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Can school centralisation foster human capital accumulation? A quasi-experiment from early XX century Italy

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Abstract

This paper shows that a shift towards a more centralized school system can benefit countries characterized by poor levels of human capital and large regional disparities in education. In 1911, Italy moved from a fully decentralized primary-school system towards centralisation through the Daneo-Credaro Reform. The Reform design allows us to compare treated municipalities with provincial and district capitals, which retained school autonomy. Our quasi-experiment, based on Propensity Score Matching (PSM), shows that centralisation substantially increased the pace of human capital accumulation. Treated municipalities were characterized by a 0.43 percentage-point premium on the average annual growth of literacy between 1911 and 1921. We discuss some of the channels through which the new legislation affected primary schooling and literacy, with important implications for long-term economic growth.

Keywords: Human capital, school management, public policy, decentralisation, centralisation, Italy.

JEL classification: N33, N34, I21.

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1. Introduction

Human capital formation is largely acknowledged as one of the main factors of economic growth, as argued by several well-established theoretical models.[1] Empirical evidence has confirmed that human skills and abilities are central to economic performance in the present day, as well as in the past.[2] Due to this, one of the crucial themes investigated by the literature is the way that school systems are organized, e.g. to what extent schooling is managed and funded by local vs central levels of government. Several pros and cons of school decentralisation[3], which is commonly defined as the devolution of school management to lower levels of the public administration, have been highlighted.[4] On the one hand, local policy makers are more aware of the needs of local communities, which is supposed to improve schooling. On the other hand, school preferences might vary considerably between the central government and the local ruling elites, hampering the diffusion of education. Similarly, in a long-term view, Lindert puts forward that centralized education may work better than local school autonomy in countries characterized by large social and economic regional disparities and slow internal convergence.[5]

Despite these contributions, little research in a historical perspective has investigated whether a shift from decentralized to centralized educational systems can be beneficial to improve mass education.[6] In this paper, we offer new evidence on this issue through a quasi-experiment that aims to explore dramatic changes in Italy's educational institutions at the beginning of the XX century, i.e. the 1911 Daneo-Credaro Reform. Due to this legislation, most municipalities moved from a decentralized school system, which had been based on the 1859 Casati Law, to direct state management and funding, while other municipalities, mainly provincial and district capitals, retained their autonomy, thus forming two distinct groups.[7] The Reform design allows us to compare these two groups through a quasi-experiment based on Propensity Score Matching (henceforth PSM). Cappelli has already provided some evidence

[1] Solow, 'A Contribution to the Theory of Economic Growth'; Lucas, 'On the Mechanics of Economic Development'; Romer, 'Endogenous Technological Change'.
[2] For example, Goldin, 'Human Capital'; Hanushek and Woessmann, 'Do Better Schools Lead to More Growth?'.
[3] For example, see Galiani and Perez-Truglia, 'School Management in Developing Countries'.
[4] See Bray, 'Centralisation Versus Decentralisation'.
[5] Lindert, *Growing Public*, pp. 104-105. Likewise, Hanushek, Link and Woessmann ('Does School Autonomy') convincingly argue that only countries characterized by a medium or high level of development have benefitted from a shift to school autonomy in recent years, while low-income economies may have been dragged back by decentralisation.
[6] Two notable exceptions being Chaudhary and Garg, 'Does History Matter?' and Cinnirella and Schueler, 'Nation Building'.
[7] Provinces and districts were called *province* and *circondari*, respectively.

on this issue, but we make a step forward.[8] First, we improve significantly the identification strategy to assess the impact of the Daneo-Credaro Reform, especially at the aggregate (national) level. We do so by constructing a novel database on human capital and socio-economic variables at municipal level, instead of focusing on regional figures. Secondly, and perhaps more importantly, we fill a gap in the historiography by providing an in-depth discussion of the way that the Reform worked, which was previously neglected – even by Cappelli's article.

We show that centralisation substantially increased the pace of human capital formation in early XX century Italy. The municipalities that switched to increased state management and funding were characterized by a 0.43 percentage-point premium on the average annual growth of literacy between 1911 and 1921, compared to those that retained autonomy.

The paper is organized as follows: after this introduction, Section 2 presents the literature review; Section 3 describes the Italian primary education system during the Liberal age, while Section 4 outlines the methodology and data used. Section 5 measures the impact of the Daneo-Credaro Reform on human capital accumulation and Section 6 reports some robustness checks; Section 7 draws the conclusions.

2. Literature review

Since Mitch pioneered a quantitative analysis of the effect of school reforms in Victorian England, the factors and policies shaping regional inequalities and national trends in public education have been explored more systematically through quantitative evidence. [9] Most of the ensuing research has adopted a political economy approach to explain the diffusion of schooling in the XIX century.[10] In contrast, other works have analysed the mutual link between education and the industrialization process.[11] Like our paper, both perspectives have focused on explaining regional disparities and the aggregate pace of human capital accumulation, which is an important issue for the Italian case. Moreover, we relate to this literature since it investigates how institutions shape the education system.

[8] Cappelli, 'Escaping from a human capital trap'.
[9] Mitch, 'The Impact of Subsidies to Elementary Schooling'.
[10] Cinnirella and Hornung, 'Landownership Concentration' for Prussia; Cvrcek and Zajicek, 'The Making of a Liberal Education' for Austria; Beltrán Tapia and Martinez-Galarraga, 'Inequality and Education in Pre-Industrial Economies' for Spain; Goñi, 'Landed Elites and Education Provision' for England and Wales and Andersson and Berger, 'Elites and the Expansion of Education' for Sweden.
[11] Semrad, 'Modern Secondary Education' for Bavaria and Diebolt, Le Chapelain, and Menard, 'Industrialization as a Deskilling Process?' for France.

Indeed, the historiographical debate on human capital formation in Italy has centred around the extent to which the country's municipalities could fund mass education. On the one hand, stressing the political-economy mechanism, A'Hearn and Vecchi have argued that municipalities where land inequality was strong did not raise surtaxes to fund primary schooling; according to their thesis, fiscal capacity was limited because of elites capturing local institutions,[12] an idea that dates back to the seminal work of Engerman and Sokoloff.[13] On the other hand, Cappelli has argued that fiscal capacity was more independent from land inequality and suffrage than commonly thought, and that economic performance is what ultimately allowed municipalities to allocate more resources to improve inputs into schooling – even though the mechanism discussed by A'Hearn and Vecchi certainly played some role.[14] Independently of the actual channel of causation, the literature agrees on the problematic nature of Italy's decentralized primary-school system in the late XIX century and its detrimental impact on innovation and technological progress.[15] Drawing on descriptive evidence, Vasta maintained that the introduction of centralized primary schooling improved the spread of education in the country.[16] Later on, Felice and Vasta have provided new estimates for the Human Development Index (HDI) for Italy's regions. Based on this, they have argued that the Daneo-Credaro Reform, which centralized primary schooling in 1911, improved regional convergence in education.[17] Cappelli has used regional (today's NUTS2) data on primary schooling to preliminarily explore this argument. He finds that regional convergence in education seems to have accelerated in the period 1911-1936 following the Daneo-Credaro Reform. Despite this, no research has yet explored whether the Reform can be seen as a structural break that improved the overall pace of human capital accumulation – mostly due to the lack of a solid identification strategy and fine-grain figures on schooling at the municipal level.[18]

3. The Italian primary education system from the Casati Law to the Daneo-Credaro Reform

[12] A'Hearn and Vecchi, 'Education.'
[13] Engerman and Sokoloff, 'Factor Endowments'; Cinnirella and Hornung, 'Landownership Concentration'.
[14] Cappelli, 'One Size'.
[15] Nuvolari and Vasta, 'The Geography of Innovation in Italy'.
[16] Vasta, 'Capitale umano, ricerca scientifica.'
[17] Felice and Vasta, cit.
[18] Cappelli, 'Escaping from a human capital trap'.

Italy's first national primary education system was established during the process of political unification, in 1859, through the Kingdom of Sardinia's Law no. 3725/1859, known as the Casati Law – and later extended to all the other areas that became part of the new Kingdom of Italy.[19] The administrative system was very decentralized – as primary schools were both funded and managed by the *comuni*, Italy's municipalities: each of them was obliged to build schools, hire teachers and enforce compulsory attendance. The Law put in place a system of compulsory yet free-of-charge primary education, organized in a two-year period for all students and two additional years for the children who lived in municipalities with more than 4,000 inhabitants, as well as those that had existent secondary schools. Under this system, education had to be offered free of charge, proportionally to the municipalities' spending capacity and according to their people's need. Only some formal aspects of the school system were centralized: the Ministry of Education set forth the curricula and established national norms regulating schooling and the teachers' wages. Essentially, such a decentralized system was lacking any sort of redistributive mechanism aimed at reducing the remarkable regional inequalities in local fiscal capacity (Figure 1).

[Figure 1 here]

The law established very weak enforcement mechanisms. Local bodies, like the *Consiglio scolastico provinciale* (provincial school board, from now on CSP), were meant to improve enforcement through a tighter control by the prefect on behalf of central authorities; yet, under the Casati Law, these often played a merely advisory role. Furthermore, the legislation put forward the existence of sanctions for parents and schools that did not make an effort to respect compulsory attendance that, according to the legislation, had to be found in the penal code; yet, no rules concerning the attendance of primary schools were actually included in the penal code before 1877, so that this legislative vacuum greatly hampered attendance in the early development of the country's education system. Except from the norms contained in the Law no. 3961/1877 (Coppino Law), which homogenized the system across regions and introduced light sanctions for nonattendance, the Casati Law remained basically unchanged until 1911.

