals are at risk of migration once they turn 14. Ad hoc assumptions had to be made in certain cases, such as Latium before 1871, because the region was not yet part of Italy and data are thus not available. The annual 1876-1913 data on height of Italian soldiers were kindly provided by Brian A'Hearn (see A'Hearn et al. 2009). The data on literacy in Italian provinces for the years 1871, 1881, 1901 and 1911 are reported in the population censuses and linearly interpolated, separately by province, for the remaining years. Similarly, the data on the share of (male) industrial labor force (the share is of course computed over total labor force in agriculture, industry and the services) are those reported in the four population censuses and interpolated in the intercensual years. For the sake of completeness we notice that the population census was not taken in 1891, the population censuses of 1871, 1881, and 1901 were taken in winter and the population census of 1911 in summer, when seasonal migrations were particularly high in certain areas of the country. This partially reduces the intertemporal homogeneity and comparability of the figures reported. Indeed, as explained in the main text, also for this reason we decided to exclude the provinces of Belluno and Udine (in the upper North-East of Italy) from the analysis. Finally, annual data on railway endowment are from Ciccarelli and Groote (2018) (data available at http://bit.do/italianrailways, last accessed March 2020).

We also use total number of births in 1871, infant mortality in 1871, number of postal offices in 1871, and number of schools in 1871. The demographic data on birth and infant mortality are from the historical source Ministero di Agricoltura Industria e Commercio, *Popolazione. Movimento* *dello Stato Civile*, *ad annum*. The data on the number of postal offices and primary schools at the provincial level in 1871 are from Antonielli, E. (1872), *Annuario statistico delle province italiane per l'anno 1872*, Tipografia Tofani: Florence.

A5 Historical legislation on migration

In order to frame the data and the analysis in the institutional framework of the time, it might be useful to briefly outline a few aspects of the Italian historical legislation on migration and its evolution over time. As reported in Sori (2003) and Del Boca and Venturini (2005), the early Italian policy makers conceived the topic of outmigration mostly as a problem of public safety. Indeed, the various ministerial directives ("circolari") issued in the 1860s and the 1870s established that a prerequisite for outmigration was the individual commitment to personally provide for own return journey in the event of illness or indigence, or to demonstrate to have a job in the destination country or, in any case, to be in possession of means of subsistence. One of the aims of these directives was to limit forced repatriations that represented a burden for the public budget.[20] The actual implementation of these ministerial instructions was left by the national government to the discretionary evaluation of the local authorities such as prefects and city mayors, given their superior knowledge of their territory.

It was only at the turn of the century, when migration outflows reached unprecedented levels, that a Parliamentary law on migration was approved (law of January 31, 1901) and it was instituted the Italian Foreign Ministry's *Commissariato dell'Emigrazione*, to oversee and facilitate the regular development of the whole emigration process. In principle, provincial differences in the implementation of the early circulars and subsequent laws might challenge our identification strategy. As a matter of fact, however, the lack of any data on the regional enforcement of such measures prevents us to address the issue empirically.[21]

B Derivation of theoretical results

In this Appendix, we derive the theoretical results outlined in Section 3. As mentioned in the main text, individuals decide among three alternatives. They choose whether to stay in the area where they were born, to move to another province in the same country, or to migrate abroad. Domestic firms are perfectly competitive in the output market, face competitive labour markets and may, or may not, operate under the protection of tariffs.[22]

[20] According to Sori (2003), p. 262 once repatriations started to represent a political issue, Luigi Bodio, one of the founding father of the statistic discipline in Italy, decided to start collecting official data on migration outflows. This would explain the initial year (1876) of official statistics on migration in Italy and the initial year of our sample period as well.

[21] It is also possible, as suggested by one of the reviewers, that interest groups related to the sugar industry might have played a role in reducing migration flows. However, based on the reading of the historical sources (see for instance Maccaferri 1883, Peglion 1917), the Parliamentary debates of the time, and the recent literature on the development of the Italian sugary industry (see for instance Sabbatucci Severini 2004), we tend to rule out this possibility.

[22] Ciccarelli et al. (2018) sketch two additional cases which consider (i) the role of monopsony in local labour markets and, (ii) the presence of liquidity constraints. With regard to the former, migration decisions can be also affected by the mechanism of local wage determination in agriculture. While it is plausible to assume competitive wage determination in areas where land ownership is fragmented and there are several competing farms, the hypothesis of monopsony in the local labour market seems more appropriate to describe wage determination in areas characterised by large estates (see, e.g., Manning 2003). On the other hand, individuals who are willing to leave their country

B1 Workers

Space is partitioned in three areas, indexed by $C = \{s, ns, f\}$. Areas $\{s, ns\}$ are provinces belonging to the same country, while the area denoted by f is abroad.

