CES ifo Working Papers

12043
2025

July 2025

From Chalkboards to Steam Engines: Early Adoption of Compulsory Schooling, Innovation, and Industrialization

Francesco Cinnirella, Elona Harka

CES ifo

Imprint:

CESifo Working Papers

ISSN 2364-1428 (digital)

Publisher and distributor: Munich Society for the Promotion
of Economic Research - CESifo GmbH

Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740

Email office@cesifo.de
https://www.cesifo.org

Editor: Clemens Fuest

An electronic version of the paper may be downloaded free of charge
· from the CESifo website: www.ifo.de/en/cesifo/publications/cesifo-working-papers
· from the SSRN website: www.ssrn.com/index.cfm/en/cesifo/
· from the RePEc website: https://ideas.repec.org/s/ces/ceswps.html

From Chalkboards to Steam Engines: Early Adoption of Compulsory Schooling, Innovation, and Industrialization*

Francesco Cinnirella†

Elona Harka‡

July 30, 2025

Abstract

Empirical evidence on the historical role of Compulsory Schooling Laws (CSL) for the spread of mass education is mixed at best. This is also due to the difficulty of identifying exogenous variation in the application of CSL. We exploit an almost unique feature of a CSL in 1877 Italy which was gradually implemented across municipalities based on the teacher to population ratio. This criterion generates a sharp discontinuity which can be exploited to estimate the causal effect of the early implementation of CSL on economic outcomes. Estimates based on a regression discontinuity design show that CSL had a positive long-term effect on innovation and industrial employment. Consistent with the main objective of the reform, CSL had a positive effect on human capital by increasing enrollment rates in technical schools and, more in general, the literacy rate. The results are robust to a series of placebo, falsification and manipulation tests. This study provides important policy implications in favor of the early implementation of CSL to increase the average level of education which, in turns, brings about positive effects on innovation and industrialization.

Keywords: Education, Industrialization, Literacy, Patents, Liberal Italy

JEL Codes: N33, O14, O43, I25

*We thank Paolo Di Martino, Giuliana Freschi, Marco Manacorda, Vitantonio Mariella, Marco Molteni, Alessandro Nuvolari, Michelangelo Vasta as well as seminar participants at the Workshop on Institutions, Individual Behaviour and Economic Outcomes in Alghero, the DSE Winter Workshop at the University of Bergamo, the PhD Workshop at the University of Torino for comments. We are grateful to Alessandro Nuvolari and Michelangelo Vasta for sharing data on 1911 patents.

†Corresponding author. Department of Economics, University of Bergamo, CESifo, CEPR, and CAGE (E-mail: francesco.cinnirella@unibg.it).

‡Department of Economics, University of Bergamo (E-mail: elona.harka@unibg.it).

1 Introduction

Education is a crucial factor for modern economic growth. Several studies show that countries endowed with higher levels of education and skills fared economically better, especially during the Second Industrial Revolution when new technologies increased the demand for human capital (Goldin, 2014; Becker et al., 2011; Kelly et al., 2023; Galor and Moav, 2006). Yet, the relative importance for the Industrial Revolution of mass education as opposed to the education of a small knowledge elite is still debated in the literature.

Regarding mass education, the literature has identified different determinants thereof, which operate both through a supply and a demand channel: landownership inequality (Galor et al., 2009; Cinnirella and Hornung, 2016), the level of centralization of public spending (Lindert, 2004; Cappelli and Vasta, 2020), and cultural factors such as religion (Becker and Woessmann, 2009; Squicciarini, 2020). In this context, also the enactment of compulsory schooling laws (CSLs) has been studied as a possible important determinant of mass education. However, CSLs have often been deemed as unimportant to increase school attendance because of weak enforcement (Landes and Solmon, 1972).

More recently, thanks also to the availability of individual full count U.S. Census micro-data, scholars have tried to reassess the importance of CSLs. Yet, evidence in favor of a positive role of CSLs is weak and, in many cases, laws on mandatory schooling are found to have an impact only in conjunction with child-labor laws (Feigenbaum and Russo, 2020; Lleras-Muney and Shertzer, 2015; Lleras-Muney, 2002; Clay et al., 2012; Margo and Finegan, 1996). Furthermore, the empirical evidence is overwhelmingly focused on the U.S. as it offers the opportunity to exploit a staggered adoption of CSLs.

In this paper, we investigate the effect of the adoption of CSL on innovation and industrialization in the context of liberal Italy which offers a unique setting. In fact, a CSL was passed in 1877 but it was implemented gradually across municipalities, with an objective rule based on population size and the relative number of teachers.[1] This rule generated a discontinuity which can be exploited to assess whether the early implementation of CSL had an impact on the economy. Indeed, estimates based on a sharp Regression Discontinuity Design (RDD) show that municipalities which implemented earlier CSL have significantly more innovation about 30 years later. Consistently, we also find that the early adoption of CSL brought about a higher employment rate in industry, mainly in the metal-working and service sector. In fact, these sectors have been shown to have increased the demand for skills during the Second Industrial Revolution (Becker et al., 2011).

[1] The Casati Law formally introduced compulsory elementary schooling across Italy in 1861. However, it was never effectively enforced, as we will explain later in more detail.

Obviously, the main purpose of CSLs was not to increase directly innovation but to raise the level of education of the society. In the Italian case, this was even more urgent given the dire situation in terms of literacy which, at the time of unification in 1861, was as low as 41% for males and 22% for females. Unfortunately, due to the absence of data on primary school enrollment rates at municipality level, we cannot study the immediate impact of CSL on education. However, consistent with the main purpose of the Law, we find that the early adoption of CSL had a large and positive effect on literacy rates measured in 1911, ca. 35 years after the passage of the Law. Furthermore, we find that early adoption of CSL affected positively also the enrollment to secondary technical schools in 1884-87. The latter result points to the importance of secondary education for later stages of development and the complementarity between primary and secondary education (Goldin and Katz, 2010; Goldin, 2014). These positive effects on education constitute also the potential mechanism linking CSL and innovation.

Identification of the causal effect is based on the notion that the rule applied to implement CSL created a discontinuity around the cutoff which generated a quasi-random assignment of the CSL. In particular, the Coppino Law required municipalities with a population of less than 5,000 to implement compulsory schooling only if they had one teacher per 1,000 inhabitants; for municipalities with a population between 5,000 and 20,000, the threshold was one teacher per 1,200 inhabitants; for municipalities larger than 20,000 inhabitants, the cutoff was set at one teacher per 1,500 inhabitants. The Law was officially planned to take effect at the beginning of the 1877/78 school-year. In municipalities that did not meet the teachers per capita criteria, compulsory schooling was implemented successively and gradually as the necessary infrastructure (mainly teachers) became available. Therefore, our results should be interpreted in terms of an earlier vs. a later adoption of compulsory schooling.

In order to substantiate a causal interpretation of the results, we present several tests. First, we show that geographic characteristics, such as altitude, temperature, and precipitation, are smooth and continuous around the cutoff. Importantly, we leverage information on educational variables collected for the school-year 1862/63 (i.e. 15 years before the application of the CSL) to show that schools per capita, municipal expenditure per student, and the student-teacher ratio are all continuous around the cutoff. This implies that the criteria adopted for the Coppino Law in 1877 did not strategically target municipalities with an advantage (or disadvantage) in terms of primary education.

To further strengthen this important point, we use as a placebo the criteria initially proposed in a previous draft of the law which were more restrictive in terms of number of teachers per capita. By using these criteria, initially thought but never passed, our RDD approach shows zero effects.

It is also important to note that the time between the discussion of the first draft of the Law (December 1876) and the official passage of the CSL (July 1877) was too short to reasonably expect an anticipation by municipalities. We argue that the institutional framework and the timing of the legislative process make sorting around the cutoff unlikely, and we present different formal tests and robustness checks consistent with no manipulation of the assignment rule.

This paper contributes to several strands of literature. First, we directly contribute to the literature on the effects of CSLs which, so far, has mainly focused on the U.S. Landes and Solmon (1972) analyze the impact of CSL on school attendance in U.S. from 1880 to 1910 and find no effect on enrollment. Angrist and Krueger (1991) use a cohort approach and compare attendance rates by state, cohort, and quarter of birth between 1960-1980 and find a positive effect of CSL on schooling. Margo and Finegan (1996) exploits a feature of the 1900 Federal Census and find, instead, an insignificant impact of CSL on school attendance. They do find, however, significant positive effects in combination with child labor legislation. Lleras-Muney and Shertzer (2015) study the impact of legislation making English the sole language of instruction in schools and the impact of compulsory schooling laws. In particular, they investigate whether these laws were effective at increasing English fluency, literacy, and overall educational levels of immigrants between 1910-1930. They find that CSLs raised immigrants' enrollment, especially for children born abroad. Feigenbaum and Russo (2020) study the effect of child labor and CSLs from 1880 to 1930 on child labor rates. They link complete count census data with state-level legislation on working age minima and compulsory years of schooling. Using a triple differences-in-differences design, they find that the legislation did bind white urban child workers. Bandiera et al. (2018) study why America did introduce CSL in the first place. They show that CSLs were introduced in U.S. as a nation-building device to instill civic values to a culturally heterogeneous society during the Age of Mass Migration. The large-scale introduction of kindergarten education in U.S. cities during the late nineteenth-century was part of such reforms (Ager and Cinnirella, 2021; Ager et al., 2025). Clay et al. (2012) use retrospective data from the 1940 Census of Population to study the effect of compulsory attendance laws passed in different states in U.S. between 1880-1920. They find that the introduction of CSL had positive effects on schooling in states that passed laws after 1880.[2]

To our knowledge, this is the first paper which studies systematically the effect of a compulsory schooling law in a European country in historical perspective.[3] We believe that the unique features

[2] There is also a literature that studies the second wave of CSL in U.S., focusing on high school attendance (Goldin and Katz, 2008; Lleras-Muney, 2002).

[3] There is a vast literature which exploits post-WWII changes in CSLs as an instrument for years of education.

of the implementation of the Coppino Law in Italy allow to exploit a quasi-random variation across municipalities within a country. Differently from previous literature, we find large effects on innovation, industrial employment, literacy and enrollment in secondary education. It is also important to note that we can discard any potential reinforcing effect of child-labor laws as these were passed ca. 10 years later in 1886.

Our paper contributes also to the literature on the role of education in modern economic growth. There is increasing empirical evidence about the importance of a knowledge elite for the transition to modern economic growth (Mokyr, 2009; Meisenzahl and Mokyr, 2012; Squicciarini and Voigtländer, 2015; Cinnirella et al., 2025). However, there is also evidence that mass education and formal education might have played an important role. Becker et al. (2011) find that formal education played a pivotal role in both phases of the Industrial Revolution in Prussia. Cinnirella and Streb (2017) show that literacy was positively related to patenting activity during the Second Industrial Revolution in Prussia. Madsen and Murtin (2017) find that education has been the most important driver of economic growth in the case of Britain, both before and after the First Industrial Revolution. This result is in part consistent with the findings of de Pleijt (2016) who finds evidence of a remarkable growth in schooling during the sixteenth and seventeenth century, yet followed by a decline after ca. 1720. Her interpretation is that education was beneficial only to pre-industrial economic growth. Consistent with this interpretation, de Pleijt et al. (2019) find that technological change in England had a positive impact on the formation of working skills but had a negative effect on the formation of primary education. More recently, de Pleijt and Frankema (2025) show a strong relationship between educational attainment and local development in 20th century Southeast Asia.

Finally, we contribute also to the literature on the determinants of education in the context of Italy. Previous research has strongly focused on political economy channels. Consistent with evidence for other countries, Mariella (2022) finds evidence of an adverse effect of land inequality on literacy rates in Italy, 1871-1921. Bozzano et al. (2023) investigate the role of pre-unitary institutions. They argue that the different models of schooling provision—universal in the North and elitist in the South—affected primary education up to WWI. Cappelli (2016) investigates the role of enfranchisement in schooling. He finds that differences in electoral franchise across municipalities do not explain regional inequalities in schooling in the long run. Fiscal capacity seems to be one of the most significant determinant of mass education. Indeed, Cappelli and Vasta (2020) show that centralization of public school accelerated human capital accumulation after 1911.