Qualitative evidence contained in coeval inquiries suggests that the decentralized system hampered the development of basic education in some areas – especially in the South of the

[19] Cives, 'La scuola elementare'; Semeraro, *Il sistema scolastico italiano*.

country.[20] Overall, Italy's human capital formation was slow: in 1870, the adult literacy rate (15+) was 32 per cent in comparison with values ranging from 69 per cent in France, 76 per cent in the United Kingdom and 80 per cent in Germany.[21] Furthermore, the country was characterized by large regional disparities in literacy. While the province of Turin in the region of Piedmont scored high (67 per cent), the province of Caltanissetta in Sicily had an average adult literacy rate of 11 per cent, which was closer to the adult literacy in Northern Africa than to that of the European periphery, based on estimates by Prados de la Escosura.[22] Still on the eve of the XX century, the Italian gap with the most advanced countries was large (Table 1) and, at the same time, the regional divide remained one of the main problem of the Kingdom.[23]

[Table 1 here]

The public debate on the opportunity to centralize the primary education system gained momentum in the early XX century, when adult (15+) literacy rate was still as low as 29 per cent in the South and Islands. Although right-wing parties and Catholic movements had long opposed a more decided intervention by the central government in local matters, only after 1900 a convergence between moderate forces on both sides of the parliament and growing demands by state-school teachers to improve their living standards brought about an intense debate.[24] Some preliminary interventions were introduced by the Law no. 45/1903 (Nasi Law), the Law no. 407/1904 (Orlando Law) and the Law no. 383/1906 (Special Law for the Southern regions of Italy). However, a decisive push to the debate about the opportunity for the state to step in more decidedly in matters concerning primary education was provoked by the Corradini Inquiry.[25] This monumental exploration of the state of Italy's primary schooling at the beginning of the XX century provided thorough information on the situation concerning the

[20] For an extensive overview of the diffusion of primary schooling at the end of the XIX century, see Ravà, 'Relazione a S. Eccellenza'.
[21] Pamuk and van Zanden, 'Living Standards'.
[22] Prados de La Escosura, 'Human Development in Africa.'
[23] Felice and Vasta, 'Passive Modernization?', tab. A2 supplementary material; Nuvolari and Vasta, 'The Ghost in the Attic?', fig. 1. For a new analysis focusing on the evolution of the human-capital regional gap from 1821 to 1911, see Ciccarelli and Weisdorf, 'Pioneering into the past'.
[24] For a detailed account of this debate and further readings on the issue, see D'Ascenzo, *Tra centro e periferia*, chapter 1.
[25] The Inquiry, known by the name of Camillo Corradini, was published by the Ministry of Education, see Ministero della Pubblica Istruzione, 'L'istruzione primaria e popolare in Italia'. Camillo Corradini, born in Avezzano in 1867, had previously worked as Secretary of the Ministry of War, and was appointed head of the cabinet at the Ministry of Education in 1903. He gained extensive experience by cooperating with the Minister of Education on the draft of the Orlando Law approved in 1904, and later became General Manager of *Istruzione primaria e popolare* (Primary and Mass Education) in 1908.

condition of schoolhouses, the capability of teachers and their wages, and an assessment of the country's educational regional divide. The results, which highlighted limited regional convergence and poor national performance in schooling, resonated strongly in political circles.

Following renewed pressure to foster schooling in the early-XX century, a clear shift was introduced by the Law no. 487/1911, known as the Daneo-Credaro Reform, named after the two Ministries of Education who conceived the new norms on the soon-to-be centralized education system.[26] The centralization of primary schooling led, first, to a rapid reorganization of the central administration. Three major changes had already been achieved by 1912: the strengthening of the administrative capacity of the Ministry;[27] the formation of the CSPs, where a large share of its members no longer came from the city councils and local political circles but were rather nominated by the central administration;[28] an increase in the number and functions of ministerial inspectors.[29] Following this, the change in the school system was twofold. On the one hand, funding by the state was expanded far more than it had ever been: the central administration was now fully committed to pay teachers' salary – with an immediate increase equal to 100 Lire, i.e. between 7.5 and 18 per cent of a teachers' salary, depending on their working conditions;[30] the state also expanded funding for the construction of new schools through interest-free loans, starting in 1912. Expenditure on primary schooling as a share of the total budget allocated to education grew from 9 per cent in the early years of the XX century to 39 per cent in 1914.[31] Indeed, during 1912 and the first six months of 1913, 61 million Lire were allocated to the construction of new buildings, roughly 40.6 million Lire per year. This figure was immense compared to previous periods: between July 1878 and July 1900, 42 million Lire had been destined towards the same goal, amounting to 1.82 million Lire annually. Even during the first, mild phase of state intervention (1900-1911), state funding for the building of schools had been equal to c. 40 million Lire, 3.33 million Lire per year.[32] Coccìa and Della Torre show that this was a clear-cut discontinuity due to the centralization brought about by the Daneo-Credaro Reform.[33]

On the other hand, the Reform introduced crucial institutional changes. First, the CSP was re-designed to manage resources allocated to primary education and to hire teachers, with

[26] Edoardo Daneo and Luigi Credaro were ministers of education in 1909-10 and 1910-14, respectively.
[27] For a detailed account on the application of the Reform, see Ministero della Pubblica Istruzione, 'L'istruzione primaria e popolare in Italia', volume 4, pp. XXII-XXIII.
[28] Ibid., pp. XXV-XXVIII.
[29] Ibid., pp. XVIII-XXIX.
[30] Ibid., pp. XXXVIII-XLI; Pruneri, 'Pluriclassi'.
[31] Luzzati, 'Introduzione allo studio'.
[32] Ministero della Pubblica Istruzione, 'L'istruzione primaria e popolare in Italia', pp. LXXII.
[33] Coccìa and Della Torre, 'La ricostruzione'.

15 members now elected – or nominated by the Ministry – among personalities linked to primary schooling. Therefore, the CSP became the government's 'armed wing' to fight inefficiencies linked to the local (municipal) provision of schooling. Furthermore, the decisions made by the CSP had to be assessed by a delegation of the government, presided by the prefect, which was placed in each provincial capital (*delegazione governativa*). The latter approved the CSP proposed budget and gave the last word on the projects needed to foster primary education within the provincial territory (art. 13). This change was crucial, in that the management of the schools was taken away from the city councils; at the same time, the CSP could redistribute resources to the municipalities that most needed them, overcoming the trap represented by limited fiscal capacity.[34] Further institutional change was represented by a reorganization of school types, with new importance attached to evening schooling and the education of adults. Therefore, the Reform affected the schooling and literacy of people beyond primary education. Because of this, older cohorts that had not been schooled and thus were characterized by high illiteracy, could now receive some education. Finally, private schools were also monitored more tightly by the state.

We posit that these were fundamental changes, and we argue that they were more important than the increase in state funding following the Reform. Indeed, state funding had been also increased prior to the Daneo-Credaro Law, but the majority of the state resources were allocated to rich municipalities, because the allocation of the subsidies depended on applications by city councils – and rural and poor ones had limited capability in doing so. Because of this, the shift of school management from the municipality to the CSP and the *delegazione governativa* was central to school effectiveness: the new institutions were created with the aim of fostering primary education and their existence bypassed problems linked to the local lack of capability to fund and expand mass schooling.

4. Data and methods

The history of the Italian primary school system provides a solid identification strategy to improve our understanding of the impact of centralized education on school outcomes in the medium and long run. Our quasi-experiment draws on a peculiar feature of the Daneo-Credaro Reform of 1911: while most of the country's municipalities (*comuni*) were forced to shift to

[34] Although the municipalities still largely managed the building of schools. According to the Bill, the municipalities that were included in the CSP system had to transfer an amount of money equal to their previous year's budget to the Treasury (art. 17). The government would then redistribute these funds to the CSP (art. 19), which would in turn finance education (art. 20).

centralized primary education (treatment group), other municipalities, mainly the provincial and district capitals, were excluded ex-ante from the state-regulated administrative structure (control group). This allows us to compare the educational performance of the *comuni* that shifted to centralisation with those that retained autonomy. As we explain in more detail below, given the absence of pre-1911 historical municipal data, PSM is the best methodological tool available to explore the impact of the shift to centralisation on the growth of literacy rates in the period 1911-1921 and, as a robustness check, also from 1911 to 1931.[35] Since the treatment and control groups are inherently different concerning a range of features that we observe (mainly schooling and education), PSM is an effective tool to study the impact of the Reform as if it were a randomized experiment.

Given that the assignment into treatment concerned the municipal administrative level, our analysis needs to focus on municipal data. This level of disaggregation has rarely been reached in economic analyses of the early XX century. Therefore, we first assemble a brand-new cross-sectional dataset on economic, social and institutional variables for a 10-per cent stratified sample representative of all Italian municipalities. We include all the *comuni* that – following the Daneo-Credaro Bill (art. 14) – were granted autonomy in education policy, i.e., the provincial and district capitals (272 observations). We include all of them given their small number compared to the total number of other Italian municipalities (8,000+). As far as the treatment group is concerned, we draw 802 observations by calculating, for each province, the number of municipalities and their cumulative population, excluding district and provincial capitals. Then, we calculate the share of these provincial figures in the national total (excluding capitals). We use this figure on provincial population as a share of the national total to understand how many observations (municipalities) we need to draw from each province. To form the final sample, we generate random numbers without replacement.[36]

[35] The last educational reform prior to World War II was conceived by the philosopher Giovanni Gentile, Minister of public education, and approved when Fascism was on the rise (1923). In adopting a more elitist view of education, the changes concerned mainly secondary education, while primary schooling was left largely intact, especially as far as the funding of schools was concerned.

[36] For example, Ancona's population, excluding the capitals, comprises 1 percent of Italy's population; therefore, we need to extract 1 percent of our sample from this province. However, discrepancies between the provincial weight in terms of population and in terms of the number of municipalities may introduce misrepresentation in our sample (e.g., many provinces in the North had several municipalities but of a very small size; therefore, their actual bearing in the population would undetected by our sampling scheme). For this reason, we calculate the median municipal population of each province (after ordering municipal populations) and randomly draw half of the observations from above the median and half of them from below it. It is worth noting that for the provinces that contain less than 0.5 percent of Italy's population (e.g., Livorno and Ravenna), we still draw four municipalities to be able to collect data from both above and below the median. If the number of municipalities to be drawn is uneven, we take the one in excess from above the median.

To check to what extent our sample is representative of the underlying population of Italian municipalities, we compare the 1911 literacy rates (age group 6+) across the country's provinces calculated through our sample (provincial mean from municipal data) with figures on literacy obtained from census data on Italy's whole population (same age group); the index of correlation reaches 0.99, which is confirmed by the pattern shown in Figure 2.