The utility of a worker who lives in a domestic area $c = \{s, ns\}$ is given by expression (1), subject to the budget the budget constraint (2) in the main text. Utility maximization yields expression (5). On the other hand, the level of utility which can be attained by migrating abroad (area f) is given by expression (6).

In the absence of liquidity-constraints that can prevent from paying the pecuniary cost of migration t, and assuming that movements between domestic provinces have a negligible cost, the probability that an individual born in s will locate in area $C = \{s, ns, f\}$ is given by (see Anderson et al., 1992, and Train, 2009):

$$\varphi_C = \frac{\exp\left\{\frac{\upsilon_C}{\phi}\right\}}{\exp\left\{\frac{\upsilon_s}{\phi}\right\} + \exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} + \exp\left\{\frac{\upsilon_f}{\phi}\right\}}, \qquad \text{with } C = \{s, ns, f\}. \tag{B1}$$

The parameter $\phi \geq 0$ is a measure of the dispersion of preferences about locations in the local population. Such a parameter can also be interpreted as an inverse measure of mobility[23], since the variance of the shock χ is equal to $\phi^2\left(\frac{\pi^2}{6}\right)$.

Thus, the labour supply for location s will be equal to:

$$n_s^s = \frac{\exp\left\{\frac{\upsilon_s}{\phi}\right\} \cdot (N_s + N_{ns})}{\exp\left\{\frac{\upsilon_s}{\phi}\right\} + \exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} + \exp\left\{\frac{\upsilon_f}{\phi}\right\}}. \tag{B2}$$

By recalling that $\upsilon_s \equiv \ln\theta + \ln\left(\frac{w_s}{P}\right)$, it can be immediately noticed that labour supply in area s is increasing in the local wage. On the other hand, the number of natives who migrate abroad from area s is given by:

$$m_s = \frac{\exp\left\{\frac{\upsilon_f}{\phi}\right\} \cdot N_s}{\exp\left\{\frac{\upsilon_s}{\phi}\right\} + \exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} + \exp\left\{\frac{\upsilon_f}{\phi}\right\}}. \tag{B3}$$

Expression (B3) implies that migration abroad will increase, when the foreign real wage W_f increases relative to domestic wages.[24]

By exploiting expressions (B2)-(B3), the rate of migration from area s, defined as the number of migrants relative to the local labour supply, $\rho_s \equiv \frac{m_s}{n_s^s}$, is given by expression (7) in the main text.

Expressions analogous to (B1)-(B2)-(B3) also hold for area ns.

may be liquidity constrained, as suggested by Faini and Venturini (1994a). In this case, local wages can have a non-monotonic impact on migration. An increase in local wages may initially favour migration by reducing liquidity constraints but, when liquidity does not bite anymore, additional wage increases will discourage migration.

[23]For instance, Armstrong and Lewis (2017, pp. 173-174) find that the degree of mobility was relatively low among Italians.

[24]An additional implication of the model is that a reduction in transport costs (here denoted by t) will raise the incentive to migrate abroad, as summarized by the value of ν_f in (6). This is what we observe in Figure 6 with the big spikes in migration outflows from Italy in 1887-88 and 1891, when Brazil granted free commuting to the Italian workforce as an attempt to compensate for the labor force shortage caused by the abolition of slavery.

B2 Producers

In what follows, we characterize the behavior of producers. We postulate that producers are price-takers for what it concerns output. Here, we consider the case of a competitive labour market, a plausible assumption when land ownership is fragmented across several landowners.[25]

B3 Producers in the protected sector

Consider first the behavior of producers located in the domestic province s. Under perfect competition in the labour market, a local farm maximizes profit by taking wage w_s as given. The first-order condition, $\frac{\partial \Pi_s}{\partial n_s} = p \cdot \frac{\partial q}{\partial n_s} - w_s = 0$, yields the following labour demand:

$$n_s^d = \Theta \cdot \left(\frac{p^* + \tau}{p'}\right)^{\frac{1-\mu}{\alpha}} \cdot \left(\frac{w_s}{P}\right)^{-\frac{1}{\alpha}} \tag{B4}$$

where $\Theta \equiv (1-\alpha)^{\frac{1}{\alpha}} \cdot \bar{L}$ is a constant and we exploited both (3) and (4).