2 Institutional framework

2.1 Compulsory Schooling Law (CSL)

Before Italy's unification in 1861, most parts of the peninsula lacked a legislation governing compulsory education. Indeed, during the initial decades of the nineteenth century, only the Kingdom of Lombardy-Venetia had regulations mandating school attendance. In accordance with the Austrian regulations of 1818, children between the ages of 6 and 12 were required to attend public primary schools (Bozzano et al., 2023).

The first comprehensive compulsory schooling law was established in 1859 in the Kingdom of Sardinia with the enactment of the Casati Law. This law was subsequently extended to the entire Italian territory following unification. Elementary schooling, which lasted four years, was divided in two levels: lower and upper elementary school, each lasting two years. Every municipality was required to establish a lower primary school, while only those with an agglomerated population exceeding 4,000 inhabitants were required to provide upper primary schooling. In the framework of the Casati Law, municipalities played a central role in the supply of elementary education. They were responsible for appointing (and confirming) teachers[4] and for allocating tax revenues to cover teachers' salaries and other expenses necessary to establish and maintain primary schools. When municipalities lacked resources, the State provided transfers to alleviate the financial burden of elementary education. Teachers were typically appointed for three-year terms and municipalities could confirm teachers for another fixed term or, at their discretion, appoint them permanently.[5] To be eligible for appointment, candidates had to hold a teaching certificate obtained via examination,[6] be at least 18 years old (17 for female teachers), and present a certificate of good moral standing issued by the mayor of their municipality of residence. Although pupils were obliged to attend lower primary schools, no sanctions were in place for non-compliance. We should note that the municipality was responsible also for the enforcement of the CSL. Therefore, as no legal sanctions were established until 1877, the absence of enforcement mechanisms significantly hindered school attendance (Cappelli and Vasta, 2020).

The legislation on compulsory schooling underwent a significant change in July 1877 with the enactment of the Coppino Law, which extended compulsory education from two to three years. In practice, this reform established the obligation for children to attend lower primary school between the ages of 6 and 9. The obligation applied only to the lower elementary cycle (the first and second

[4] Appointments required approval by the Provincial School Council ("Consiglio provinciale per le scuole"), a supervisory body chaired by a superintendent known as "Regio Provveditore".
[5] Appointments were automatically renewed unless the teacher was formally dismissed with at least six months notice.
[6] Candidates were trained in dedicated schools. For lower elementary teachers, this training took place in "scuole magistrali".

grades), which was later reorganized into three grades in 1888.[7] The curriculum for lower primary education included instruction in civics, reading, writing, and the fundamental principles of the Italian language. Furthermore, the curriculum included instruction in arithmetic and the metric system.

The Coppino Law was the first to introduce sanctions for non-compliers. The mayor of the municipalities had to compile annual lists of school-aged children and cross-check them with school enrollment records in order to identify those who were not attending. Parents who failed to comply would first receive a warning, followed by fines starting at 50 cents, which could increase progressively to a maximum of 10 lire for persistent non-attendance. Additionally, non-compliant parents could also be denied access to public subsidies and other benefits, such as government stipends and the right to carry weapons.

Importantly, the Coppino Law included provisions for the gradual enforcement of compulsory schooling based on (*i*) the population size of the municipality and (*ii*) the number of primary-school teachers. The first draft law which was presented to the Parliament in December 1876 stipulated that, two months after its enactment, municipalities with a population of less than 5,000 inhabitants had to implement compulsory schooling if they had at least one teacher per 800 inhabitants. For municipalities with a population ranging from 5,000 to 20,000 inhabitants, the threshold was set at one teacher per 1,000 inhabitants. In larger municipalities, the cutoff was set at one teacher per 1,200 inhabitants. During the parliamentary process, however, the commission proposed changes to these thresholds and in the final version passed in 1877 the rules were made less restrictive.[8] Specifically, municipalities with a population of less than 5,000 were obliged to implement compulsory schooling if they had one teacher for every 1,000 inhabitants. For municipalities with a population between 5,000 and 20,000, the threshold was set at one teacher for 1,200 inhabitants. Finally, for larger municipalities ($> 20,000$), the cutoff was set at one teacher for 1,500 inhabitants. The law was officially scheduled to take effect at the beginning of the 1877-78 school-year.

Importantly, in municipalities that did not meet these conditions, compulsory schooling was implemented successively as the necessary infrastructure became available. The School Council was required to annually review and classify municipalities based on their compliance with these requirements, publishing the results. Municipalities failing to establish the required schools were subject to intervention from the provincial deputation, which could mandate budget adjustments to ensure compliance, reallocating discretionary funds if necessary. State subsidies were primarily to be allocated to municipalities in which the Law's application was suspended, for the purpose of increasing the number of schools, improving facilities, and providing the necessary equipment and teachers.

[7] In fact, only the population residing within a two-kilometer radius of a municipal school was subject to the obligation.
[8] A detailed timeline of the Coppino is included in Appendix 7.

To address teacher shortages, the Ministry committed to opening teacher training schools ("scuole magistrali") where needed, particularly in provincial capitals, district capitals, or other significant municipalities.

According to the official report on the application of the Coppino Law, 81% of the municipalities could already enforce mandatory education, while others were in the process of hiring teachers to meet the Law's requirements.[9] There were notable discrepancies in the availability of teachers across Italian regions. In regions like Piedmont and Lombardy only a small fraction of the municipalities did not meet the criteria to enforce the Law.[10] However, the availability of teachers was rather scarce in other regions of Italy. In Central Italy, approximately 16% of municipalities in Lazio and as many as 42% in Tuscany were unable to meet the requirements. In Southern Italy and the Islands, the situation was even more pronounced, with 69% of municipalities in Sicily and 77% in Basilicata unable to implement compulsory schooling.

In fact, it took several years to implement the Coppino Law across all Italian municipalities. For instance, in the school-year 1882/83, there were still 601 municipalities (out of more than 8,000) where compulsory schooling was only enforced in certain areas of the municipality, while in 122 municipalities, the teacher-to-population ratio remained below the threshold required for enforcing mandatory education. By the following school year, the number of municipalities in which the Law could be only partially enforced decreased to 306, and in 90 municipalities it was not yet possible to implement the Law. By the 1886/87 school year, only 79 municipalities had yet to meet the criteria, and by 1891/92, compulsory schooling had been fully implemented in all Italian municipalities.[11]

Even though compulsory schooling was in force in all municipalities by the 1890s, marked differences in enrollment rates across Italian regions persisted. In the 1893/94 school year, there were 2,292,005 children between the ages 6-9, of whom 1,505,169 (65%) were enrolled in lower primary schools. In regions like Piedmont and Lombardy, enrollment rates were 91% and 89%, respectively. In Center Italy, attendance rates ranged between 52% and 72%, while in the South and Islands they ranged between 35% and 53%. These statistics seem to suggest that compliance with the Coppino Law and student enrollment were closely linked.

[9] See Section 3 for a detailed description of the official report on the implementation of the Coppino Law across Italian municipalities.

[10] In Piedmont only the 0,7% of municipalities and in Lombardy around 3.6%.

[11] These statistics are drawn from a series of publications titled "Statistics on primary schooling" from the Ministry of Agriculture, Industry and Trade.

2.2 Patenting system and innovative activity

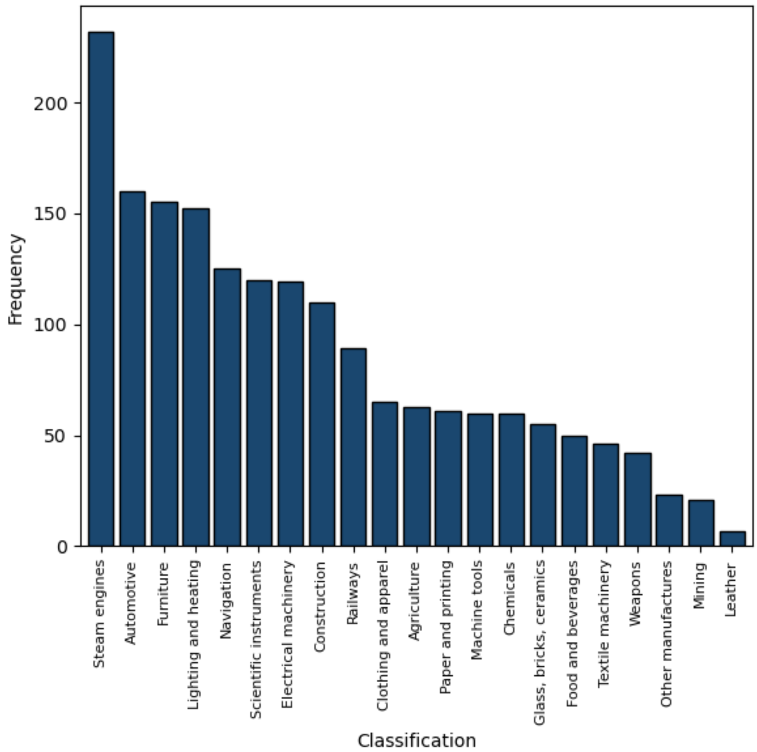
After Italy's unification in 1861, the Kingdom of Sardinia's patent system, which was based on the 1855 Piedmontese law, was extended to the entire country. Under Italian law, patents (known as "privativa industriale") granted inventors the exclusive rights to implement and benefit from their industrial inventions for a period ranging from 1 to a maximum of 15 years. A patent initially granted for less than 15 years could be extended ("prolungamenti") by one or more years, provided the total duration did not exceed 15 years. Additionally, inventors were allowed to request a supplementary certificate ("attestato completivo") for any modifications they made to their original invention.

Under this system, an invention or discovery qualified as industrial if it was directly related to: (1) an industrial product, (2) an instrument, machine, device, artifact or any mechanical arrangement, (3) a process or method of industrial production, (4) an engine or the industrial application of an already known motor power, or (5) the technical application of a scientific principle, provided it produced immediate industrial results. Specific categories of inventions were excluded from patent protection, including those pertaining to industries considered illicit, unethical or against public safety, those without the aim of producing tangible products, purely theoretical discoveries, and medicines. An invention or discovery was deemed novel if it was unknown. If previously known, it could still be deemed as novel if the specific details required for its implementation were unknown. However, the Italian patent system followed the French model and functioned on a registration basis, meaning that patents were granted without examining the novelty of the inventions (Nuvolari and Vasta, 2019).

Applicants—whether nationals or foreigners, individuals, corporations, or any other legal entities—had to file their request with the Ministry of Agriculture, Industry and Trade through local Prefectures or Subprefectures. Shortly after unification, independent inventors dominated patent filings. Although their share declined over time, they still represented a substantial majority, with about 86% of patents filed by independent inventors in 1911 (Nuvolari and Vasta, 2014).

Patenting activity in Italy was highly concentrated in the "industrial triangle" of Genoa, Milan, and Turin (Nuvolari and Vasta, 2017). Although independent inventors played a significant role in the total number of patents, their contributions were generally of lower quality compared to those of firms and foreign residents (Nuvolari and Vasta, 2014). In the early post-unification years, Italian inventors primarily focused on traditional sectors and their inventions displayed low levels of technological sophistication (Buonanno et al., 2024), whereas foreign patentees were more engaged in modern sectors. However, an analysis of the frequency of patents granted to Italian residents by technological class (Figure 1) shows that the top categories by 1911 were steam engines, automotive, furniture, lighting and heating, and navigation.

Figure 1: Patents in 1911 — Frequency of patents by technological class



Note: The figure shows the distribution of patents by technological class granted to Italian residents in 1911. Source: "Bollettino della proprietà intellettuale."

3 Data

We draw data on the application of the compulsory schooling law from an official report published in 1878 by the Ministry of Public Education.[12] This report provides details on the status of public elementary school teachers across Italian municipalities and specifies in which municipalities the compulsory schooling law was immediately enacted and in which it was not enacted at the beginning of the 1877/78 school year.