[Figure 2 here]

We focus on the impact of the Daneo-Credaro Reform on literacy, which is the only index capturing educational outcomes available at the municipal level in 1911. It is worth noting that, in the case of Italy, literacy seems to be a good index of human capital for the early XX century: it correlates strongly with both the primary Gross Enrolment Ratio and the Average Years of Schooling of the population (Figure 3). Furthermore, literacy is one of the main factors prompting the first steps of economic development. Indeed, some authors have shown that primary education played a crucial role in fostering economic growth via innovation in the late XIX century, in Italy as well as other regions or countries, such as Prussia.[37]

[Figure 3 here]

At the municipal level, data on literacy are not available by age in historical censuses. For this reason, it is impossible to assess the impact of school centralisation on the human capital of people within different age groups. Furthermore, data on literacy were not published at the municipal level before 1911, which undermines our capability to follow pre-reform trends in literacy for each municipality in our sample.[38] Thus, we cannot rely on a DiD set up. However, we include the 1911 level of literacy in the probit regression used to estimate the propensity scores for our PSM model: this means that only municipalities with a very similar level of literacy – hence the same level of development and scope for future human capital accumulation – will be compared to assess the impact of the Daneo-Credaro Reform, limiting potential bias due to different pre-treatment trends. Finally, it is very likely that the Daneo-Credaro Reform had a stronger impact on the literacy of children aged 6 to 10 – i.e. in

[37] Cinnirella and Streb, 'The Role of Human Capital'; Nuvolari and Vasta, 'The Geography of Innovation in Italy'.

[38] We contacted several archives to explore the possibility that municipal data may still exist unpublished. However, these major archives have replied that the original sources are no longer available. Previous censuses were published between 1861 and 1901 at ten-year interval (apart from the year 1891) but unfortunately, they did not provide figures on literacy at the municipal level.

compulsory-school age. Since we can only use the literacy rate for the total population (age 6+) as a dependent variable, our results should be probably read as a conservative estimate of the actual impact of centralized education. Despite this, one may note that age cohorts other than 6-10 commonly enrolled in primary schools, so that the potential measurement bias due to using literacy 6+ should not be large. Children and youngsters aged 0 to 20 (who to some extent were affected by primary schooling in the period 1911-21) represented 44 per cent of the total population of the Kingdom of Italy in 1921.[39] All of this means that the literacy rate 6+ captures fairly well the literacy rate of the population affected by the Reform in the period that we study.

Considering the above, our outcome variable is the average annual growth of literacy (population 6+) in 1911-1921, calculated based on the 1911 and the 1921 censuses, while all other variables are measured at the beginning of the period. The treatment is identified by the Reform dummy, i.e., a variable that takes a value of one if a municipality shifted to centralized primary education. To capture the impact of the Reform, we track whether – and *when* – each of the municipalities in our sample shifted to centralized primary education. The Law was implemented through dozens of Royal Decrees, each of which normally concerned very few municipalities under the same district or province. We use the information contained in the *Gazzetta Ufficiale* (Official Gazette of Italy) to reconstruct the pattern of diffusion and implementation of the Reform. We discover that all the *comuni* in our sample that were bound to change by the new norms – together with c. 100 district and provincial capitals that were supposed to retain school autonomy – moved to the centralized school system in 1914-15. The Law stated that the municipalities that were bound to shift to centralisation could apply to retain school autonomy within three years after the Reform was passed. Only 11 municipalities in our sample (1 per cent) did so, because obtaining this exception was very difficult. The municipalities that wanted to retain autonomy had to comply with severe constraints, one of which was to have an illiteracy rate less than 25 percent, according to the 1911 census. Despite this, local (often landed) elites pushed for keeping autonomy from the central government to retain their political power. The Law no. 346/1896 and the Law no. 103/1903 are examples of the success of local elites in that sense. According to the 1896 Law, all mayors became elective – while previously the majority of them was nominated by the central authority. The 1903 Law established municipal public utilities, which would manage the provision of public services at

[39] MAIC, 'Censimento della popolazione, 1921'.

the very local level.[40] It is worth noting that this feature does not affect our identification strategy, since we consider the 1911 literacy rates – one of the main constraints to retain school autonomy – in our models. The district and provincial capitals could also apply to shift to centralisation. A large share of them did so, provided the incentive to ease pressure on their budgets. Although there was no precise constraint to limit this shift, we overcome the potential problem of self-selection using the PSM. Indeed, we include variables that capture both material and immaterial factors that may have influenced this decision. It is also worth noting that many district and provincial capitals were often small and poor municipalities, which wanted to shift to state education given their limited schooling and literacy – as well as spending capacity and preferences concerning educational expenditure, which are included in the model.

Based on the pattern observed, we code the Reform dummy variable, assigning a value of 1 if the shift happened in 1914 or 1915. We choose this time span because our first aim is to explore the impact of the Daneo-Credaro Reform in the 1911-1921 period. For this reason, in our analysis, we do not consider moves to centralized schooling that occurred after 1916.[41] However, when including all units that shifted after 1916, our results are confirmed.

We also collect data on various aspects of Italy's municipalities. As previously explained, we include the initial level of literacy among the covariates because we expect municipalities with lower education to experience a faster accumulation of human capital over time. We draw on the Corradini Inquiry to collect detailed data on inputs into schooling across Italy's municipalities in 1907-08, since we expect them to affect the growth of literacy.[42] The Inquiry also provided several reports by inspectors who witnessed the state of schools across Italy's provinces and detailed data on municipal expenditures, teachers, school houses, didactic material, performance, and state subsidies. We rely on the Inquiry to draw municipal figures on compulsory primary schools per 1,000 citizens, elective schools per 1,000 inhabitants,[43] the ratio between state and total expenditure on primary education,[44] the share of educational expenditure on the municipality's total budget, municipal per capita expenditure on primary education.

[40] Fari and Giuntini, 'Public utilities'.
[41] A handful of municipalities regained autonomy between 1916 and 1917. Therefore, in these cases, the Reform dummy is equal to 0, given the short amount of time available to implement the Reform.
[42] Ministero della Pubblica Istruzione, 'L'istruzione primaria e popolare in Italia'.
[43] Elective schools were schools that the municipalities were not obliged to build according to the legislation.
[44] This is the sum of municipal and state expenditure.

Other important variables that can affect the development of primary schooling are included to capture supply and demand-side determinants of education. We include three variables that capture geographic features potentially linked to schooling: the altitude above sea level is aimed to capture ruggedness, which is known to have determined access to schools. Access to sea is included to capture the openness of local environment to trade and cultural flows, and it is coded as a dummy equal to one if part of the municipal district is less than five kms away from the coast. The linear distance from the closest university (in kms) measures the existence of a cultural environment promoting education and knowledge.

The share of local electors – people allowed to vote in municipal and provincial elections – on total population is constructed from electoral statistics published in 1897, which is the closest available publication to 1911 reporting municipal data.[45] The number of local electors reflects the growing importance attached to political voice and accountability by the literature on the rise of mass education. It is worth noting that, at the municipal level in 1911, it is impossible to obtain data on land inequality. However, voting rights prior to 1911 depended to a great extent on access to land.[46] We capture the demand for skills by drawing figures on industrial workers per 1,000 inhabitants and the number of industrial HP per 1,000 inhabitants from the 1911 industrial census, which was published in 1913.[47] We include population density drawn from census data to proxy for the role played by agglomeration economies in the formation of skills. Moreover, we build a new dataset of military casualties in World War I per 1,000 inhabitants for each Italian municipality – based on individual data for more than 500,000 soldiers. This factor is important in that human capital growth may have been affected by the different geographical incidence of conflict-related deaths. The literacy of conscripts was 70 per cent, thus high compared to the 62 per cent characterizing the total population.[48] Therefore, we may expect casualties to have lowered the overall growth rate of literacy in the decade 1911-21. If we find a positive impact of the Reform on human capital accumulation, this mechanism will reinforce our result, because in the absence of deaths linked to World War I (anyways included in our OLS model) the growth of literacy would have been even higher.[49]

Table 2 reports the summary statistics for all variables included in our dataset.

[45] MAIC, *Statistica elettorale*.
[46] Engerman and Sokoloff, 'Factor Endowments'; Cappelli, 'One Size'.
[47] MAIC, *Censimento degli opifici*.
[48] MAIC, *Annuario Statistico*.
[49] It is important to note that World War I deaths were rather evenly distributed across regions, according to our data. Furthermore, starting in 1909, the literacy of conscript was not self-reported, but checked by the authorities through a written and oral exam. MAIC, *Annuario Statistico*.

[Table 2 here]

The issue of migration must be also discussed, since it may have affected the change in literacy via the educational selection of people moving to other regions or abroad;[50] systematic municipal data on the selection of internal and outward migrants do not exist. One exception is the recent research by Spitzer and Zimran, who relied on data on the heights of Italian migrants going to the US (Ellis Island) in the period 1907-1925.[51] They find that, at the national level, migrants were negatively selected. By contrast, within provinces, migrants tended to be positively selected, and the selection was more positive in the poorest areas of the country – such as the rural and the Southern regions. Thus, in the absence of migration, the growth of literacy due to the Reform would have been even larger: since the selectivity was more positive in the most backward regions of the country, and given that migration rates were higher in those areas, such a mechanism is likely to reinforce our results. It is more difficult to speculate on the direction of the bias linked to internal migrations, because of the absence of detailed data on such flows. If migrants moving to the cities were positively selected, this would also reinforce our argument about the Reform, since in the absence of migration the growth of literacy in the control group (mainly district and provincial capitals) would have been even lower. However, Beltrán Tapia and Miguel Salanova have shown that, in the case of Spain, short-distance migrants to the smaller cities were negatively selected from the underlying population.[52] Although we do not have a prior on this specific issue, it is worth stressing that internal migration in Italy was much less important than migration abroad. Therefore, even if this brought about some measurement bias, we do not expect this issue to affect our results to a great extent.[53]

5. The impact of the Daneo-Credaro Reform

In this Section, we introduce our first approach, which is an OLS model used to regress the growth of adult literacy rates in 1911-1921 on the Reform dummy (treating the Reform as if it were a randomized experiment, see Equation 1). Although we place more emphasis on the PSM identification later in the paper, this methodology allows us to explore some other aspects of human capital accumulation beyond the impact of the Reform. The coefficient of this OLS