By equating local labour demand (B4) to local labour supply (B2), we obtain an implicit expression for the equilibrium wage level in province s, $\frac{w_s}{P}$, given the wage paid in province ns, and the wage paid abroad:

$$\frac{\exp\left\{\frac{\upsilon_s}{\phi}\right\} \cdot (N_s + N_{ns})}{\exp\left\{\frac{\upsilon_s}{\phi}\right\} + \exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} + \exp\left\{\frac{\upsilon_f}{\phi}\right\}} = \Theta \cdot \left(\frac{p^* + \tau}{p'}\right)^{\frac{1-\mu}{\alpha}} \cdot \left(\frac{w_s}{P}\right)^{-\frac{1}{\alpha}}. \tag{B5}$$

Next, we analyze what happens in province ns, which produces goods that are not protected by tariffs.

B4 Producers in non-protected sectors

Consider now the behavior of producers located in province ns, the domestic province specialized in the production of goods that are not protected by tariffs. In a competitive labour market, labour demand is given by:

$$n_{ns}^d = \Theta \cdot \left(\frac{p^* + \tau}{p'}\right)^{-\frac{\mu}{\alpha}} \cdot \left(\frac{w_{ns}}{P}\right)^{-\frac{1}{\alpha}} \tag{B6}$$

where we used (3) and (4). Since local labour supply is given by the analog of (B2), equilibrium in the local labour market will imply the following condition:

$$\frac{\exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} \cdot (N_s + N_{ns})}{\exp\left\{\frac{\upsilon_s}{\phi}\right\} + \exp\left\{\frac{\upsilon_{ns}}{\phi}\right\} + \exp\left\{\frac{\upsilon_f}{\phi}\right\}} = \Theta \cdot \left(\frac{p^* + \tau}{p'}\right)^{-\frac{\mu}{\alpha}} \cdot \left(\frac{w_{ns}}{P}\right)^{-\frac{1}{\alpha}}. \tag{B7}$$

Expressions (B5) and (B7) constitute a system of two equations in domestic wages, $\frac{w_s}{P}$ and $\frac{w_{ns}}{P}$, given the (exogenous) wage paid abroad.

[25] With regard to the labour market, since the main focus is about agricultural commodities, it would be plausible to consider both the competitive case and the monopsony case, depending on the local concentration of land ownership. The details about the monopsony case, which better fits the case for locally-concentrated land ownership, are briefly sketched in Ciccarelli et al. (2018). Large estates were more concentrated in Southern Italy, compared to Northern Italy which hosted sugar production.

B5 Equilibrium

To analyze the impact of domestic wages on migration rates - which are the variables we observe - we first divide equations (B5) and (B7) side by side. Then, recalling that $(\upsilon_s - \upsilon_{ns})/\phi = \left[\ln\left(\frac{w_s}{P}\right) - \ln\left(\frac{w_{ns}}{P}\right)\right]/\phi$ and taking logs, we obtain the following:

$$\ln\left(\frac{w_s}{P}\right) - \ln\left(\frac{w_{ns}}{P}\right) = \frac{\alpha\phi}{\alpha+\phi}\left[\ln(p^* + \tau) - \ln p'\right] \tag{B8}$$

since $p = p^* + \tau$.

Expression (B8) shows that the elasticity of relative provincial wages to the relative price of the goods produced in such provinces is equal to $\frac{\alpha\phi}{\alpha+\phi} \geq 0$. As a consequence, if a tariff artificially increases the price of goods produced in province s, the real wage in that area will increase, relative to province ns. Further, the positive impact of the tariff on relative wages crucially depends on the presence of imperfect mobility among individuals, that is, on the condition $\phi > 0$.[26]

By exploiting (B2) and the analogous expression holding for province ns, the relative level of equilibrium employment in the two domestic provinces is given by:

$$\ln n_s - \ln n_{ns} = \frac{1}{\phi}\left[\ln\left(\frac{w_s}{P}\right) - \ln\left(\frac{w_{ns}}{P}\right)\right] = \frac{\alpha}{\alpha+\phi}\left[\ln(p^* + \tau) - \ln p'\right] \tag{B9}$$

Expression (B9) shows that the elasticity of relative employment to relative wages is larger, the smaller ϕ, our measure of labour mobility imperfection. In other words, when the variability of idiosyncratic preferences for location is rather small, population movements will become more sensitive to the local wage level.[27] By the same token, relative employment will react more to changes in relative local prices (and tariffs, thus), when labour mobility is higher, that is, when ϕ is little.