Contemporary data collection began following a ministerial directive issued on April 30, 1877, addressed to the Superintendents of Education, in which the minister instructed local authorities to collect comprehensive information on the state of education in each municipality.[13] As shown in Figure A.1, the local authorities were required to report the municipal population according to the latest census (i.e. the national census of 1871), the current number of teachers for lower and upper elementary levels, the number of teachers to be appointed, and the number of schools that needed to be established to fulfill the new Law's requirements. This data collection was explicitly aimed at

[12] "Sull'obbligo della istruzione elementare nel Regno d'Italia: attuazione della legge 15 luglio 1877".
[13] "Circolare N. 517 - Roma 30 aprile 1877."

assessing the existing state of schools across municipalities and determining where the new Law could be enforced.

Following the enactment of the Law, another ministerial directive was addressed to the prefects, in their role as presidents of the Provincial School Councils, instructing them to ensure that the Law would be enforced in eligible municipalities starting from the upcoming school year.[14] Superintendents of Education were required to submit, by the end of August, a classification of municipalities eligible for implementation, to be approved by the school council. Once the council's decision was made, each municipality had to be officially notified so that the mayor could, by the end of September, prepare a list of school-age children.

Figure A.2 presents an example of the final data included in the publication. We have information on municipal population, the number of teachers for lower (and upper) primary schools and, crucially, the classification of municipalities based on whether compulsory schooling was to be enforced or not. According to this classification, compulsory schooling was enforced in 6,742 out of 8,301 Italian municipalities (81%). As clarified in the introductory notes to the publication, this classification refers to municipalities considered "able to implement compulsory education without increasing the current number of teachers." A further 400 municipalities were reported to be accelerating preparations to implement the Law. Note that these municipalities were not included among those officially classified as implementing the Law.[15]

Our analysis of the CSL focuses on municipalities with 5,000 inhabitants and more, that is, those with a threshold of one teacher per 1,200 inhabitants (5,000-20,000) and those with one teacher per 1,500 inhabitants (20,000-). We take this approach for various reasons. First, since we are interested in assessing whether CSL had an impact on innovation and industrial development, it makes sense to focus on larger municipalities which were the driver of economic growth. Furthermore, since we are investigating the effect on patenting activity, including smaller municipalities would imply having a large number of zeros in terms of patents. Second, to understand the mechanism behind the relationship between CSL and innovation, we will analyze enrollment in technical schools. These schools were mainly in provincial capitals and, when these municipalities did not agree to bear the costs, the State could grant financial support to another municipality within the same province, provided that it had fulfilled the requirements for primary education.[16] Because of these

[14] "Circolare N. 526 – Roma, 3 agosto 1877".

[15] Municipalities are listed according to the administrative boundaries of the year the CSL was passed in 1877. For municipalities with changes in administrative boundaries, the report reconstructs the 1871 population according to the new boundaries. However, in some cases, the publication provides population data according to the 1871 boundaries. For consistency, in these cases, we recalculated the population based on the boundaries in 1877.

[16] Secondary education was also subject to specific requirements. For instance, lower secondary schools (Ginnasi) were to be established in provincial capitals or district capitals, while at least one upper secondary school (Liceo) was

considerations, we decided to focus on larger municipalities (5,000+) as they are the ones that could have ultimately contributed to technological advancement and economic growth. However, as robustness check, we show that our results hold also when using all municipalities.

Thus, our regression sample consists of 542 municipalities in which compulsory schooling was enforced (treatment group) and 555 which did not meet the criteria to enforce it (control group).[17] Figure 2 shows the geographic distribution of the municipalities in our sample. The picture is consistent with the pattern outlined in Section 2, with the majority of the control municipalities (no enforcement of the CSL) concentrated in the Center and in the South.

The first draft of the law also contained information on the number of teachers in elementary schools across Italian municipalities in 1875. Specifically, an appendix provided information on municipal population and the total number of teachers, categorized by lower and upper elementary classes. The appendix further distinguished between municipalities with a school for every 600, 700, or 800 inhabitants, and those lacking a school for every 800 inhabitants. As explicitly stated, the purpose of the appendix was to determine which municipalities could implement compulsory schooling. We use the information on the number of teachers from the appendix and the thresholds specified in the draft law to perform a placebo test. With these criteria, we have 371 treated and 722 control municipalities.[18]

Our primary measure of innovation is the number of patents per 10,000 inhabitants granted in the period 1908-1912.[19] The main source of information on patenting activity is a series of publications titled "Bollettino della proprietà intellettuale" published by the Ministry of Agriculture, Industry and Trade. These publications include, among others, information on the inventors' name, municipality of residence, duration of the patent, title and technological class. We digitized the patents issued between 1908–1912 and assigned each patent to the municipality of residence of the inventor.[20]

to be established per province. Other municipalities that were not required to establish lower secondary schools were still allowed to do it, but only after demonstrating to the Ministry of Public Education that they had fulfilled their obligations regarding primary schools. Likewise, municipalities with an existing lower secondary school could open institutions offering upper secondary education, but only after meeting the requirement of establishing technical schools. In addition, Technical Institutes could be established in municipalities which needed a skilled labor force for industrial and/or commercial activities.

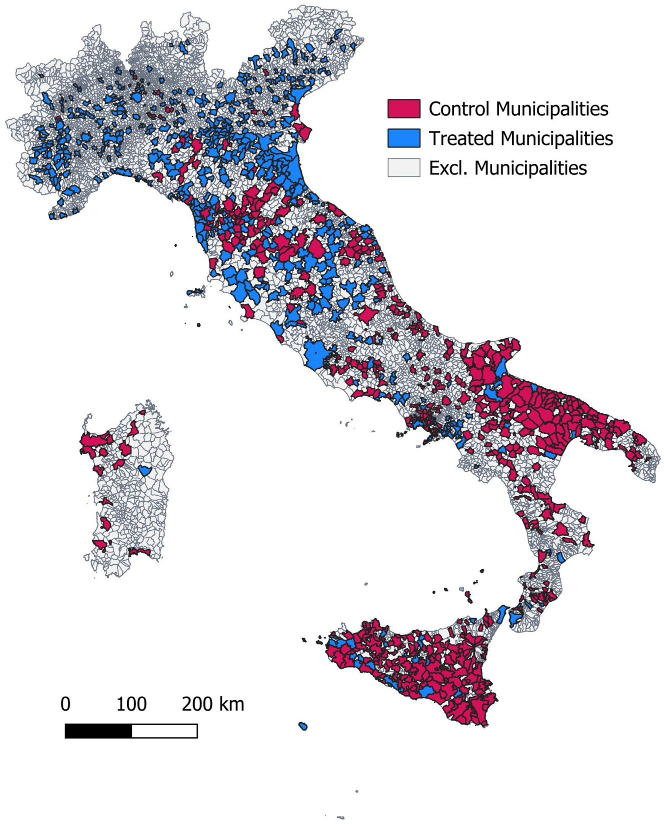
[17] In 16 municipalities, our calculation of treatment status does not match the classification in the official publication. Fifteen municipalities are below the threshold (teachers per capita range between 0.70 and 0.998) but are listed as treated. One municipality exceeds the threshold (1.12) but is listed as non-treated. In our robustness checks, we show that excluding these cases or reclassifying some observations (e.g. assigning treated status to those above 0.99) does not affect the results.

[18] The publication contains some errors on the municipal population. Therefore, we use the population used for the main analysis.

[19] We standardize the number of patents by the municipal population as reported in the 1911 census. We reconstructed patent data according to the administrative boundaries in effect when the Law was passed in 1877.

[20] We exclude extensions (*prolungamenti*) and supplementary certificates (*completive*). Following common practice in the literature, we assigned the municipality of residence of the first inventor when multiple inventors are reported.

Figure 2: Geographic distribution of municipalities with and without CSL enforcement



Note: The map shows the geographic distribution of municipalities where compulsory schooling was enforced (in blue) and where it was not enforced (in red), based on a strict application of the rule. Source: "Sull'obbligo della istruzione elementare nel Regno d'Italia: attuazione della legge 15 luglio 1877".

Patents, arguably the best measure for innovation in historical context, can be problematic as innovations were not always patented and secrecy constituted in some cases a valid alternative (Moser, 2005). As an alternative measure for innovation, we use the number of trademarks for 10,000 inhabitants. Under the Italian 1868 law, a trademark (referred to as "marchio or segno distintivo") granted exclusive rights to distinguish industrial products, commercial goods, or animals of a specific breed. The characteristic signature of the producer, merchant, or owner—engraved, sealed, applied by any durable method, or even handwritten—could also constitute a trademark or distinctive sign. The source for these data is the digitized collection of the records of the Italian Office of Patents and Trademarks kept at the National Central Archive[21], and in our empirical strategy we use trademarks issued between 1900-1912.[22] The trademark data include several key variables,

[21] Available at: `http://dati.acs.beniculturali.it/mm/local/`

[22] The number of trademarks is normalized by using the population from the 1901 census.

such as the registration date, filing date, type of trademark, holder, and municipality of residence. In our sample, the correlation coefficient between the number of patents and trademarks per 10,000 inhabitants is 0.595, suggesting some complementarity between the two measures.[23]

For data on employment in industry, we rely on the census of factories and industrial firms conducted in 1911.[24] The industrial census contains information on the number of firms, number of people employed and the motive power in dynamic horsepower. The number of surveyed firms includes those that operated in a given location with no fewer than two people. The number of employed people includes workers, family members of the owners employed in the firm (whether paid or not), along with managerial, supervisory, technical, and administrative staff. The census classified firms into seven categories: agriculture-related industries, textiles, metalworking, extractive industries, construction, chemicals, and services.

To investigate the mechanisms through which compulsory schooling may have affected innovation and industrialization, we use data on literacy rates and enrollment in technical schools. For literacy, we rely on the Population Census of 1911, which was the first census to include literacy rates for all Italian municipalities. Data on enrollment in technical schools are instead drawn from the series titled "Statistica dell'istruzione secondaria e superiore", published by the Ministry of Agriculture, Industry, and Trade. Our measure for education is the number of students per 10,000 inhabitants enrolled in technical schools during the school years 1884/85, 1885/86, and 1886/87.[25]

Descriptive statistics of the discussed variables for treatment and control municipalities are presented in Table 1. In panel A, we provide summary statistics for our innovation variables. In municipalities without compulsory schooling in 1877, the average number of patents per 10,000 inhabitants in 1908-12 is 0.39, while in municipalities with compulsory schooling the average is significantly higher at 1.4. A similar pattern is observed for trademarks, where municipalities without compulsory schooling have an average of 0.24 trademarks for 10,000 inhabitants, compared to 0.71 in municipalities with compulsory schooling.

In panel B, we provide descriptive statistics for industrial employment normalized per 1,000 inhabitants.[26] We note that in municipalities without CSL in 1877, the total number of workers employed in the industrial sector in 1911 is 40 per 1,000 inhabitants, while in municipalities with

[23] This is based on the normalization adopted in the main analysis. The correlation coefficient is similar when using 1911 population to normalize both variables (0.568) or 1901 population (0.594).

[24] "Censimento degli opifici e delle imprese industriali al 10 giugno 1911, Volume 1".

[25] For this variable, we calculated the average of enrollment in technical schools over the three school years and normalized it using the 1881 population data.

[26] The sample in Panel B is slightly smaller due to data availability. The industrial census reports data on 7,166 municipalities (86% of the total). The remaining 1,157 municipalities did not provide any information and are considered as missing.

CSL the average is significantly higher at 73. In fact, with the exception of extractive industries, the average number of workers employed in industry is higher in municipalities with CSL across all sectors.

Finally, Panel C reports the summary statistics of our educational variables. Municipalities that enforced CSL in 1877 exhibit higher literacy rates (around 68%) in 1911 compared to those without CSL (around 43%). We observe a similar pattern for enrollment in technical schools. Averaging the three years of technical schools, we can see that municipalities with CSL have higher enrollment in technical schools (39 students per 10,000 inhabitants) compared to municipalities without CSL (25 students per 10,000 inhabitants).