[50] Hatton, 'The cliometrics of international migration'; Abramitzky, Platt Boustan and Eriksson, 'Europe's tired'.
[51] Spitzer and Zimran, 'Migrant self-selection'.
[52] Beltrán Tapia and Miguel Salanova, 'Migrants' self-selection'.
[53] Federico, Nuvolari and Vasta, 'The origins of the Italian regional divide', Table 7.

estimation might be biased given the potential endogeneity of the assignment into treatment, because policy makers wanted the Reform to push the most disadvantaged areas down a path of human capital-sustained growth. In the second model, the initial literacy rate in 1911 is included to capture the negative relationship between literacy rates and their growth over time given an upper bound of 100 per cent. In a third model (Equation 2), we also include the vector X of control variables that we presented in the previous Section.

$$litgr_i = \beta_0 + \beta_1 ref_i + \varepsilon_i \quad (1)$$

$$litgr_i = \beta_0 + \beta_1 lit_i + \beta_2 ref_i + \beta_3 X_i + \varepsilon_i \quad (2)$$

Table 3 shows the results based on the three OLS models outlined above. The shift to centralisation introduced by the Daneo-Credaro Reform is associated with a positive premium on human capital accumulation. In column 1, the premium given by the Reform to the average annual growth rate is equal to 0.89 percentage points, a remarkable effect considering that about 80 per cent of municipalities in the sample experienced a growth of literacy ranging between zero and three per cent. As we discussed earlier, we expect the coefficient of the Reform dummy in equation 1 to be biased because of the potential endogeneity of the assignment into treatment with respect to the condition of schooling in Italy's municipalities. Columns (2) and (3) in Table 3 show the estimated marginal impact of the Reform on the growth of literacy once other factors related to the municipalities' social, economic and institutional features – as well as their historical performance in terms of human capital growth – are considered. Once they are controlled for, the premium on the pace of human capital accumulation given by the Reform oscillates around 0.20 percentage points per annum. In column 4, we interact the Reform dummy with a dummy variable for the municipalities belonging to the South (including Sardinia and Sicily), to understand if the marginal impact of the Reform on the growth of literacy was larger in the South compared to the North. This specification shows that this was indeed the case (the marginal effect of the Reform is statistically significant at 1 percent). Finally, we explore whether school centralization was most effective in the municipalities characterized by limited access to electoral franchise (column 5). The coefficient of the interaction between the latter and the Reform dummy shows that the impact of centralization was greater where franchise was most restricted.

Overall, it is worth noting that the increase in the R^2 throughout the specifications outlined suggests that pre-treatment factors played a role in the growth of literacy between

1911 and 1921. Because of this, in the next Section, we rely on PSM to test the link between the Reform and more rapid human capital accumulation even when potential self-selection is considerably reduced.

[Table 3 here]

Next, we study the impact of the Daneo-Credaro Reform by estimating the difference in the growth of the literacy rate between a treatment and a control group that are obtained through a selection on observables. We rely on PSM to assess the impact of shifts in education policy that cannot be fully considered a completely exogenous treatment.[54] In the case of early XX century Italy, we know that the 1911 Daneo-Credaro Reform required all Italian municipalities, except those from district and provincial capitals, to move towards centralisation. The Reform did generate a treatment and a control group, but the assignment to treatment itself depended on the level of literacy and economic development of the country's municipalities. District and provincial capitals (and a few other municipalities) were left with school autonomy because they were expected to fare better than the others. Therefore, a simple OLS regression may provide biased evidence on the impact of centralisation.

PSM aims at comparing very similar units that differ only in terms of assignment into treatment. This statistical tool matches observations between treatment and control groups using propensity scores, i.e., estimated probabilities of being subject to treatment based on observable variables, which in turn should be measured before the potentially endogenous treatment. Two assumptions must be unviolated to use PSM. One is unconfoundedness, the assumption that all variables that might influence *both* the assignment into treatment and the outcome are considered. This cannot be tested and must be evaluated by using economic theory and experience. The second assumption is common support, which means that we need enough units belonging to the two distinct groups with similar propensity scores (i.e., with a very similar chance to be treated).

The procedure followed to apply PSM is summarized here, while the detailed description of the methodology is presented in the Appendix A. We first estimate the propensity scores via a probit model.[55] Secondly, we visually assess if there is common support. Thirdly, we match

[54] For an overview on PSM, see Rosenbaum and Rubin, 'The Central Role of the Propensity Score'; for a recent application on PSM to investigate changes in education policy in XIX-century Bavaria, see Semrad, 'Modern Secondary Education'.

[55] We follow Heinrich, Maffioli and Vázquez, 'A Primer for Applying', who argue that the researcher should model selection into treatment using several core variables. They recommend expanding the number of factors included in different specifications as a robustness check.

the similar units in the treatment and control groups using the radius algorithm, which allows each unit in the treatment group to be matched to several ones in the control group – insofar as their propensity scores are within a certain caliper (distance) from the propensity score of the treated unit. Since we have a large treatment group and a relatively small control group this is a sensible approach.

Our core probit model for estimating the propensity scores is based on variables concerning education and primary schooling, which were directly observed by the policy makers and hence primarily influenced the criteria for assignment into the centralized system introduced by the Daneo-Credaro Reform.[56] The rationality behind this choice is that policy makers assumed that the provincial and district capitals were the best performers in terms of schooling, and they chose to exclude all of them from the post-1911 education system. Furthermore, other variables capturing inputs into schooling and educational outcomes should capture well the chance to be in the treatment condition, and, in addition, they should identify the latter less exactly – a property that is required in the estimation of propensity scores.

Table 4 shows the probit model used to calculate the propensity scores, where marginal effects are reported. All variables are highly statistically significant. Literacy rates, expenditure on education per capita and the extent of state funding have the expected signs. The positive sign of educational expenditure on total municipal budget can be explained by the fact that poorer municipalities (with a smaller budget) had to make a greater relative effort to comply with the national norms requiring city councils to provide primary schooling. Additionally, the poor municipalities that spent a large share of their budget for education, would be more prone to move to centralized education to relieve pressure on their balance sheets – indeed, the Southern municipalities called for more state intervention during those years, as discussed by D'Ascenzo.[57] Likewise, the positive signs of the school-per-capita variables makes sense once noting that every municipality had been obliged by the law to build at least one schoolhouse since the late XIX century; thus, the smallest municipalities were usually the ones characterized by the highest density of schools normalized to population.

[Table 4 here]

[56] A detailed summary of the parliamentary debate and on the process towards passing the Bill can be found in D'Ascenzo, *Tra centro e periferia*. On the extent to which policy makers looked at figures on schooling and literacy to implement school reforms, see Gomes and Machado, 'Primary education', on Portugal.
[57] D'Ascenzo, *Tra centro e periferia*, Chapter 1.

The distribution of propensity scores confirms that there is wide common support (see Figure A1 in Appendix A). Here, we report the literacy rate for the treatment and control groups in 1911 (Figure 4), because literacy certainly represents human capital across Italy's municipalities; yet, it also captures other important aspects of economic and social development that we cannot observe directly through our data at the municipal – such as income per capita, health, fiscal capacity and innovation capabilities.[58] Figure 4 shows that there is a clear similarity between the two distinct groups – district and provincial capitals and other municipalities in our sample –, which means that they are indeed comparable.

[Figure 4 here]

We rely on a PSM procedure that imposes common support automatically. Table 5 shows that the means of each variable in the matched (M) samples are extremely balanced compared to the original unmatched sample (U) at the 5-per cent level, i.e., they are approximately equal between the treatment and control group after matching according to the distribution of our observable variables. As a robustness check, we also drop municipalities with extreme propensity scores – i.e. lower than 0.1 and larger than 0.99, and the stricter matching procedure leads to a very similar balance between the two samples (Table A1 in Appendix A). Likewise, Figure 5 presents the radius matching when imposing common support, while the results when dropping extreme scores are presented in Figure A2 in Appendix A.

[Table 5 and Figure 5]

Based on our matched observations, Table 6 shows the magnitude and significance of the Average Treatment Effect on the Treated (ATT). The impact of centralisation is significant at 5 per cent, showing that the treated municipalities experienced an average annual growth of 2.28 per cent in 1911-1921, while the value was 1.85 per cent for the control group during the same period. This result implies a premium on human capital accumulation ranging around 0.43 percentage points per annum. The results remain virtually unchanged when considering the more restrictive matching procedure outlined above.[59]

[Table 6 here]

[58] UNDP, *Human Development Report 1990*; Felice and Vasta, 'Passive Modernization?'; Cappelli, 'One Size'; Nuvolari and Vasta, 'The Geography of Innovation in Italy'.
[59] We provide the results in Table A2 in Appendix A.

6. Robustness checks

This Section presents some robustness checks. First, we deal with the issue of pre-1911 trends in literacy, which could not be measured due, as said, to the lack of surviving historical data at the municipal level before 1911. Although XIX century censuses did not report municipal data, they did present provincial figures on literacy rates. Therefore, pre-reform trends can be reconstructed for Italy's 69 provinces at ten-year intervals between 1881 and 1921 (T = 4).[60] We can thus rely on such data to run a pseudo-DiD model aimed to assess the impact of the Daneo-Credaro Reform across Italy's provinces over time. We call this robustness check a pseudo-DiD because we are just assuming that a province was treated if the share of its municipalities that switched to centralized education was greater than the median (see below). Since there is a lot of heterogeneity within provinces, this approach should be considered as a second best with respect to PSM with municipal data, but it is nonetheless complementary, to explore pre-treatment trends in literacy rates between the pseudo-treatment and pseudo-control groups.

To implement the pseudo-DiD, a synthetic pseudo-treatment at the provincial level is constructed, by using an official Inquiry on primary education published by the Italian government in 1923.[61] This volume reports the number of municipalities in each province that retained school autonomy, as well as the number of those that shifted to the new centralized system. Our pseudo-treatment in this approach is based on the share of total municipalities within each province that shifted to centralized education after 1911. The pseudo-treatment dummy for the cross-sectional units is equal to 1 if the share of municipalities administered by the central government after 1911 is larger than the median, which is 97.4 per cent, while the time-dimension of the pseudo-treatment is identified by a dummy equal to one for the period 1911-1921, so that we assess pre-1911 trends and whether the Daneo-Credaro Reform prompted any change in the period 1911-21.[62] The dependent variable is the average annual growth of literacy in each period. Table 7 shows the results of this alternative approach. It appears that provinces that experienced the Daneo-Credaro Reform to a different extent where on a common path of human capital accumulation before 1911, while throughout the decade

[60] Since the 1891 census was not held due to financial reasons, we have estimated literacy figures for this year by means of linear interpolation.
[61] Ministero della Pubblica Istruzione, *Relazione sul numero*.
[62] For a similar use of the median to identify treatment and control groups, see Havnes and Mogstad, 'Is Universal Child'.