We summarize these conclusions in the following Remark:

Remark. The introduction of a tariff which protects production of area s will increase wages in this area, relative to wages paid in areas specialized in non-protected goods. Further, the tariff will increase local employment, relative to provinces which do not benefit from trade protection.

Turning now to the impact of tariffs on the decision to migrate abroad, we can use (7) and the analogous expression for area ns, to analyze the relative behaviour of migration rates across domestic provinces, the main object of the empirical analysis.

The relative migration rate between area s and area ns is given by

$$\ln \rho_s - \ln \rho_{ns} = \ln\left(\frac{N_s}{N_{ns}}\right) - \frac{1}{\phi}\left[\ln\left(\frac{w_s}{P}\right) - \ln\left(\frac{w_{ns}}{P}\right)\right], \tag{B10}$$

[26]When $\phi \to 0$, the variance of idiosyncratic location preferences, $\phi^2\left(\frac{\pi^2}{6}\right)$, shrinks to zero. In this case, the model degenerates into the case where individuals are homogeneous and perfectly mobile, as in Roback (1982). Moreover, under perfect labour mobility, expression (B8) implies that local wages will be unaffected by local protection. On the relation between local wages and mobility, see also Accetturo et al. (2019).

[27]Notice that, when ϕ shrinks to zero, a tiny change in the local wage will generate a large change in local labour supply. As emphasized by Topalova (2010), this is exactly what happens in the standard trade model where factors are fully mobile.

which yields expression (8) in the main text. This equation predicts that the relative migration rate between the two domestic provinces is inversely related to their relative wages. As a consequence, the implementation of a tariff which protects area s will reduce the incentive of its residents to move abroad, relative to provinces which do not benefit from protectionism.

C Further empirical evidence

This appendix reports some further empirical evidence. It includes two subsections. The first concerns the DID, the second the SCM.

C1 Additional DID evidence

C1.1 Alternative control groups

As mentioned in Section 4, the common trend assumption might be particularly problematic in our context. Indeed, the Italian regional divide suggests that the treated provinces are likely to be intrinsically different from a large number of provinces, such as those located in the South, that we include as controls. To circumvent the problem of heterogeneity, we first consider neighboring provinces, since shorter physical distance should favor comparability.

In Appendix Table C1, column 1, we restrict the selection of controls to neighboring provinces, i.e. the provinces which are adjacent to the treated ones. This is approximately equivalent to selecting controls within a cut-off distance of 150km, as in Ciccarelli and Fachin (2017). The estimate we obtain points to the same direction as the DID estimates in Table 2, but it is much smaller in size and not significant. A plausible explanation of this -seemingly disappointing- result is that neighboring provinces might not be suitable controls. Indeed, it is plausible to believe that such provinces were affected, to some extent, by spillovers stemming from the treated areas. Specifically, the mobility of workers from neighbor to treated provinces tended to mitigate the effect of interest, as suggested in the theoretical discussion in Section 3. On the other hand, distance was still likely to matter. In particular, internal mobility was limited by factors like physical distance and transportation costs. For these reasons, we propose a selection of controls which favors comparability with the treated provinces while excluding, at the same time, areas that are possibly contaminated by the treatment itself. As shown in column 2 of Appendix Table C1, when we limit the control group to the Northern and Centre provinces (the provinces belonging to the northern regions of Piedmont, Liguria, Lombardy and Veneto, and to the central regions of Emilia Romagna, Tuscany, Marches, Latium, and Umbria) while excluding the provinces adjacent to the treated ones, the estimated DID coefficient is again back in line with the results presented in Table 2.

Appendix Table C1: DID Estimates of the Effect of Sugar Duty on Emigration Rate using Alternative Control Groups

	neighbours	selected Northern provinces
	(1)	(2)
Treated Province	-12.73***	4.319*
	(2.111)	(2.232)
Post_1895	1.183	2.205***
	(0.845)	(0.761)
Treated Province*Post_1895	-0.610	-2.165**
	(0.836)	(0.866)
Birth Rate	0.175*	0.120
	(0.094)	(0.093)
Height	-1.007***	0.317
	(0.340)	(0.376)
Literacy Rate	50.88***	48.74***
	(6.294)	(6.388)
Railways	0.012	-0.0033
	(0.0087)	(0.0045)
Share Industry	-8.329	19.06
	(13.85)	(15.21)
Constant	153.2***	-83.76
	(55.52)	(60.52)
Province FE	✓	✓
Observations	950	1,140
R-squared	0.557	0.510

Note: Standard errors in parentheses: $***p < 0.01, **p < 0.05, *p < 0.1$. Control provinces in column (1) include only neighbour provinces of the treated ones. Control provinces in column (2) exclude Southern provinces and Northern provinces that are neighbours of the treated provinces.