Table 1: Descriptive statistics of outcomes by CSL enforcement

	Munic. without CSL			Munic. with CSL		
Panel A: Innovation	Mean	Std. dev.	Obs.	Mean	Std. dev.	Obs.
Patents per 10,000 inhab. (1908-12)	0.390	0.920	555	1.399	3.274	542
Trademarks per 10,000 inhab. (1900-12)	0.239	0.876	555	0.711	2.188	542
Panel B: Industry (1911)						
Agriculture	18.20	16.80	548	20.78	16.46	537
Textiles	6.800	17.80	548	22.86	45.46	537
Metals	4.316	8.581	548	11.28	28.14	537
Extractive	3.348	18.10	548	2.321	9.799	537
Construction	5.077	13.65	548	9.905	13.40	537
Chemicals	1.475	4.852	548	3.417	9.955	537
Services	0.768	1.424	548	2.086	4.853	537
Industry (total)	39.99	38.60	548	72.65	73.82	537
Panel C: Education						
Literacy rate (1911)	42.82	14.14	555	67.68	17.26	541
Enrollment technical schools (1884-87)	25.33	13.25	128	39.40	18.64	163

Note: Panel A shows the descriptive statistics for our measures of innovation. The data source for patents is the "Bollettino della proprietà intellettuale", 1908–1912. The data source for trademarks is the digitized collection of the records of the Italian Office of Patents and Trademarks kept at the National Central Archive. Panel B shows the descriptive statistics for industrial employment in selected sectors, drawn from the 1911 census of factories and industrial firms. Panel C shows our measures for educational attainment. Literacy rates are from the Population Census of 1911. For technical schools, the data source is "Statistica dell'istruzione superiore e secondaria" for the school years 1884/85, 1885/86, and 1886/87.

In our empirical analysis, we will account for pre-existing conditions in several domains. In terms of education, we exploit data from primary schooling statistics which include information on the number of primary schools, students, teachers, and municipal expenditures for the school-year 1862/63. Based on this information, we construct three measures of pre-existing conditions in education, namely, the number of schools per 1,000 inhabitants, municipal expenditure per student and student-teacher ratio.[27] We include also several geographical control variables, such as altitude drawn from the

[27] This is a publication from the Ministry of Agriculture, Industry, and Trade titled "Istruzione elementare pubblica per comuni, anno scolastico 1862-1863." Note that for the region of Veneto and the province of Rome, which joined the Kingdom of Italy, respectively, in 1866 and 1871, there are no data.

National Institute of Statistics (Istat), temperature and precipitation from Fick and Hijmans (2017). Additionally, we also account for pre-unification states fixed effects according to the boundaries of 1850. The source for these data is Harka et al. (2023).[28] Descriptive statistics of these variables are reported in Panel B of Table A1.

Other variables used in the analysis include population growth between 1861 and 1871 (based on Population Census data), the number of teachers per capita in the 1862/63 school year, and employment in the industry drawn from a publication from the Ministry of Agriculture, Industry, and Trade titled "Notizie statistiche sopra alcune industrie". Data collection began in 1876, and the volume was published in 1878. Importantly, we also constructed data on patenting activity *prior* to the passage of the CSL. In particular, we focus on the stock of patents granted to Italian inventors from 1863 to 1875. Finally, we also collected data on patents granted immediately after the reform (1878–1883), to use as a placebo outcome.[29] Descriptive statistics of this last set of variables are reported in Panel C of Table A1.

4 Empirical strategy and results

We study whether education affects innovative activity by comparing municipalities that implemented earlier compulsory schooling with those that did not meet the required threshold for its implementation in 1877. The institutional framework described in Section 2 allows us to implement a one dimensional regression discontinuity design (RDD) where assignment to the treatment is based on the number of teachers per capita, T_m, calculated as follows:

$$T_m = \frac{Teachers_m}{Population_m} \cdot \lambda \tag{1}$$

where $Teachers_m$ denotes the number of teachers in lower primary schools in municipality m and $Population_m$ represents the population in the same municipality in 1871. The parameter λ takes on different values based on the population size of the municipalities. Specifically, it is set to 1,000 for municipalities with a population below 5,000 inhabitants; 1,200 for municipalities with a population between 5,000 and 20,000 and 1,500 for municipalities with population over 20,000. Municipalities with a T_m greater than or equal to one are assigned to the treatment condition.

[28] At the time, the Italian peninsula was divided into several independent states: the Kingdom of Sardinia, the Duchy of Parma, the Duchy of Modena, the Kingdom of Lombardy-Venetia under Austrian rule, the Grand Duchy of Tuscany, the Papal States, and the Kingdom of the Two Sicilies. Due to the small number of municipalities, the Duchy of Parma and the Duchy of Modena are pooled into a single fixed-effect category.

[29] The source for these data is the Official Gazette and various publications by the Ministry of Agriculture, Industry and Trade, titled "Bollettino delle privative industriali del Regno d'Italia".

A key identifying assumption in RDD is the absence of manipulation of the running variable around the cutoff. While population—drawn from the 1871 census—is fully predetermined with respect to the 1877 reform, a possible concern in our setting is that municipalities could have adjusted or misreported the number of teachers in anticipation of enforcement. However, several features of the legislative process make such manipulation very unlikely. First, under the framework of the Casati Law and subsequent legislation passed in 1876, teachers could not be dismissed unless formal notice was given at least six months before the end of the school year. Dismissal had to occur no later than in mid-April, otherwise the teacher was automatically reconfirmed (typically for an additional six years).[30] Second, it is unlikely that the teacher supply could be adjusted on such a short term. In fact, as discussed in Section 2, one of the transitional provisions of the 1877 law was the Ministry's commitment to open new teacher training schools. Third, data collection on the number of teachers started in April 1877, while the final version of the Law was passed in July 1877 giving a very short time-window for adjustment. Fourth, as discussed in Section 3, the classification of treated municipalities refers to those which could implement the Law without increasing the number of teachers. Fifth, the final classification of municipalities subject to enforcement had to be approved by the school council. Municipalities were officially notified only after this process. In addition to these arguments, we also perform the usual set of formal RD validity checks. In fact, we also construct a (false) pre-reform treatment and assess the robustness of our results to this alternative classification.

We formally estimate the following sharp regression discontinuity model:

$$P_m = \alpha + \tau\, CSL_m + \gamma\, T_m + \rho(CSL_m \times T_m) + X'_m\beta + States_m + \varepsilon_m \tag{2}$$

where P_m denotes our outcome of interest—patents per 10,000 inhabitants. CSL_m is an indicator equal to one for municipalities that, from a strict application of the Law, had to enforce compulsory schooling, and zero otherwise. The parameter of interest, τ, captures the effect of being assigned to the treatment and is interpreted as a local average treatment effect. T_m denotes the linear polynomial in teachers per capita, centered at the cutoff.

Following the RD literature, we estimate the model restricting our sample within an optimally chosen bandwidth and implement the robust inference methods proposed by Cattaneo et al. (2020a). In our setting, the conditional variance of the outcome and the curvature of the regression function (likely) differ across the cutoff. As emphasized by Cattaneo et al. (2020a), in such cases, asymmetric bandwidth selection is recommended. In our baseline analysis, we use a uniform kernel, which assigns

[30] See Legge 9 luglio 1876, n. 3250, art. 3, and Manuale degli amministratori comunali e provinciali e delle opere pie, 1878.

the same weight to all observations to reduce the possibility of assigning greater weight to units near the cutoff that may fall in the upper tail of the outcome distribution—i.e., exhibit high patenting rates— and drive the results. As a robustness check, we also report estimates using a triangular kernel, which gives more weight to observations near to the threshold.

To increase the precision of our estimates, we extend Equation (2) by including a set of predetermined covariates, X_m, which comprises geographical characteristics such as altitude, precipitation and temperature. We also include specifications controlling for pre-existing conditions in primary schooling such as school per capita, student-teacher ratio, and municipal expenditure per student in 1862/63. Finally, $States_m$, denotes fixed effects for pre-unification States based on the 1850 administrative boundaries.[31]

As a robustness check, to take into account that for a small proportion of our sample there is imperfect compliance with the above rule ($n = 16$)—i.e., the conditional probability of assignment to treatment jumps less than one at the cutoff—we also estimate a fuzzy RD specification where we instrument the treatment actually received with the treatment assignment rule.

4.1 Baseline results

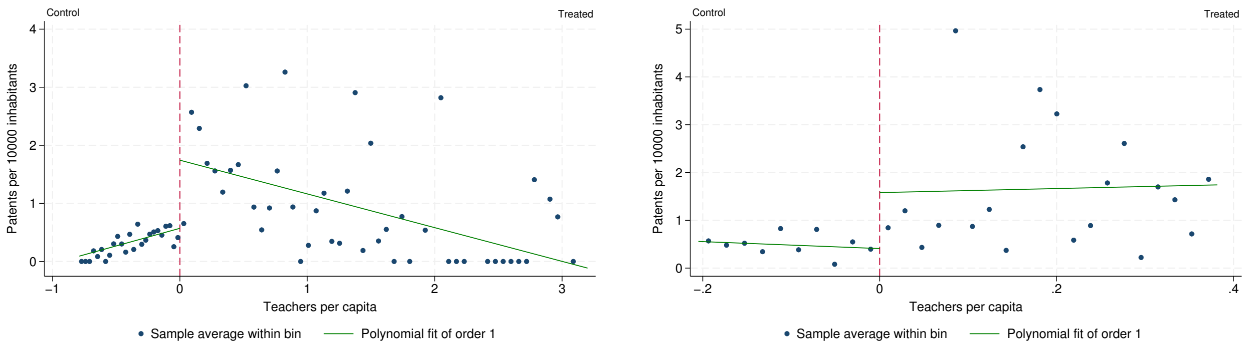
In Figure 3, we show the conditional expectation of our outcome of interest (patents per 10,000 inhabitants) against the running variable (T_m), with no bandwidth restriction (i.e. using the whole sample of municipalities with population larger than 5,000)[32] and with an optimally chosen bandwidth. The visual inspection of both plots shows a discontinuity in patents per capita and indicates that municipalities above the cutoff display a higher patenting activity. The corresponding estimate using the optimal bandwidth is reported in column (1) of Table 2. The coefficient indicates that municipalities which implemented compulsory schooling earlier have around 1.17 more patents per capita in 1908-12. Results are substantially identical when we control for geographic characteristics (column (2)), and when we account for pre-existing conditions in primary schooling (column (3)). In column (4), we include fixed effects for pre-unification states. Their inclusion mitigates concerns that the estimated effect may be confounded by broader North–South differences or by persistent differences in past institutions, such as the presence of pre-existing compulsory schooling laws (Bozzano et al., 2023). The results remain remarkably stable and are virtually unchanged compared to column (3), where the optimal bandwidth is computed using the same sample.

[31] For the list of pre-unitary states see footnote 26.

[32] The municipality of Aosta is excluded from the plot as it is an outlier that disproportionately affects the scale of the graph. This observation is included in all regressions and has no influence on the results.

Figure 3: Regression discontinuity plots

(a) No bandwidth restriction (b) Optimal Bandwidth



Note: Panel (a) displays the RD plot of patents per 10,000 inhabitants using the the full sample of municipalities with population 5,000 and over, while panel (b) displays the RD plot within the optimal bandwidth. The green lines show the linear polynomial fit for the RD estimate with no controls and employing a uniform kernel. The dots show the observed average values within bin.˙ Plots obtained using rdplot command from Cattaneo et al. (2020a)

Our results are remarkably significant at the conventional level of significance also when using inference procedures proposed by Cattaneo et al. (2020a). The robust *p-values* range from 0.004 to 0.02, and the corresponding robust confidence intervals span from [0.170, 1.985] to [0.387, 2.069], indicating a significant effect on patenting activity of an earlier adoption of compulsory schooling.