1911-1921 literacy rates between the two groups grew at a different pace. Indeed, the difference in the differences of the growth rates that we find post-1911 is positive and equal to a 0.6 percentage-point premium, being statistically significant at the 5-per cent level. Changing the threshold that identifies the pseudo-treatment (e.g. from the median to the 25th or the 75th per centiles) does not change the results, showing that the pre-1911 trends were similar between the pseudo-treatment and pseudo-control groups. Figure 6 shows the pre-treatment and post-treatment trends for both the pseudo-treatment and pseudo-control groups.

[Table 7 and Figure 6 here]

Next, we test whether methodological issues undermine the validity of our PSM results. First, we expand the probit model used to calculate the propensity scores by including all observables (Table B1 in Appendix B). The results are consistent (Table B2 in Appendix B) with those presented in the previous Section. Second, we expand our list of matching algorithms by adding nearest-neighbourhood (Table B3 in Appendix B) and kernel matching (Table B4 in Appendix). The results remain virtually unchanged when tested through these checks.

Moreover, we test whether the results on the importance of centralisation hold when considering the 1911-1931 period. It is worth noting that in the second half of the 1920s Fascism ruled Italy; yet, this did not significantly change the organization of primary schooling, since the Gentile Reform of 1923 left the centralized system basically unchanged. Thus, the effect of the 1911 Reform can still be tested throughout the 1920s, to assess whether the impact of the Daneo-Credaro Reform was consolidated over time. Since we are now considering this longer time span, the Reform dummy is coded to consider also the small number of municipalities that shifted to centralized education between 1916 and 1922. Table 8 shows evidence based on the core probit model for the calculation of the propensity scores and radius-matching algorithm with common support.

[Table 8 here]

The standard procedure shows a premium equal to 0.58 percentage points; once a more satisfying matching is performed by dropping extreme scores, the magnitude of the estimated impact of the Reform on the growth rate of literacy remains around 0.40 percentage points per annum (Table B5 in Appendix B). This finding is somewhat surprising because a strong cumulative effect could be expected as time goes by. However, the strong coalition supporting

centralized schooling, which had risen in the first decade of the XX century, was heavily weakened by the length of the administrative procedures that were necessary for its implementation, as stated by De Fort.[63] Furthermore, as we explained in previous sections, the Reform took some years before it could be fully applied to most of Italy's municipalities. Naturally, this was also due to World War I, which absorbed many resources and diverted the attention from schooling to more strategic aims.[64] This disappointed the public at times, given the high expectations that the Daneo-Credaro Law had elicited among teachers, local policy makers and the populations involved. Finally, the funds devoted to the improvement of schoolhouses and teaching proved insufficient towards the end of the 1920s. This fact illustrates how the Reform was a success but was not convincingly supported by successive governments. By the same token, the rise of Fascism and of the idea of a more elitist education characterized by fewer but more efficient primary schools likely limited the efficacy of the Daneo-Credaro Reform further during the 1920s.[65]

Finally, provided that in the OLS models (Table 3) we find the Reform to have had a larger impact in the South, we need to further explore heterogeneity in the PSM. Indeed, by relying on the variables previously introduced, we might still match municipalities located in different macro-areas, which could have different preferences towards state intervention. Therefore, in a robustness check, we rerun the core PSM model by adding a dummy for the South and Islands capturing potential heterogeneity linked to location in different macro-areas of the country. The results in Table 9 show that the impact remains significant compared to the one observed in Table 6. Indeed, the estimated effect of the Reform is still statistically significant, even though the magnitude is somewhat smaller.[66]

[Table 9 here]

7. Conclusions

This article has offered fresh evidence on the role that school centralisation can play in the process of human capital formation. Italy's peculiar shift to centralized primary education at the beginning of the XX century allowed us to estimate the impact of school centralisation across Italian municipalities. Centralisation played a positive role within a country that was

[63] De Fort, *La scuola elementare*.
[64] *Ibid*, chapter 4.
[65] The Fascist Regime centralized education for the district and provincial capitals as well, in the early 1930s.
[66] We have also tested other combinations by using dummies for other macro-areas, alone or together. Results are consistent with those presented here.

characterized by low levels of education and very large regional disparities in human capital – which implied a low rate of convergence towards the economic leaders of that time. This evidence allows to add a new case to the historical empirical literature on the factors that prompted human capital formation during the rise of mass schooling. In the early XX century, under a decentralized education system, some areas of Italy lacked enough resources to fund primary schooling; this, in turn, delayed social and economic growth in the rural and southern regions of the country.[67]

The Daneo-Credaro Reform of 1911 started to push Italy down a path of development characterized by a faster pace of human capital accumulation. Our estimates suggest that the municipalities that shifted to centralized education were characterized by a premium on the average annual growth of literacy equal to 0.43 percentage points, compared to those that retained autonomy. Our identification strategy, based on the Reform design and PSM, confirms that the link between the shift to centralisation and faster human capital accumulation was indeed significant, and these results are robust to several different specifications and tests.

The Reform affected primary schooling positively through changes in funding and management. Although funding increased considerably and was certainly important, we argue that, without any change in the administrative structure of the school system and school management, the Reform would have had a more limited impact on literacy.

The centralization that we describe improved the overall functioning of the school system and the efficiency of school funding. First, by reducing the distance between the central government and the city councils through the CSP. The control by the Ministry not only reassured the teachers that their salary would be increased; more importantly, the government could now guarantee that they would get paid regularly, which was not always the case when the municipalities managed primary schooling. The inspective function by central authorities was also strengthened, hence improving the enforcement of the rules. Secondly, the range of school types was reorganized extensively to give more importance to evening schooling and the education of adults, meaning that adult literacy was also affected by the Reform. Thirdly, additional funding was provided to build new schools, with an increase that appears to have been very large with respect to previous years. This increase was relevant, but its impact was amplified by the reorganization of the school system and the supervision by the central authority: the funds could be directed where they were most needed.

[67] Felice and Vasta, 'Passive Modernization?'. Cappelli, 'One size'.

To sum up, in Italy, the 50-year persistence of decentralized primary schooling hampered the accumulation of human capital and regional convergence in basic education, thus casting a long shadow on the future pace of aggregate and regional economic growth. Centralizing primary education through the Daneo-Credaro Reform in 1911 was a major breakthrough, which fostered the spread of literacy and allowed the country to reduce the human-capital gap with the most advanced economies.

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Tables

Table 1 – Average years of schooling in major European countries, 1870 – 1910

Country	1870	1880	1890	1900	1910
France	4.04	4.93	5.76	6.63	7.35
Germany	5.25	5.55	5.96	6.36	6.92
Italy	0.88	1.40	2.00	2.58	3.06
Spain	2.43	3.11	3.90	4.51	4.93
United Kingdom	4.13	4.97	5.40	5.84	6.35

Source: Morrisson and Murtin, 'The Century of Education', table 3.

Table 2 – Summary statistics for the 1911-1921 period

Variable	N	Mean	SD	Min	Max
Average annual growth literacy 1911-21	1,073	2.15	2.36	-1.94	21.25
Reform	1,074	0.83	0.37	0.00	1.00
Literacy rate 1911	1,074	60.59	22.37	14.00	99.00
Compulsory schools x 1,000 inhabitants	1,074	1.70	0.77	0.00	12.05
Elective schools x 1,000 inhabitants	1,074	0.20	0.90	0.00	22.56
Education budget: state % total	1,074	0.19	0.12	0.00	1.73
Education total % municipal budget	1,074	0.21	0.11	0.02	0.90
Municipal expenditure education p.c.	1,074	3.08	2.61	0.39	27.97
Altitude MSL	1,074	288.71	267.32	0.00	1,725.00
Electors (local) / population	1,071	0.09	0.07	0.01	1.06
HP x 1,000 inhabitants	1,074	42.10	384.72	0.00	12,268.83
Industrial workers x 1,000 inhabitants	1,074	50.10	72.88	0.00	1,166.55
Population density 1911	1,074	3.18	12.75	0.02	344.00
Access to sea < 5 km	1,074	0.19	0.39	0.00	1.00
Linear distance from nearest university	1,074	66.66	56.83	0.00	334.00
WWI deaths x 1,000 inhabitants	1,074	15.48	5.67	0.00	49.45

Note: We are left with 1,073 observations for the growth of literacy in 1911-1921 because due to changes in municipal borders after 1921, the literacy data for the municipality of Precotto cannot be collected. The same problem applies to data on local electors. They were published in 1905, right before three of the municipalities in our dataset were created due to changes in municipal borders; this results in 1,071 observations.

Table 3 – OLS regression for the 1911-1921 period

Average annual growth literacy 1911-21	(1)	(2)	(3)	(4)	(5)
Reform	0.886***	0.200**	0.229**	-0.094	0.852***
	(0.000)	(0.018)	(0.038)	(0.317)	(0.000)
Reform x South and Islands regions (dummy)				0.749***	
				(0.001)	
Reform x Electors (local) / population					-7.231***
					(0.001)
Literacy 1911	N	Y	Y	Y	Y
Other control variables	N	N	Y	Y	Y
Reform x Electoral franchise	N	N	N	N	Y
Reform x South and Islands regions	N	N	N	Y	N
Observations	1,073	1,073	1,070	1,070	1,070
Adjusted R-squared	0.019	0.445	0.451	0.463	0.453

Robust p-values in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: The control variables (all referring to c. 1911 if not otherwise indicated) included in the third model are literacy rates (6+), compulsory schools per 1,000, elective schools per 1,000, state % total expenditure on education, expenditure on education % of municipal budget, municipal expenditure on education per capita, altitude MSL, access to sea (dummy = 1 if distance < 5 km), linear distance from nearest university, administrative electors % population (1897), industrial HP per 1,000 inhabitants, industrial workers per 1,000 inhabitants, population density, WWI deaths per 1,000 inhabitants.