C1.2 Trimming

In this section we present further summary statistics by treatment status in the full period and the pre-treatment period. These complement those provided in the main text where we use the propensity score to trim the sample and directly test for the balancing of the covariates (before the treatment) in the full sample and the trimmed sample.

Appendix Table C2: Summary statistics, by treatment status

A. Whole period (1876-1913)

	Treated (1)	Control (2)	p-value (3)
Migration Rates (per thousand pop.)	7.931	10.523	0.000
	(0.359)	(0.248)	
Height (cm)	165.447	163.630	0.000
	(0.043)	(0.045)	
Birth Rate (per thousand pop.)	33.608	34.007	0.026
	(0.156)	(0.0792)	
Literacy Rate	0.444	0.375	0.000
	(0.005)	(0.004)	
Railways (km)	130.779	213.855	0.000
	(2.653)	(3.122)	
Share Industry	0.225	0.229	0.196
	(0.001)	(0.002)	
Observations	494	2,052	

B. Pre-reform period (1876-1894): covariates in the full and trimmed sample

	Full sample			Trimmed sample		
	Treated (1)	Control (2)	p-value (3)	Treated (4)	Control (5)	p-value (6)
Height	164.86	163.11	0.000	164.86	163.71	0.000
	(0.04)	(0.06)		(0.04)	(0.06)	
Birth Rate	34.35	34.14	0.336	34.35	34.10	0.278
	(0.22)	(0.10)		(0.22)	(0.11)	
Literacy Rate	0.37	0.32	0.000	0.37	0.35	0.049
	(0.00)	(0.01)		(0.00)	(0.01)	
Railways	109.57	170.62	0.000	109.57	147.75	0.000
	(3.56)	(3.58)		(3.56)	(3.64)	
Share Industry	0.23	0.23	0.742	0.23	0.23	0.733
	(0.00)	(0.00)		(0.00)	(0.00)	
Observations	247	1,026		247	758	

Notes: The table provides a comparison between treated and control provinces. Panel A includes the full sample. Panel B compares the full and (25%) trimmed sample. In both panels, column (3) reports the p-value associated to the t-statistic of the difference in means.

C2 Synthetic Control Methods

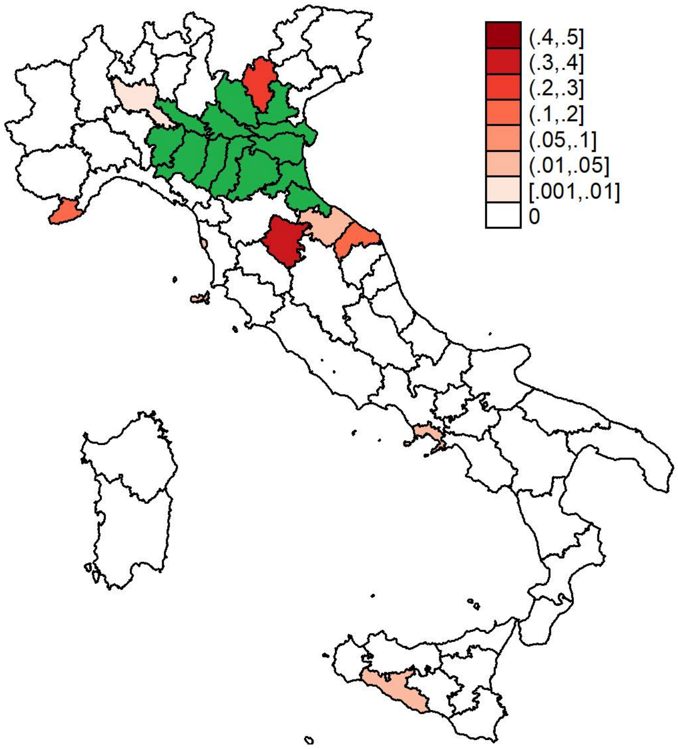
C2.1 Weights

Appendix Table C3 displays the weights of each province in the synthetic control group. The weights indicate that the emigration rate in the "treated" area prior to the sugar reform is best reproduced by a combination of Ancona, Arezzo, Girgenti, Leghorn, Milan, Naples, Pesaro, Porto Maurizio, and Vicenza. All other provinces in the donor pool are assigned zero weight. Interestingly, with the only exception of the provinces of Naples and Girgenti (but with a very low weight), the provinces forming the synthetic control group are located in the Center and the North of Italy (see the map reported in Appendix Figure C1).