Table 2: Compulsory schooling and patenting

	(1)	(2)	(3)	(4)
SRD Estimate	1.171***	1.211***	1.110***	1.073**
	(0.390)	(0.375)	(0.383)	(0.418)
Robust CI	[0.387 ; 2.069]	[0.390 ; 2.022]	[0.212 ; 1.905]	[0.170 ; 1.985]
Robust p-value	0.00420	0.00377	0.0143	0.0199
Bandwidth(left)	0.205	0.229	0.224	0.194
Bandwidth(right)	0.382	0.485	0.699	0.453
Observations(left)	199	221	199	175
Observations(right)	237	288	295	228
Mean Outcome	1.126	1.122	1.144	1.122
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, these results indicate that municipalities which implemented CSL comparatively earlier experienced a higher level of patenting activity circa 30 years later. Put differently, our reduced form results suggest that the extension of mandatory education from two to three years, in combination with a stricter enforcement of mandatory education, fostered innovation across Italian municipalities. However, as discussed in Section 2, the enforcement of the Coppino Law seems to be positively correlated with enrollment rates. In Section 6, we delve further into this relationship, exploring literacy and enrollment in technical education as potential channels driving our results.

4.2 Falsification and placebo tests

We carry out several falsification and placebo tests to assess the validity of our RDD. First, we show that predetermined covariates are continuous at the cutoff. We estimate Equation (2) focusing on the effect of treatment assignment on predetermined covariates. As shown in Table A2, there is no discontinuity in geographic characteristics such as altitude, temperature, and precipitation (Panel A). Importantly, this test also shows that treated and control municipalities are similar in terms of pre-existing (1862/63) conditions in primary schooling such as the number of schools per capita, expenditure per student, and the student-teacher ratio (Panel B). These results clearly show that the municipalities which, about 15 years later, will be able to enforce the Coppino Law did not have any significant educational advantage. Figure A.3 displays the corresponding RD plots.

In Table A3, we report balancing tests using additional predetermined characteristics not included in the main specification. These include population growth between 1861 and 1871, teachers per capita in 1863, and industrial employment shares in 1878. Most importantly, we examine also patenting activity between 1863 and 1875, prior to the implementation of compulsory schooling in 1877. Figure A.4 displays the corresponding RD plots. We find no evidence of discontinuities in any of these variables. The absence of a discontinuity in patenting activity before the CSL also mitigates concerns that municipalities with higher innovation potentially invested more in primary education prior to the reform.

As an additional falsification test, we use patenting activity over the period 1878–1883, a time-window that immediately follows the reform. It is highly implausible that any effect on innovation could have materialized so quickly after the implementation of a reform on primary education. In other words, since the Law affected children aged 6 to 9, it is implausible that a human capital mechanism could have an effect within just a few years of implementation. The estimates are shown in Table 3, where we include also specifications with predetermined covariates and pre-unification state fixed effects. We find no evidence of a discontinuity across all specifications, consistent with the notion that a reform on primary education could not have an immediate effect on innovation.

As standard in the RD literature, we examine treatment effects using placebo cutoffs. First, using only the municipalities to the left of the real cutoff, we estimate Equation (2) at artificial cutoffs of 0.6

Table 3: RD estimates with patents 1878-1883 as a placebo outcome

	(1)	(2)	(3)	(4)
SRD Estimate	-0.00532	0.0517	0.220	0.166
	(0.215)	(0.219)	(0.207)	(0.208)
Robust CI	[-0.523 ; 0.412]	[-0.469 ; 0.485]	[-0.256 ; 0.650]	[-0.292 ; 0.602]
Robust p-value	0.816	0.973	0.394	0.496
Bandwidth(left)	0.118	0.115	0.139	0.137
Bandwidth(right)	0.465	0.564	0.657	0.491
Observations(left)	108	103	113	112
Observations(right)	284	319	289	247
Mean Outcome	0.428	0.431	0.401	0.405
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents (1878-1883) per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

teachers per capita (i.e., -0.4 for the normalized running variable). We carry out a similar exercise on the right of the cutoff, using a placebo cutoff at 1.4 teachers per capita (i.e., $+0.4$ for the normalized running variable). As shown in Figure A.5, we do not find any statistically significant RD treatment effect.

A first draft of the CSL, with slightly different thresholds, was presented to the Parliament in December 1876, i.e. about 6 months before the official passage of the Law. As discussed in Section 2, the draft law included more restrictive thresholds for the gradual enforcement of compulsory schooling: at least one lower primary teacher per 800 inhabitants in municipalities with fewer than 5,000 inhabitants, one per 1,000 in municipalities with 5,000–20,000 inhabitants, and one per 1,200 in larger municipalities. These thresholds provide an ideal placebo test. Indeed, we use these thresholds and the data on the number of teachers in 1875 reported in the original appendix of the draft law to define which municipalities would have implemented CSL had the original proposal been adopted. Since the thresholds of the actually enacted Law were less restrictive, the majority of the municipalities implementing the criteria under the draft law would be actually treated. Therefore, we should not expect any significant effect when applying the thresholds of the draft law. On the contrary, finding a significant effect would suggest that our RDD is related to some pre-existing non-random characteristics.

Table 4 presents the results of this placebo test. Similar to the baseline estimates, we show results without controls (column (1)), with geographical controls (column (2)), with geographical and pre-

Table 4: Placebo estimates using the first draft of the Law

	(1)	(2)	(3)	(4)
SRD Estimate	-1.234	-1.145	-0.811	0.223
	(0.876)	(0.811)	(0.751)	(0.756)
Robust CI	[-3.524 ; 0.528]	[-3.294 ; 0.510]	[-2.961 ; 0.665]	[-1.630 ; 1.925]
Robust p-value	0.147	0.151	0.215	0.871
Bandwidth(left)	0.241	0.268	0.353	0.368
Bandwidth(right)	0.512	0.511	0.617	0.347
Observations(left)	180	221	275	287
Observations(right)	241	241	211	159
Mean Outcome	1.475	1.306	1.244	1.154
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

existing schooling characteristics (column (3)), and also adding pre-unification states fixed effects (column (4)). In columns (1)-(3), all the point estimates are negative and statistically not different from zero. In column (4), the coefficient turns positive but it is not statistically significant and small relative to the mean of the outcome. These results strongly suggest that only the municipalities affected by the CSL approved in July 1877 experienced a significant increase in patenting activity.

4.3 Manipulation of the assignment rule

A crucial assumption for the validity of the RDD approach is that there is no manipulation of the assignment rule. To investigate such assumption we carry out the formal test on the continuity of the density of the running variable proposed by Cattaneo et al. (2020b). We carry out the test using our baseline sample of municipalities larger than 5,000 inhabitants. The value of the test is -0.456 and the *p-value* is 0.65.[33] Therefore, we fail to reject the null hypothesis of continuity of the density of teachers per capita.[34]

Always in the context of manipulation, we check whether our results could be driven by few municipalities close to the cutoff. To do that, we perform a donut-hole analysis.[35] We start by excluding 16 observations from the sample (donut-hole radius 0.01) and repeat the exercise until we

[33] We carry out the test using a third order local polynomial to construct the density estimators.

[34] For completeness, we carried out the test using all 8,302 Italian municipalities to ensure that the no-manipulation assumption holds across the entire sample. The value of the test is 1.005 with the *p-value* of 0.32.

[35] The donut-hole approach is informative even in the absence of manipulation, since local polynomial estimates can be particularly sensitive to units near the threshold Cattaneo et al. (2020a).

exclude 65 municipalities (donut-hole radius 0.05). In panel (a) of Figure A.6 we report the plot of the sharp RD parameter and the robust confidence intervals for the model in column (1) of Table 2. In panel (b), (c), and (d) we carry out the exercise for the models including the control variables. The point estimates are similar to the baseline and remain statistically significant across all models indicating that the main results are not driven by the few municipalities close to the cutoff.

Finally, we test whether there has been manipulation in the sense of strategically changing the number of teachers. One could argue that municipalities particularly eager to implement CSL, foreseeing the thresholds of the Law, strategically hired more teachers. The opposite strategic behavior, that is stopping hiring teachers to avoid the implementation of CSL, is also conceivable. In order to identify such potential "manipulators", we apply the actual threshold to the official number of teachers in place in 1875, that is two years before the passage of the Law. This allows us to infer which municipalities would have met the final criteria for the implementation of CSL without adjusting the number of teachers. Municipalities that did not meet the criteria in 1875 but met them in 1877 are instead defined as "switchers", that is, municipalities that potentially changed strategically the number of teachers.[36] Among 1,093 municipalities for which we have data on teachers in both years,[37] 90.5% would have retained the same treatment status, while 7.1% switched from control to treated and only 26 municipalities switched from treated to control.

We thus estimate of our baseline models excluding the "switchers", that is, those municipalities that would not have met the enforcement thresholds using 1875 teacher data but were classified as treated based on 1877 data, and vice versa. This leaves us with a sample of 989 municipalities. As shown in Table 5, the coefficients remain statistically significant, although slightly higher compared to the baseline. This exercise shows that, even if we drop potential manipulators, results are similar to the baseline estimates.

4.4 Robustness checks

In this subsection, we provide several tests to ensure the robustness of our baseline estimates. First, our application of the thresholds for the CSL diverges from the list of municipalities reported in the official publication for 16 municipalities.[38] We thus estimate a fuzzy RDD. In Panel (a) and (b) of Figure A.7, we plot the first stage and the reduced form for the fuzzy design using an optimally

[36] We are reasonably assuming that municipalities could only change the number of teachers and not the underlying population to affect the number of teachers per capita. In addition, we acknowledge that some differences in the number of teachers between 1875 and 1877 may reflect responses to changes in enrollment rates, teachers' retirements, municipality boundary changes, or even errors in the data. Nevertheless, this comparison allows us to approximate, to a reasonable extent, the municipalities whose treatment status changed.

[37] Two municipalities were created in 1877, and we were not able to match two other municipalities.

[38] See footnote 17 for more details.

Table 5: RD estimates excluding switchers between 1875-1877

	(1)	(2)	(3)	(4)
SRD Estimate	2.054***	1.924***	1.914***	1.439**
	(0.542)	(0.608)	(0.676)	(0.615)
Robust CI	[0.783 ; 3.331]	[0.592 ; 3.336]	[0.508 ; 3.448]	[0.085 ; 2.835]
Robust p-value	0.00156	0.00502	0.00836	0.0374
Bandwidth(left)	0.250	0.170	0.209	0.215
Bandwidth(right)	0.706	0.612	0.481	0.587
Observations(left)	221	149	167	173
Observations(right)	286	258	178	210
Mean Outcome	1.191	1.375	1.179	1.267
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

chosen bandwidth.[39] In Panel (a), we plot the probability of enforcing compulsory schooling against our running variable. At the cutoff, the probability of receiving the treatment is around 0.61. Note that to the left of the cutoff, the fitted regression line is steep because the few non-compliers have values of teachers per capita very close to the cutoff. For comparison, when using the full sample of urban municipalities, the probability of receiving the treatment is 0.91, as the broader range of observations produces a less steep slope. For completeness, in Panel (b), we show the corresponding reduced form for the fuzzy RDD. The coefficients of the fuzzy RDD are reported in Table A4. The point estimates are larger compared to our baseline across all specifications, with fuzzy RD estimates ranging from 1.5 to 2.3. This difference is expected due to the scaling of the reduced form by the first stage.

In panel B of Table A4, we drop these 16 observations and re-estimate our baseline models. Finally, in Panel C, we slightly adjust treatment classification for a small number of municipalities near the threshold. Specifically, we reclassify municipalities with teacher per capita greater than 0.99 as treated, under the assumption that minor rounding may have led to discrepancies in the definition of the treatment status. In both cases results remain stable and statistically significant, indicating that our findings are not sensitive to minor misclassifications around the cutoff.