Table 4 – Core probit model for estimating propensity scores, Reform (1911-1921)

Reform (mfx)	**(1)**
Literacy rate 1911	-0.002***
	(0.001)
Compulsory schools x 1,000 inhabitants	0.040**
	(0.018)
Elective schools x 1,000 inhabitants	0.190***
	(0.040)
Education budget: state % total	0.917***
	(0.139)
Education total % municipal budget	0.655***
	(0.106)
Municipal expenditure education p.c.	-0.016***
	(0.004)
Observations	1,074

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5 – Output of the matching procedure based on the radius algorithm imposing common support

Variable	**Sample**	**Mean of treated**	**Mean of control**	**T-test: p > |t|**
Literacy rate 1911	U	58.91	68.72	0.00
	M	58.76	60.68	0.07
Compulsory schools x 1,000 inh.	U	1.72	1.65	0.32
	M	1.68	1.66	0.39
Elective schools x 1,000 inh.	U	0.22	0.06	0.03
	M	0.13	0.13	0.67
Education budget: state % total	U	0.20	0.12	0.00
	M	0.19	0.18	0.03
Education total % municipal budget	U	0.22	0.17	0.00
	M	0.21	0.23	0.00
Municipal expenditure education p.c.	U	2.80	4.45	0.00
	M	2.82	2.92	0.27
	Sample	**Pseudo R squared**	**p > Chi squared**	**Mean bias**
Summary statistics	U	0.25	0.00	44.00
	M	0.01	0.01	6.90

Notes: U refers to the sample before matching, and M refers to the sample based on matched observations. The last column shows the p-value based on the null hypothesis that the difference between the means of the treatment and control groups is zero.

Table 6 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921

(A) Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1921	Unmatched	2.30	1.41	0.88	0.19	4.65
Radius (caliper 0.05)	Matched	2.28	1.85	0.43	0.17	2.53

Table 7 – Pseudo-DiD at the provincial level (N = 69) at ten-year intervals between 1881 and 1921 (T = 4). Dependent variable: annual growth of literacy in each decade

Outcome variable	**Annual average growth of literacy**	**S. Err.**	**P>|t|**
Before			
Control	0.020		
Treated	0.022		
Diff (T-C)	0.002	0.001	0.116
After			
Control	0.015		
Treated	0.023		
Diff (T-C)	0.008	0.002	0.001***
Diff-in-Diff	0.006	0.003	0.047**
R-squared: 0.05			

Note: means and Standard Errors are estimated by linear regression; *** p<0.01; ** p<0.05; * p<0.1.

Table 8 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model for the period 1911-1931

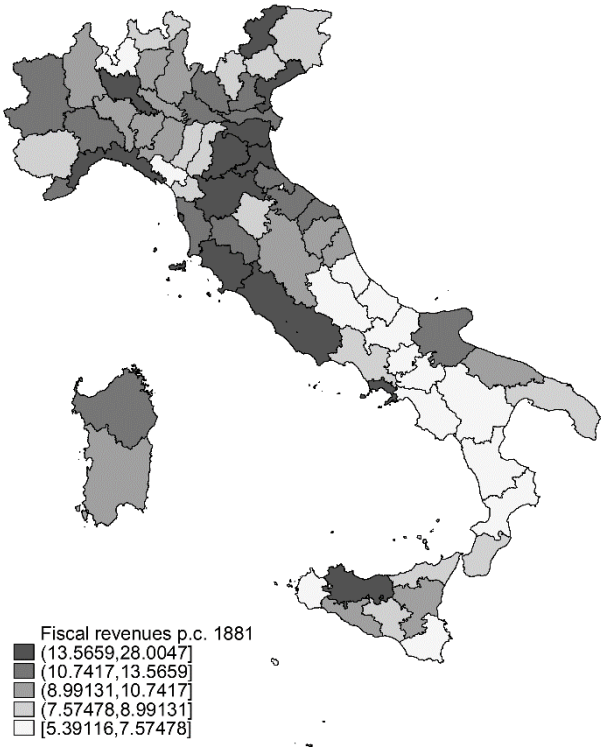
Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1931	Unmatched	2.00	1.05	0.96	0.15	6.52
Radius (caliper 0.05)	Matched	2.00	1.41	0.58	0.16	3.70

Table 9 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921, including a dummy for the South and Islands

(A) Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1921	Unmatched	1.41	1.41	0.88	0.19	4.65
Radius (caliper 0.05)	Matched	1.91	1.85	0.31	0.17	1.82

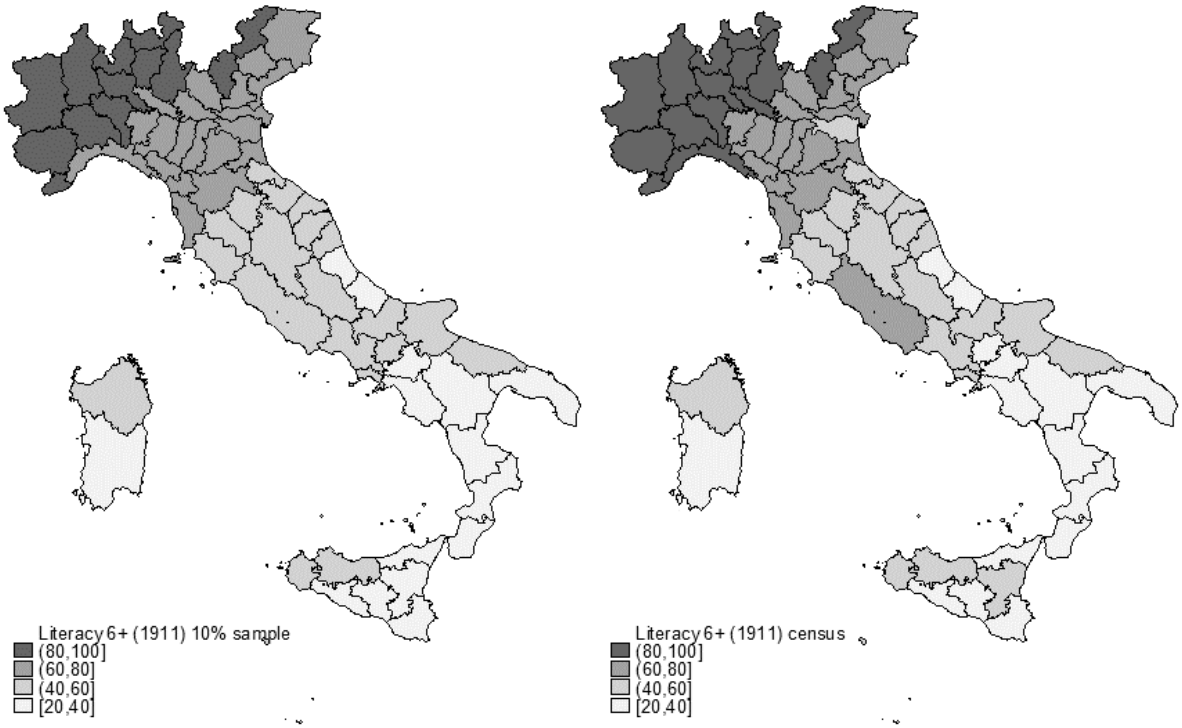
Figures

Figure 1 – Fiscal capacity: municipal ordinary fiscal revenues per inhabitant (current Lira), 1881



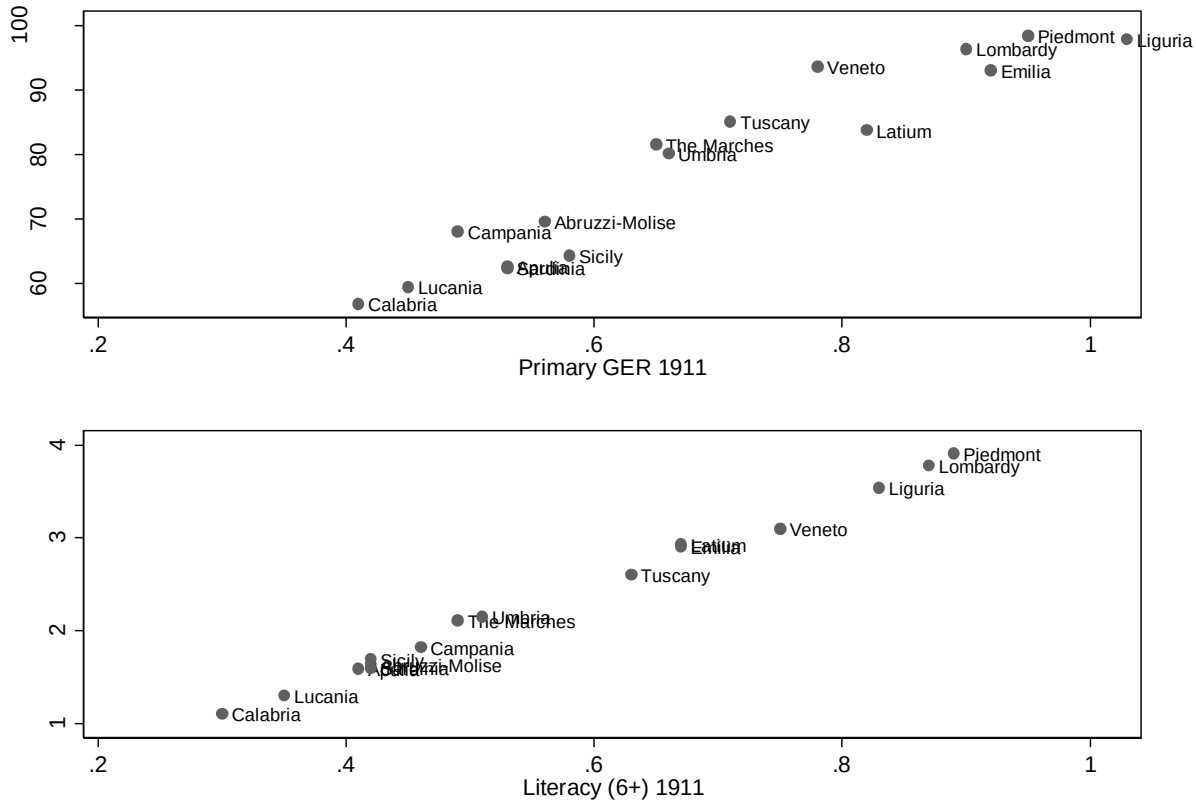
Source: MAIC, *Bilanci Comunali, 1880 e 1881*.
Note: maps are obtained from the Istat website: http://sistat.istat.it/

Figure 2 – Provincial literacy rates (age 6+) in 1911: sample data versus census data



Note: the Index of correlation is equal to 0.99; maps are obtained from the Istat website: http://sistat.istat.it/.