Appendix Table C3: Weights (x 100) of provinces in the synthetic control group

Province	Weight	Province	Weight
Alessandria	0	Lucca	0
Ancona	16.6	Macerata	0
Aquila	0	Massa C.	0
Arezzo	32.3	Messina	0
Ascoli P.	0	Milan	0.6
Avellino	0	Naples	2.5
Bari	0	Novara	0
Benevento	0	Palermo	0
Bergamo	0	Pavia	0
Brescia	0	Perugia	0
Cagliari	0	Pesaro	2.5
Caltanissetta	0	Pisa	0
Campobasso	0	Porto M.	13.9
Caserta	0	Potenza	0
Catania	0	Reggio C.	0
Catanzaro	0	Rome	0
Chieti	0	Salerno	0
Como	0	Sassari	0
Cosenza	0	Siena	0
Cuneo	0	Syracuse	0
Florence	0	Sondrio	0
Foggia	0	Teramo	0
Genoa	0	Turin	0
Girgenti	2.4	Trapani	0
Grosseto	0	Treviso	0
Lecce	0	Venice	0
Leghorn	3.2	Vicenza	25.8
		TOTAL	*100.0*

Figure C1: Geographical distribution of the SCM weights



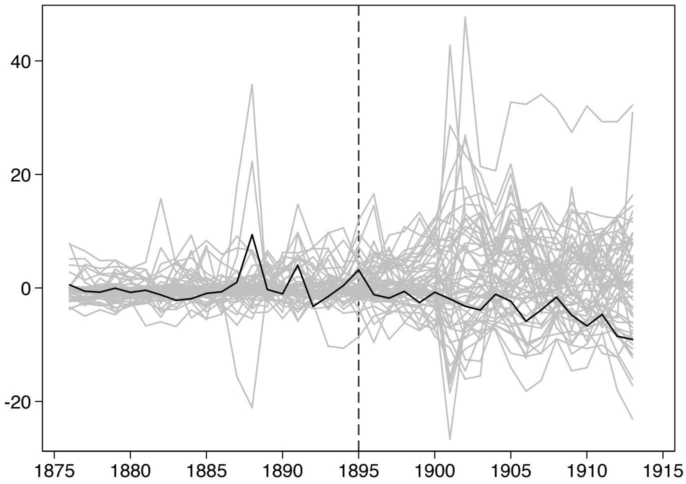
Source: Appendix Table C3. This Figure illustrates the geographical distribution of the provinces involved in the SCM.

C2.2 Robustness

Abadie *et al.* (2014) propose a falsification exercise, called "in-space placebo" test, which consists in reassigning the intervention to units that are not exposed to it. A particular implementation of this idea consists in applying sequentially the synthetic control algorithm to every province in the pool of potential controls and compare the placebo with the baseline results. Were we to find that the placebo studies create gaps of magnitude similar to the one found for the treated provinces, then the credibility of the empirical strategy would be undermined. We follow this approach and apply iteratively the SCM to every potential control province. In each iteration, we reassign the sugar reform to one of the 55 control provinces, moving the original "treated" province to the donor pool. We finally compute the estimated effect associated with each placebo run. The procedure creates a distribution of estimated gaps for the provinces not affected by the intervention.

Figure C2 shows the results of this test. The grey lines represent the gap associated with each of the 55 runs of the test, i.e. the difference in emigration rates between each province in the donor pool and its synthetic counterpart. The black line instead represents the true gap between the original "treated" area and its synthetic control. At the end of the sample period, the estimated gap for the treated province ranked 7^{th} out of 55 tests. This indicates that the probability of estimating a larger effect by chance is 7/55=12.7%. In a confidence interval setting, this would be

Figure C2: Placebo test



Note: The graph reports the difference, in terms of emigration rate, between the "treated" province and its synthetic control (black line), as well as the same differences for all the other Italian provinces (placebo in gray lines).

equivalent to say that the pseudo-p value is slightly above 10%.