We further assess the robustness of our findings by exploring an alternative kernel function, different specifications, methods for the bandwidth choice and samples. In Panel A of Table A5, we report the results when employing a triangular kernel. In Panel B, we estimate both models including a second

[39] The falsification tests presented above also apply to the fuzzy RD framework, as noted in Cattaneo et al. (2023).

order polynomial in the running variable. The estimations confirm our baseline findings and the point estimates are statistically significant.

In our baseline model, we employ two different MSE-optimal bandwidth selectors. As a further robustness check, we re-estimate the model using a symmetric bandwidth and also by using the whole sample of urban municipalities (see Table A6). Also in this case the estimates are in line with our baseline results. In Panel B, we estimate the model using a CER-optimal bandwidth and finally in Panel C, we do not apply any bandwidth restriction and use the whole sample of the municipalities 5,000 and over.

As a further robustness check, we apply our baseline models (Equation 2) to the full sample of 8,301 Italian municipalities—including those with populations below 5,000. In Panel A of Table A7, we find statistically significant estimates across all specifications, with point estimates ranging from 0.2 to 0.324. Given the sample mean of approximately 0.46, these are substantial effects indicating that the implementation of compulsory schooling had a significant impact on patenting activity.

Given the nature of the main outcome (patents), we estimate count data models with the absolute number of patents as dependent variable (Table A8). Given the large number of zeros in our outcome variable, we estimate zero-inflated negative binomial models (columns (1)-(4)) and zero-inflated Poisson models (columns (5)-(8)).[40] In both cases, we report sharp RD estimates for the same models and bandwidths as in Panel A of Table 2. For all models and specifications the coefficients remain positive and significant indicating a positive effect of CSL also on the absolute number of patents.

As a further check, we winsorize the outcome variable (patents per 10,000 inhabitants) at the 1st and 99th percentiles to ensure that the results are not driven by municipalities with extreme patenting values (see Table A9). The point estimates are slightly lower and remain statistically significant across all specifications.

As shown in Figure 2, municipalities which did not meet the criteria to implement compulsory schooling are predominantly located in the South. While our specifications already include pre-unification state fixed effects, we further assess robustness by excluding municipalities that belonged to the Kingdom of the Two Sicilies. The idea is that municipalities in the South might have been culturally different in their approach to investments in education, thus not representing an ideal control group. The resulting sample includes 609 municipalities, of which 453 are treated and 156 control. Using this sample, we estimate our baseline models using the same kernel, bandwidth selection, and estimation procedure. As reported in Table A10, the results remain strongly significant despite the

[40] To account for potential overdispersion and excess zeros in patent counts, we include population in 1911 as an inflate variable to predict the structural zeros in the data.

smaller sample size. Point estimates are slightly larger, which is consistent with the higher average level of patenting observed in this sample.

In a similar way, we assess robustness by excluding municipalities that had universities. Here the idea is to discard the potential confounding effect of universities which might have had complementarities with mass education. The resulting sample includes 1,076 municipalities, of which 529 are treated and 547 control. The results, reported in Table A11, remain statistically significant and virtually the same as the baseline.

We further test the robustness of our results by using an alternative measure for innovation. Driven also by the availability of data, we use trademarks. Trademarks were (and still are) used to brand products and/or firms and thus gave the exclusive right to use a brand and thus to appropriate the economic returns on both innovative and more established products. In this sense, trademarks should be viewed as complementary to patents.

In Table A12, we report estimates using as outcome the stock of trademarks per 10,000 inhabitants registered in the period 1900-12.[41] Panel A presents the model without controls, showing that municipalities implementing CSL registered approximately 0.785 more trademarks per capita. The estimates are virtually identical when accounting for geographic characteristics (column (2)), additional controls for pre-existing schooling conditions (column (3)), and including pre-unification states fixed effects (column (4)). The results are statistically significant at conventional levels, as well as under robust inference methods.

5 CSL, education, and industrialization

The analysis so far has shown that the early implementation of CSL, i.e. the obligation of attending three years of elementary school assisted by a more stringent enforcement, subsequently brought about a significant increase in innovation as witnessed by a larger number of patents and trademarks. This is consistent with previous literature linking different dimensions of human capital, knowledge, and innovation in the context of historical Germany (Cinnirella and Streb, 2017; Cinnirella et al., 2025).

Yet, whether there is a positive relationship between formal education and industrialization is still hotly debated in the literature despite mounting empirical evidence in favor of it (Becker et al., 2011; de Pleijt et al., 2020; Franck and Galor, 2022). Our study allows to contribute also to this question. In fact, our institutional setting allows to study whether and to what extent an educational supply shock, in the form of an earlier implementation of CSL, affects industrialization. In particular,

[41] We standardize the number of trademarks by the municipal population in 1911.

we can estimate the impact of the early implementation of CSL in 1877 on employment in various industrial sectors, namely extractive, agriculture-related, metalworking, mineral and construction, textiles, chemicals, and services in 1911. Each variable is defined as the number of people employed in a given sector per 1,000 inhabitants, allowing us to capture sector-specific and overall industrial employment per capita. Additionally, we consider total industrial employment. To credibly identify the effect of CSL on industrial employment, we exploit the RDD as before.

The estimates of the effect of CSL on industrial employment are reported in Table 6. The table is structured as follows: in Panel A we present estimates without any control variable; in Panel B we include geographical controls; in Panel C we add schooling controls; finally in Panel D we further add pre-unification states controls. The columns report the different industrial sectors (2–8), with exception of column 1 which reports estimates for the total industrial employment.

The results suggest an overall positive effect of early implementation of compulsory schooling on total industrial employment as shown in column (1). The effect becomes significant when subsequently adding control variables. When looking at the singular sectors, a pattern emerges as the effect of early implementation of CSL is large and significant only in the metal and service sector (columns (4) and (8)).

Table 6: Compulsory schooling and industry structure

	Industry (total)	Workers per 1,000 inhabitants by industry						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Agriculture	Textiles	Metals	Extractive	Construction	Chemicals	Services
Panel A: No Controls								
SRD Estimate	15.84	-0.615	1.450	8.607***	0.282	1.027	2.059	1.818**
	(10.60)	(3.879)	(5.885)	(2.808)	(0.978)	(3.064)	(1.528)	(0.854)
Robust CI	[-7.776 ; 40.323]	[-9.987 ; 8.329]	[-12.519 ; 14.402]	[2.158 ; 14.081]	[-2.018 ; 2.308]	[-6.642 ; 6.833]	[-1.533 ; 5.512]	[-0.331 ; 3.820]
Robust p-value	0.185	0.859	0.891	0.00760	0.895	0.978	0.268	0.0996
Bandwidth(L/R)	0.232 / 0.414	0.177 / 0.451	0.262 / 0.388	0.222 / 0.627	0.132 / 0.361	0.132 / 0.392	0.217 / 0.696	0.236 / 0.594
Observations(L/R)	220 / 251	172 / 266	245 / 239	213 / 334	118 / 221	118 / 241	208 / 354	222 / 328
Mean Outcome	58.48	21.08	13.54	9.184	1.773	8.432	2.789	1.953
Panel B: Geographical Controls								
SRD Estimate	16.52*	0.922	-1.532	7.479**	0.396	1.495	2.608	1.983***
	(9.650)	(3.714)	(5.785)	(3.003)	(0.938)	(2.706)	(1.651)	(0.760)
Robust CI	[-6.007 ; 38.038]	[-7.753 ; 9.508]	[-15.257 ; 10.870]	[0.615 ; 13.986]	[-1.570 ; 2.588]	[-4.225 ; 8.133]	[-0.890 ; 6.455]	[0.184 ; 3.767]
Robust p-value	0.154	0.842	0.742	0.0323	0.631	0.535	0.138	0.0307
Bandwidth(L/R)	0.215 / 0.480	0.181 / 0.469	0.255 / 0.449	0.223 / 0.804	0.145 / 0.409	0.164 / 0.439	0.200 / 0.452	0.215 / 0.707
Observations(L/R)	206 / 284	173 / 281	241 / 266	213 / 390	132 / 251	155 / 263	192 / 268	205 / 359
Mean Outcome	60.89	21.10	15.11	9.647	1.762	8.391	2.869	1.948
Panel C: Geographical & Schooling Controls								
SRD Estimate	16.91*	1.548	2.183	9.325***	0.556	1.752	1.254	1.820**
	(9.874)	(3.926)	(6.292)	(3.117)	(1.034)	(2.986)	(1.710)	(0.911)
Robust CI	[-7.799 ; 37.884]	[-7.606 ; 10.707]	[-13.252 ; 15.724]	[2.572 ; 15.777]	[-1.888 ; 2.659]	[-5.215 ; 8.735]	[-3.061 ; 4.362]	[-0.260 ; 3.975]
Robust p-value	0.197	0.740	0.867	0.00646	0.740	0.621	0.731	0.0855
Bandwidth(L/R)	0.217 / 0.509	0.186 / 0.466	0.200 / 0.500	0.237 / 0.630	0.160 / 0.468	0.172 / 0.364	0.162 / 0.678	0.236 / 0.641
Observations(L/R)	191 / 249	161 / 235	175 / 247	205 / 276	136 / 235	150 / 187	138 / 288	205 / 280
Mean Outcome	61.70	21.18	16.96	9.079	1.621	8.193	2.853	1.932
Panel D: Geographical, schooling & Pre-unification States Controls								
SRD Estimate	17.46*	1.846	2.427	10.79***	-0.0202	-0.115	1.021	1.813**
	(9.657)	(3.881)	(5.834)	(3.343)	(1.141)	(3.070)	(1.721)	(0.868)
Robust CI	[-6.688 ; 37.586]	[-7.555 ; 10.612]	[-11.287 ; 15.585]	[4.170 ; 18.385]	[-2.608 ; 2.309]	[-7.434 ; 5.928]	[-3.272 ; 4.213]	[-0.205 ; 3.820]
Robust p-value	0.171	0.742	0.754	0.00187	0.905	0.825	0.805	0.0784
Bandwidth(L/R)	0.198 / 0.391	0.186 / 0.473	0.180 / 0.410	0.237 / 0.490	0.099 / 0.453	0.123 / 0.404	0.159 / 0.694	0.219 / 0.700
Observations(L/R)	174 / 200	163 / 235	157 / 209	207 / 243	71 / 225	100 / 203	136 / 290	194 / 293
Mean Outcome	59.40	21.17	15.32	9.131	1.674	8.492	2.876	1.931

Note: Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Panel A, we find that municipalities which meet the criteria to implement CSL experienced an increase of 8.6 workers per 1,000 people in the metal sector and 1.8 in the service sector. The effect on employment in the metal sector increases to 10.8 in the more demanding specification in Panel D, whereas the effect on employment in the service sector remains constant. With a mean employment of, respectively, 9.1 and 1.9 per 1,000 people, these effects are particularly large. For total industrial employment (column (1), Panel D) the estimated effect is an increase of 17.5 workers per 1,000 people, which is about 30% of the mean.

Overall, these findings suggest that the early implementation of compulsory schooling contributed to industrialization by supplying a relatively more skilled and disciplined workforce apt to work in a more technically advanced sector such a metal working. The service sector also benefited from a more educated workforce. These results are in line with previous findings of Becker et al. (2011), who find that basic education significantly contributed to industrialization in Prussia. Based on computed measure for years of schooling, the authors find that education is positively related to industrialization measured as the share of people employed in factories. In particular, they find significant effects for employment outside the metal and textile industries in 1849. For the Second Industrial Revolution (1882), they find an effect also on employment in the metal industry and zero for the textile sector. Differently from their study, we can isolate the effect of a supply shock through the implementation of a CSL. However, similar to their results, we also find no effect on the textile industry, but a large effect on employment in the metal sector.

6 Educational attainment as a mechanism

In the introductory notes of the official survey for the implementation of the CSL published in 1878, the Minister of Public Education Coppino, argues that the level of education of the Kingdom will benefit from the full implementation of the CSL.[42] In this section, we assess whether the early implementation of compulsory schooling, indeed, contributed to improve the average level of education of the population. This would constitute also a potential mechanism of our findings as innovative activity (and industrial employment) could be due to higher levels of education attained through CSL.