Figure 3 – Indices of human capital across Italian NUTS2 regions.



Note: The first graph shows the correlation between the literacy of people aged 15-19 in 1921 (Y axis) and the enrolment rate of the same cohort ten years before, in 1911 (X axis). The second graph relates the average years of schooling of the population with the literacy of people older than six in the same year (1911).

Figure 4 – Visual analysis of the distribution of literacy rates, 1911

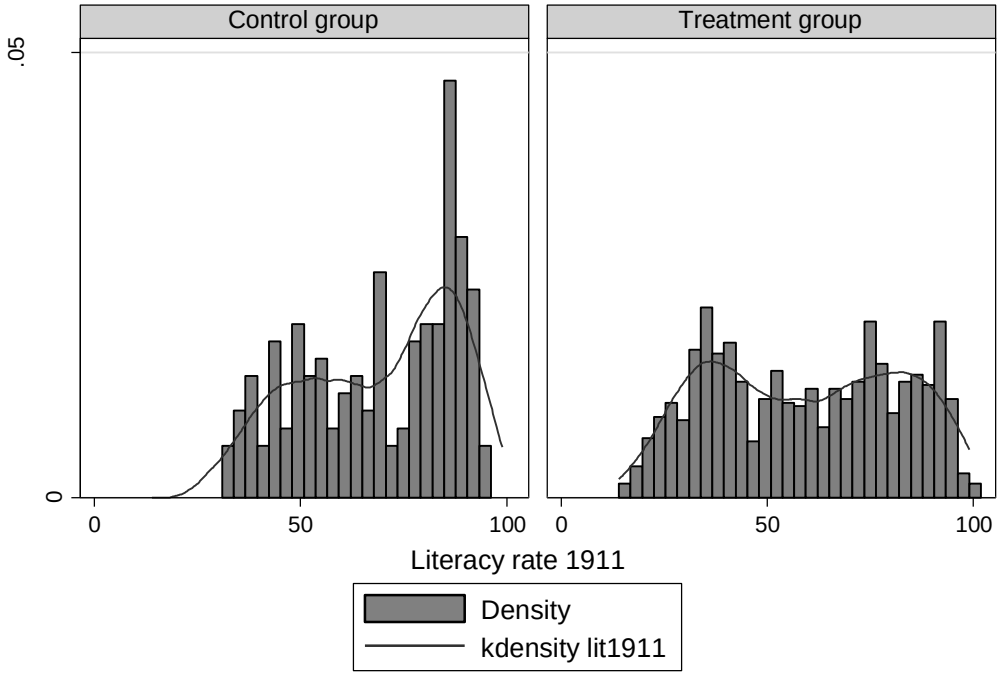
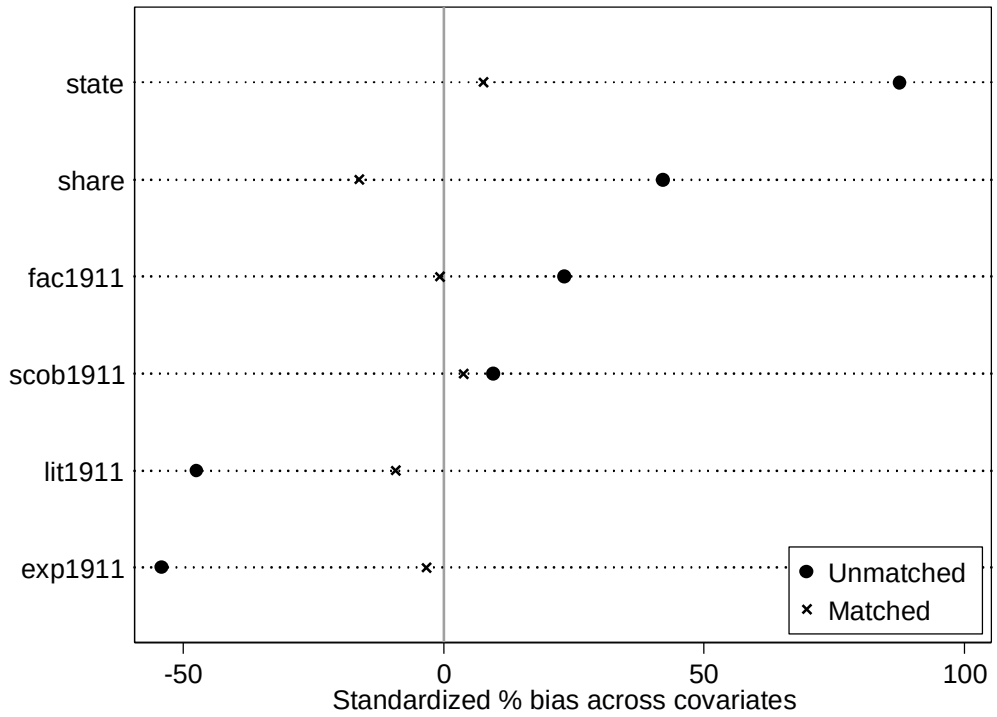
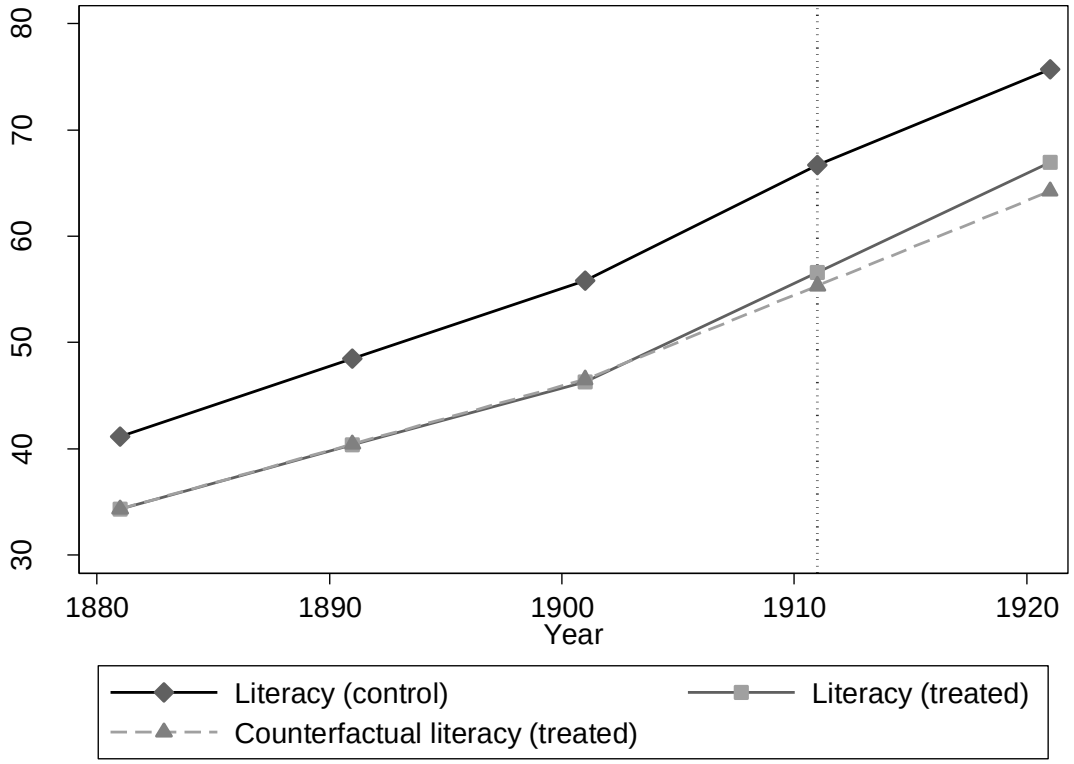


Figure 5 – Visual analysis of the reduction of the bias between treatment and control groups after matching (radius algorithm) with imposed common support.



Note: The variables are those used in the probit model reported in Table 4. All of them refer to 1911. The variable 'state' refers to state expenditure on education % of total; 'share' to the share of the municipal budget destined to education; 'fac1911' and 'scob1911' are the numbers of elective and compulsory schools, respectively, normalized to 1,000 inhabitants; 'lit1911' is the literacy rate; 'exp1911' is expenditure on education per capita.

Figure 6 – Literacy rate in the treatment and control groups, pseudo-DiD.