Ideally, to assess the effectiveness of the reform, we would like to observe on a yearly basis the trend of enrollment rates by municipality. This is not possible as such data simply do not exist.

[42] In fact, beyond the statistic tables, our main statistical source, the survey has a very detailed description of the educational situation of the Kingdom. The survey describes region by region strengths and weaknesses of the school system. One of the most recurring weakness reported is the absolute lack of teachers.

Instead, we analyze whether CSL affected (i) enrollment in technical schools shortly after the reform (1884-87) and (ii) the literacy rate in 1911.[43] For the analysis of technical education, we study the intensive margin restricting the sample to municipalities that had at least one technical school. According to the Casati Law, these schools were to be established primarily in provincial capitals, or, alternatively, in other sizable municipalities that met specific conditions—such as having both lower and upper primary schools in place. Since the data are recorded at the school level and aggregated at the municipality level, municipalities without a technical school would mechanically report zero enrollment. We therefore restrict our RDD approach to municipalities in which technical schools were actually established.

Data on literacy are, instead, available for all municipalities but only in 1911, i.e. about 35 years after the implementation of the reform. Differences in literacy, strictly around the cutoffs, are likely to be vanished after 35 years as the reform was about earlier vs. later adoption of CSL and the literacy data includes also children above 6 years old.[44] For this reason we carry out the analysis using the whole sample of municipalities with 5,000+ inhabitants without applying the optimal bandwidth.[45]

In Panel A of Table 7, we report results when considering the average enrollment rate in technical schools in the years 1884-87 as dependent variable. We obtain positive and significant coefficients across all specifications. In our most restrictive model which includes pre-unification states fixed effects, our results suggest that CSL increased the enrollment in technical schools by 7.3 students per 10,000 inhabitants, that is about 23% of the mean. In Panel B, we show estimates using the literacy rate in 1911 as outcome. The coefficient is always positive and significant, implying that municipalities which implemented CSL in 1877 have higher levels of literacy about 35 years later. In terms of magnitude, the point estimates are less stable but the most conservative specification implies that an earlier implementation of CSL would have increased the literacy rate by 2.6 percentage points, i.e about 5% of the mean.

The CSL was thought to improve the educational attainment of the central and southern regions which lagged behind with respect to the rest of the country. In order to have a better understanding of the efficacy of the reform on these regions, we performed the same RD analysis excluding the northern regions.[46] It should be noted that in the Center and the South there is a disproportionally higher number of control municipalities than treated ones (see Figure 2). For the same reason of low variation,

[43] This is the first year for which municipality-level data on literacy are available.

[44] This reasoning does not apply to the case of patenting and industrial employment as we consider the adult population.

[45] Indeed, when applying the optimal bandwidth, the coefficients are consistently positive but imprecisely estimated. Estimates not shown but available upon request.

[46] Specifically, we exclude municipalities located in the former Kingdom of Sardinia and the Kingdom of Lombardy–Venetia. Note that the Sardinia region, although geographically southern, is included because it belonged to the Kingdom of Sardinia.

Table 7: Human capital as a mechanism: CSL, Technical Schools, and Literacy

	(1)	(2)	(3)	(4)
Panel A: Dep.var: Enrollment technical schools (1884-87)				
SRD Estimate	10.22***	10.07***	7.454**	7.277**
	(3.419)	(3.349)	(3.255)	(3.140)
Robust CI	[-1.633 ; 15.730]	[-1.814 ; 15.115]	[-2.802 ; 13.051]	[-1.495 ; 13.655]
Robust p-value	0.112	0.124	0.205	0.116
Bandwidth(left)	0.800	0.800	0.800	0.800
Bandwidth(right)	3.200	3.200	3.200	3.200
Observations(left)	128	128	119	119
Observations(right)	163	163	140	140
Mean Outcome	33.21	33.21	32.14	32.14
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes
Panel B: Dep. var: Literacy rate 1911				
SRD Estimate	10.83***	6.576***	5.154***	2.651**
	(1.661)	(1.422)	(1.358)	(1.103)
Robust CI	[0.991 ; 10.963]	[0.619 ; 9.022]	[-0.279 ; 7.862]	[-0.414 ; 6.160]
Robust p-value	0.0188	0.0245	0.0679	0.0867
Bandwidth(left)	0.800	0.800	0.800	0.800
Bandwidth(right)	3.200	3.200	3.200	3.200
Observations(left)	555	555	528	528
Observations(right)	541	541	447	447
Mean Outcome	55.09	55.09	53.50	53.50
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable in Panel A is the enrollment rate in technical schools (1884-87). The dependent variable in Panel B is the literacy rate of individuals older than 6 years old in 1911. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in these regressions we do not include pre-unitary states fixed effects. The estimates are reported in Table 8. The positive and significant coefficients confirm the positive effects of CSL on literacy in the central and southern regions. In terms of magnitude, the most conservative specification in column (3) indicates that CSL increased the literacy rate by about 7% with respect to the mean.

In sum, these estimates provide strong evidence in favor of the efficacy of the CSL and suggest a mechanism behind the effect of CSL on innovation and industrial employment. The early implementation of CSL, with three mandatory years of primary education and a more stringent enforcement, generated a higher level of educational attainment, visible to us through higher enrollment rates in technical schools and higher literacy rates. The higher level of educational

Table 8: CSL and literacy in the Center and South of Italy

	(1)	(2)	(3)
SRD Estimate	6.394***	4.382***	3.089**
	(1.592)	(1.429)	(1.395)
Robust CI	[-0.344 ; 8.818]	[0.320 ; 8.338]	[-0.389 ; 7.602]
Robust p-value	0.0699	0.0343	0.0769
Bandwidth(left)	0.800	0.800	0.800
Bandwidth(right)	3.200	3.200	3.200
Observations(left)	521	521	501
Observations(right)	303	303	287
Mean Outcome	46.95	46.95	46.69
Geographical Controls	No	Yes	Yes
Schooling Controls	No	No	Yes
Pre-unification States FE	No	No	No

Note: The dependent variable is the literacy rate of individuals older than 6 years old in 1911. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

attainment translated later into higher levels of innovation and industrial employment, especially in the metal and service sector.

7 Conclusions

There is an increasing amount of empirical evidence stressing the importance of education for growth and development. Yet, the study of the impact of compulsory schooling laws (CSL) on the economy has received less attention also because of the difficulty of identifying exogenous variation of reforms which, in general, occur at state level. Furthermore, CSLs operated often in conjunction with child labor laws, making it even more complicated disentangling the effects of the two laws.

In this paper, we exploit an almost unique feature of a CSL passed in 1877 Italy, according to which the implementation of the Law followed a rule based on the ratio of teachers over population. This feature created discontinuities across municipalities in the implementation of CSL which can be exploited to assess the effect on mandatory education (and more stringent enforcement) on economic outcomes.

We find that municipalities above the cutoff, which thus implemented CSL earlier, experienced later significantly higher levels of innovation, measured in terms of patents and trademarks per capita. Consistently, we also find a general higher level of employment in industry, in particular in metalworking and service sector. These results are explained by the fact that, in line with the purpose of the proponents, CSL increased literacy levels and had also a positive impact on the enrollment in technical secondary schools.

In terms of policy implications, our findings provide a strong support for the role of CSLs in promoting growth and development. More specifically, our results suggest that an earlier implementation and enforcement of compulsory education in Italy could have significantly augmented the level of human capital. This, in turn, could have placed the country on a different path and a more rapid transition to economic growth, characterized by more innovation and a more rapid adoption of technological change.

References

Ager, P. and Cinnirella, F. (2021). Froebel's Gifts: How the Kindergarten Movement Changed the American Family. *CEPR Discussion Paper DP 15146.*

Ager, P., Cinnirella, F., Eriksson, K., and Malein, V. (2025). Kindergartens and Intergenerational Mobility. *AEA Papers and Proceedings*, 115:415–20.

Angrist, J. D. and Krueger, A. B. (1991). Does Compulsory School Attendance Affect Schooling and Earnings? *The Quarterly Journal of Economics*, 106(4):979–1014.

Bandiera, O., Mohnen, M., Rasul, I., and Viarengo, M. (2018). Nation-building Through Compulsory Schooling during the Age of Mass Migration. *The Economic Journal*, 129(617):62–109.

Becker, S. O., Hornung, E., and Woessmann, L. (2011). Education and catch-up in the industrial revolution. *American Economic Journal: Macroeconomics*, 3(3):92–126.

Becker, S. O. and Woessmann, L. (2009). Was weber wrong? a human capital theory of protestant economic history. *Quarterly Journal of Economics*, 124(2):531–596.

Bozzano, M., Cappelli, G., and Vasta, M. (2023). Whither Education? The Long Shadow of Pre-Unification School Systems into Italy's Liberal Age (1861–1911). *The Journal of Economic History*, 84(1):149–190.

Buonanno, P., Cinnirella, F., Harka, E., and Puca, M. (2024). Books Go Public: The Consequences of the Expropriation of Monastic Libraries on Innovation. *CESifo Working Paper No. 11015.*

Cappelli, G. (2016). One size that didn't fit all? Electoral franchise, fiscal capacity and the rise of mass schooling across Italy's provinces, 1870–1911. *Cliometrica*, 10:311–343.

Cappelli, G. and Vasta, M. (2020). Can school centralization foster human capital accumulation? A quasi-experiment from early twentieth-century Italy. *The Economic History Review*, 73(1):159–184.

Cattaneo, M. D., Idrobo, N., and Titiunik, R. (2020a). *A Practical Introduction to Regression Discontinuity Designs: Foundations.* Elements in Quantitative and Computational Methods for the Social Sciences. Cambridge University Press.

Cattaneo, M. D., Idrobo, N., and Titiunik, R. (2023). A Practical Introduction to Regression Discontinuity Designs: Extensions.

Cattaneo, M. D., Jansson, M., and Ma, X. (2020b). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.

Cinnirella, F. and Hornung, E. (2016). Landownership concentration and the expansion of education. *Journal of Development Economics*, 121:135–152.

Cinnirella, F., Hornung, E., and Koschnick, J. (2025). Flow of Ideas: Economic Societies and the Rise of Useful Knowledge. *Economic Journal*, forthcoming.

Cinnirella, F. and Streb, J. (2017). The role of human capital and innovation in economic development: Evidence from post-malthusian prussia. *Journal of Economic Growth*, 22(2):193–227.

Clay, K., Lingwall, J., and Stephens, Melvin, J. (2012). Do Schooling Laws Matter? Evidence from the Introduction of Compulsory Attendance Laws in the United States. Working Paper 18477, National Bureau of Economic Research.

de Pleijt, A., Nuvolari, A., and Weisdorf, J. (2019). Human Capital Formation During the First Industrial Revolution: Evidence from the use of Steam Engines. *Journal of the European Economic Association*, 18(2):829–889.

de Pleijt, A., Nuvolari, A., and Weisdorf, J. (2020). Human capital formation during the first industrial revolution: Evidence from the use of steam engines. *Journal of the European Economic Association*, 18(2):829–889.

de Pleijt, A. M. (2016). Human capital formation in the long run: evidence from average years of schooling in England, 1300–1900. *Cliometrica*, 12(1):99–126.

de Pleijt, A. M. and Frankema, E. (2025). The deeper roots of human capital formation and economic development in Southeast Asia, 1900–2000. *Journal of Development Economics*, 176:103506.

Feigenbaum, J. and Russo, G. (2020). The Effect of Child Labor Laws on Child Labor. *mimeo.*

Fick, S. E. and Hijmans, R. J. (2017). WorldClim 2: New 1-km Spatial Resolution Climate Surfaces for Global Land Areas. *International Journal of Climatology*, 37(12):4302–4315.

Franck, R. and Galor, O. (2022). Technology-skill complementarity in early phases of industrialization. *Economic Journal*, 132(642):618–643.