APPENDIX A – Procedure to implement the Propensity Score Matching (PSM)

Different steps must be followed to implement PSM.[68] We first estimate the propensity scores via a probit model. Heinrich, Maffioli and Vázquez suggest including the criteria that determine the selection into treatment, if they are available and if they do not undermine the common-support assumption. Furthermore, they argue that the researcher should model selection into treatment using several core variables that are deemed to be important for the probability of being sorted into treatment and the outcome, based on economic theory and experience. Finally, they recommend expanding the number of factors included in further specifications by adding blocks of variables and by testing whether the fitness of these models improves. This should lead to an optimal model that includes all important variables but excludes redundant factors.[69]

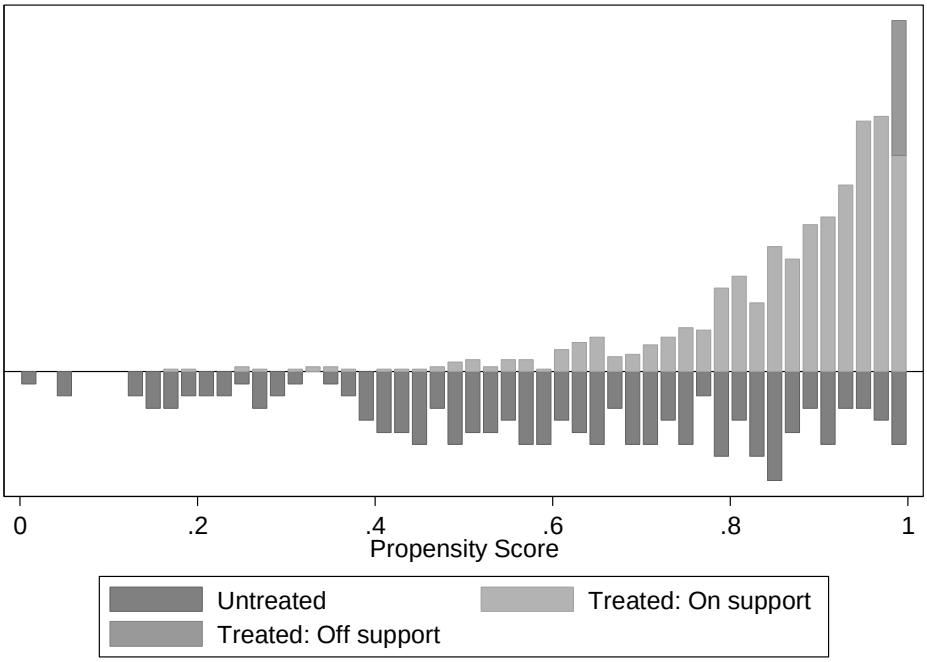
Second, once the propensity scores are estimated, a visual inspection can reveal whether there is common support by plotting the frequency of observations over the range of scores for the two groups separately (treated versus untreated observations). For the sake of matching, the condition of common support can also be imposed via the matching algorithm.

The third step is the attempt to find the best match in the control group for each of the treated observations. We use the radius algorithm for matching, since it allows each unit in the treatment group to be matched to several units in the control group insofar as their propensity scores are within a certain caliper (distance) from the propensity score of the treated unit. Since we have a large treatment group and a relatively small control group (mostly comprising district and provincial capitals), this is a sensible approach. It must be noted that using a strict caliper is likely to reduce the bias (difference) between the two groups. However, the reduced number of observations in the control group used to match treated units might increase the variance and hence reduce the statistical significance of the results. The conventional approach, at least in research that relies extensively on PSM, is to use a caliper equal to 0.20.[70] We decide to use the radius matching with a caliper equalling 0.05, which imposes a high degree of similarity for the matching to happen.

Table 4, presented and discussed in the main text, shows the probit model used to calculate the propensity scores. Figure A1 shows that, based on these propensity scores, there is wide common support.

[68] Caliendo and Kopeinig, 'Some Practical Guidance'.
[69] Heinrich, Maffioli and Vázquez, 'A Primer for Applying'.
[70] Austin, 'Optimal Caliper Widths'; Wang et al., 'Optimal Caliper'.

Figure A1 – Visual analysis of common support for estimated propensity scores



Note: The graph below the horizontal line refers to the density of untreated observations, while the graph above the line refers to the treated observations. The graph is based on the radius matching algorithm (caliper = 0.05).

The probit model identifies many municipalities in both the treatment and control groups that are characterized by very similar levels of development. Therefore, a matching algorithm can be used to reduce the potential bias from the endogenous assignment into centralized education. We match municipalities between the treatment and control groups using radius matching. In a nutshell, each treated municipality can be matched to several non-treated municipalities, which is an ideal solution given the small size of our control group. Indeed, the overlapping municipalities are skewed to the right in Figure A1, meaning that the matching observations for each treated municipality will be limited. When we impose common support, 55 municipalities are left out, which is equal to only 5 percent of the total observations in our 10-percent sample. As a robustness check on our matching procedure, we further restrict the common support by excluding, a priori, the municipalities with extreme propensity scores, i.e. scores lower than 0.1 and larger than 0.99.[71] Such thresholds were chosen to provide a balance between eliminating municipalities that are very unlikely to be matched and maintaining a relatively large sample size – 966 units, of which only 6 are now excluded by imposing common support, as expected. This more conservative matching procedure leads to more similar treatment and control groups (Table A1).

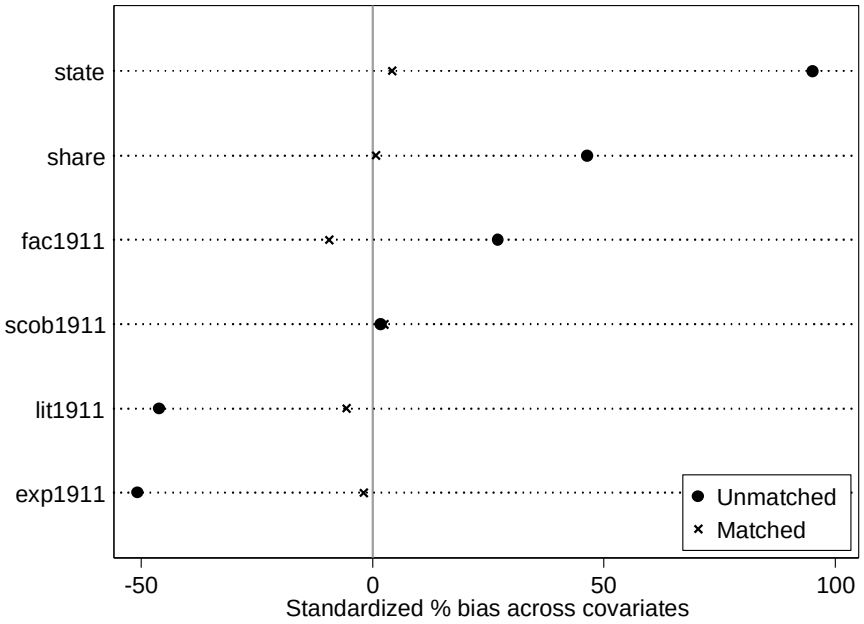
[71] See Semrad, 'Modern Secondary Education' for a similar approach.

Table A1 – Output of the matching procedure based on the radius algorithm excluding municipalities with extreme propensity scores (<0.1 and >0.99)

Variable	Sample	Mean of treated	Mean of control	T-test: p > |t|
Literacy rate 1911	U	59.07	68.43	0.00
	M	59.16	60.33	0.28
Compulsory schools x 1,000 inh.	U	1.65	1.64	0.85
	M	1.65	1.64	0.63
Elective schools x 1,000 inh.	U	0.11	0.06	0.01
	M	0.11	0.13	0.12
Education budget: state % total	U	0.18	0.12	0.00
	M	0.18	0.18	0.38
Education total % municipal budget	U	0.21	0.17	0.00
	M	0.21	0.21	0.89
Municipal expenditure education p.c.	U	2.82	4.26	0.00
	M	2.82	2.88	0.57
	Sample	**Pseudo R squared**	**p > Chi squared**	**Mean bias**
Summary statistics	U	0.24	0.00	44.60
	M	0.00	0.63	4.10

Notes: U refers to the sample before matching, and M refers to the sample based on matched observations. The last column shows the p-value based on the null hypothesis that the difference between the means of the treatment and control groups is zero.

Figure A2 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921, with radius matching excluding extreme propensity scores



Note: The variables are those used in the probit model reported in Table 4 in the main text. All of them refer to 1911. The variable 'state' refers to state expenditure on education % of total; 'share' to the share of the municipal budget destined to education; 'fac1911' and 'scob1911' are the numbers of elective and compulsory schools, respectively, normalized to 1,000 inhabitants; 'lit1911' is the literacy rate; 'exp1911' is expenditure on education per capita.

Table A2 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921, with radius matching excluding extreme propensity scores

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1921	Unmatched	2.25	1.42	0.83	0.18	4.65
Radius (caliper 0.05)	Matched	2.24	1.82	0.41	0.17	2.41

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APPENDIX B – Robustness checks

Table B1 – Expanded probit model for estimating propensity scores, Reform 1911-1921

Reform (mfx)	**(1)**
Literacy rate 1911	-0.0023***
	(0.001)
Compulsory schools x 1,000 inhabitants	0.0216
	(0.018)
Elective schools x 1,000 inhabitants	0.1547***
	(0.041)
Education budget: state % total	0.8207***
	(0.138)
Education total % municipal budget	0.5626***
	(0.104)
Municipal expenditure education p.c.	-0.0138***
	(0.004)
Altitude MSL	0.0000
	(0.000)
Electors (local) / population	0.2317
	(0.167)
HP x 1,000 inhabitants	0.0001
	(0.000)
Ind. workers x 1,000 inhabitants	-0.0004***
	(0.000)
Population density 1911	0.0000
	(0.001)
Access to sea < 5 km	-0.0802***
	(0.023)
Linear distance from nearest university	-0.0003*
	(0.000)
Observations	1,071

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The model should include variables influencing the treatment and the outcome that are measured before the assignment into treatment; hence, the model does not include WWI deaths per 1,000 inhabitants. However, as a further check, we perform a regression including this factor (not reported for the sake of brevity); the results do not change.

Table B2 – Average Treatment Effect on the Treated (ATT) after matching based on the expanded probit model, 1911-1921, with radius algorithm (caliper = 0.05)

Variable	**Sample**	**Treated**	**Controls**	**Difference**	**S.E.**	**T-stat**
Annual av. growth literacy 1911-1921	Unmatched	2.31	1.41	0.89	0.19	4.68
Radius (caliper 0.05)	Matched	2.27	1.82	0.45	0.18	2.49

Table B3 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921, with nearest-neighbourhood matching (with replacement = 10)

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1921	Unmatched	2.30	1.41	0.89	0.19	4.65
NN matching (w. replacement, 10)	Matched	2.28	1.82	0.46	0.18	2.61

Table B4 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model, 1911-1921, with kernel matching algorithm (standard bandwidth = 0.06)

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1921	Unmatched	2.30	1.41	0.89	0.19	4.65
Kernel matching	Matched	2.28	1.83	0.45	0.16	2.79

Table B5 – Average Treatment Effect on the Treated (ATT) after matching based on the core probit model for the period 1911-31: with radius matching excluding extreme propensity scores

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Annual av. growth literacy 1911-1931	Unmatched	1.92	1.11	0.81	0.14	5.64
Radius (caliper 0.05)	Matched	1.91	1.50	0.41	0.16	2.54