Galor, O. and Moav, O. (2006). Das Human-Kapital: A Theory of the Demise of the Class Structure. *Review of Economic Studies*, 73(1):85–117.

Galor, O., Moav, O., and Vollrath, D. (2009). Inequality in Landownership, the Emergence of Human-Capital Promoting Institutions, and the Great Divergence. *Review of Economic Studies*, 76(1):143–179.

Goldin, C. (2014). *Human Capital*, pages 1–27. Springer Berlin Heidelberg.

Goldin, C. and Katz, L. F. (2008). *Mass Secondary Schooling and the State: The Role of State Compulsion in the High School Movement*, pages 275–310. University of Chicago Press.

Goldin, C. and Katz, L. F. (2010). *The Race between Education and Technology.* Harvard University Press.

Harka, E., Nunziata, L., and Rocco, L. (2023). The Alabaster Ceiling: The Gender Legacy of the Papal States. GLO Discussion Paper 1315, Global Labor Organization (GLO), Essen.

Kelly, M., Mokyr, J., and Ó Gráda, C. (2023). The mechanics of the industrial revolution. *Journal of Political Economy*, 131(1):59–94.

Landes, W. M. and Solmon, L. C. (1972). Compulsory Schooling Legislation: An Economic Analysis of Law and Social Change in the Nineteenth Century. *The Journal of Economic History*, 32(1):54–91.

Lindert, P. (2004). *Growing Public. Social Spending and Economic Growth since the Eighteenth Century.Growing Public (Vol. 1).* Cambridge, MA: Cambridge University Press.

Lleras-Muney, A. and Shertzer, A. (2015). Did the Americanization Movement Succeed? An Evaluation of the Effect of English-Only and Compulsory Schooling Laws on Immigrants. *American Economic Journal: Economic Policy*, 7(3):258–90.

Lleras-Muney, A. (2002). Were Compulsory Attendance and Child Labor Laws Effective? An Analysis from 1915 to 1939. *The Journal of Law and Economics*, 45(2):401–435.

Madsen, J. B. and Murtin, F. (2017). British economic growth since 1270: the role of education. *Journal of Economic Growth*, 22(3):229–272.

Margo, R. A. and Finegan, T. A. (1996). Compulsory schooling legislation and school attendance in turn-of-the century America: A 'natural experiment' approach. *Economics Letters*, 53(1):103–110.

Mariella, V. (2022). Landownership concentration and human capital accumulation in post-unification Italy. *Journal of Population Economics*, 36(3):1695–1764.

Meisenzahl, R. R. and Mokyr, J. (2012). The rate and direction of invention in the british industrial revolution: Incentives and institutions. In Lerner, J. and Stern, S., editors, *The Rate and Direction of Inventive Activity Revisited*, pages 443–479. University of Chicago Press.

Mokyr, J. (2009). *The Enlightened Economy: An Economic History of Britain 1700–1850*. Yale University Press.

Moser, P. (2005). How Do Patent Laws Influence Innovation? Evidence from Nineteenth-century World's Fairs. *American Economic Review*, 95(4):1214–1236.

Nuvolari, A. and Vasta, M. (2014). Independent Invention in Italy during the Liberal Age, 1861–1913. *The Economic History Review*, 68(3):858–886.

Nuvolari, A. and Vasta, M. (2017). The Geography of Innovation in Italy, 1861–1913: Evidence from Patent Data. *European Review of Economic History*, 21(3):326–356.

Nuvolari, A. and Vasta, M. (2019). Patenting the Risorgimento: Economic Integration and the Formation of the Italian Patent System (1855–1872). *Jahrbuch für Wirtschaftsgeschichte / Economic History Yearbook*, 60(1):93–122.

Squicciarini, M. P. (2020). Devotion and Development: Religiosity, Education, and Economic Progress in Nineteenth-Century France, journal = American Economic Review. 110(11):3454–91.

Squicciarini, M. P. and Voigtländer, N. (2015). Human capital and industrialization: Evidence from the age of enlightenment. *Quarterly Journal of Economics*, 130(4):1825–1883.

Appendix

Timeline of The Coppino Law

16 Dec 1876 Presentation of the draft law by Minister Michele Coppino. The draft included the following threshold rules for implementation: compulsory schooling would be implemented in municipalities with fewer than 5,000 inhabitants if they had at least one lower primary teacher per 800 residents; in municipalities with 5,000 to 20,000 inhabitants if the ratio was at least one teacher per 1,000; and in larger municipalities if there was at least one teacher per 1,200 inhabitants. *Source: Parliamentary Acts, Chamber of Deputies, Session 1876–77, Documents, Draft Laws and Reports, No. 42.*

19 Feb 1877 The Parliamentary Commission proposed more relaxed thresholds: compulsory schooling would be enforced in municipalities with fewer than 5,000 inhabitants if they had at least one lower primary teacher per 1,000 inhabitants; in municipalities with 5,000 to 20,000 inhabitants if the ratio was at least one teacher per 1,200 inhabitants; and in larger municipalities if there was at least one teacher per 1,500 inhabitants. *Source: Parliamentary Acts, Chamber of Deputies, Session 1876–77, Documents, Draft Laws and Reports, No. 42-A.*

5–10 Mar 1877 Parliamentary discussion of the law began in the Chamber of Deputies on March 5. The law was approved on March 10 with 208 votes in favor and 20 against. The approved version included the relaxed implementation thresholds proposed by the Parliamentary Commission. *Source: Parliamentary Acts, Chamber of Deputies, Session 1876–77, Discussions.*

30 Apr 1877 Minister Michele Coppino issues Ministerial Directive N. 517, instructing Superintendents to collect municipal-level data on teachers while the law awaited Senate approval. The goal was to assess, for each municipality, whether the new law could be implemented immediately or whether local conditions still fell short of the required thresholds. *Source: "Sull'obbligo della istruzione elementare nel Regno d'Italia: attuazione della legge 15 luglio 1877"*

29 May – 4 Jun 1877 Senate debate opened on May 29.The law was approved on June 4 with some modifications, but the implementation thresholds remained unchanged. Final vote: 66 in favor, 10 against. *Source: Parliamentary Acts, Chamber of Senators, Session 1876–77, Discussions.*

9 Jun 1877 The Chamber of Deputies discusses and approves the final version of the law, which was passed with 178 votes in favor and 15 against. *Source: Parliamentary Acts, Chamber of Deputies, Session 1876–77, Discussions.*

15 Jul 1877 Royal promulgation of the Coppino Law on compulsory elementary education.

3 Aug 1877 Minister Coppino issues Ministerial Directive N. 526, addressed to the prefects in their capacity as presidents of the Provincial School Councils. The directive outlined the final steps to undertake before the start of the school year in order to implement the new law. *Source: "Sull'obbligo della istruzione elementare nel Regno d'Italia: attuazione della legge 15 luglio 1877"*

Appendix Tables

Table A1: Descriptive statistics

Panel A: Education pre-covariates	Mean	Std. dev.	Obs.
Schools per 1,000 inhabitants s.y 1862/1863	0.737	0.604	976
Expenditure per student s.y 1862/1863	14.60	9.481	976
Student teacher ratio s.y 1862/1863	41.32	18.33	976
Panel B: Geographical covariates			
Altitude	261.1	245.4	1097
Temperature	13.81	2.038	1097
Precipitation	778.4	238.4	1097
Panel C: Other covariates			
Population growth 1861-1871	0.0665	0.0976	976
Teachers per 1,000 inhabitants s.y 1862/1863	0.756	0.597	976
Employment industry per 1,000 inhabitants (1878)	5.791	25.26	1097
Patents per 10,000 inhabitants (1863-1875)	0.418	1.584	1097
Patents per 10,000 inhabitants (1878-1883)	0.294	0.971	1097

Note: Panel A and B show the descriptive statistics of our control variables. Panel C shows the descriptive statistics of other variables used in the analysis. The source for these data are provided in Section 3.

Table A2: Balancing of pre-determined covariates

	(1)	(2)	(3)
Panel A	**Altitude**	**Temperature**	**Precipitation**
SRD Estimate	18.20	0.105	47.15
	(55.87)	(0.424)	(60.04)
Robust CI	[-114.938 ; 146.570]	[-0.738 ; 1.193]	[-83.796 ; 196.480]
Robust p-value	0.813	0.644	0.431
Bandwidth	0.183	0.195	0.197
Observations	288	313	319
Mean Outcome	234.3	14.25	786.4
Panel B	**Schools per capita**	**Expenditure per student**	**Student-teacher ratio**
SRD Estimate	-0.00191	-1.667	5.585
	(0.0804)	(2.780)	(5.460)
Robust CI	[-0.197 ; 0.175]	[-7.455 ; 5.21]	[-5.207 ; 19.705]
Robust p-value	0.907	0.728	0.254
Bandwidth	0.173	0.144	0.148
Observations	243	192	201
Mean Outcome	0.570	14.88	40.34

Note: Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Balancing of additional pre-treatment characteristics

	(1)	(2)	(3)	(4)
	Population growth	Teachers per capita 1863	Employment ind. 1878	Patents 1863-75
SRD Estimate	-0.0137	-0.0316	-0.323	-0.0315
	(0.0222)	(0.0799)	(3.457)	(0.255)
Robust CI	[-0.069 ; 0.032]	[-0.227 ; 0.144]	[-8.616 ; 7.853]	[-0.639 ; 0.493]
Robust p-value	0.473	0.664	0.928	0.801
Bandwidth	0.227	0.176	0.184	0.253
Observations	329	250	290	404
Mean Outcome	0.0609	0.587	6.505	0.420

Note: Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Compulsory schooling and patenting

	(1)	(2)	(3)	(4)
Panel A: Fuzzy RDD				
FRD Estimate	2.312***	2.323***	1.514***	1.540**
	(0.582)	(0.763)	(0.528)	(0.610)
Robust CI	[1.179 ; 3.701]	[0.746 ; 4.046]	[0.379 ; 2.715]	[0.342 ; 2.990]
Robust p-value	0.000148	0.00442	0.00943	0.0136
Bandwidth	0.146	0.127	0.224	0.194
Observations(left)	135	116	199	175
Observations(right)	370	321	295	228
Mean Outcome	1.263	1.359	1.144	1.122
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
‘”Pre-unification States FE”’	No	No	No	Yes
Panel B: Sharp RDD (compliers only)				
SRD Estimate	1.288***	1.120***	1.155***	1.048**
	(0.395)	(0.397)	(0.403)	(0.453)
Robust CI	[0.470 ; 2.188]	[0.218 ; 2.001]	[0.181 ; 1.971]	[0.071 ; 2.055]
Robust p-value	0.00242	0.0147	0.0185	0.0358
Bandwidth(left)	0.237	0.224	0.206	0.174
Bandwidth(right)	0.398	0.574	0.665	0.444
Observations(left)	211	202	170	141
Observations(right)	246	322	291	224
Mean Outcome	1.098	1.216	1.181	1.151
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes
Panel C: Sharp RDD (near-cutoff treatment redefinition)				
SRD Estimate	1.031***	0.922**	0.874**	1.019**
	(0.352)	(0.384)	(0.393)	(0.411)
Robust CI	[0.199 ; 1.700]	[0.035 ; 1.730]	[-0.065 ; 1.676]	[0.199 ; 1.989]
Robust p-value	0.0131	0.0413	0.0696	0.0165
Bandwidth(left)	0.215	0.220	0.216	0.188
Bandwidth(right)	0.372	0.504	0.585	0.378
Observations(left)	199	206	184	159
Observations(right)	239	307	282	204
Mean Outcome	1.101	1.183	1.178	1.079
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: Fuzzy regression discontinuity estimates in Panel A. Sharp regression discontinuity estimates in Panel B and C. The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: RD estimates with alternative kernel and polynomial order

	(1)	(2)	(3)	(4)
Panel A: Triangular kernel				
SRD Estimate	1.017***	1.171***	1.001***	0.874**
	(0.337)	(0.353)	(0.386)	(0.392)
Robust CI	[0.225 ; 1.677]	[0.396 ; 1.944]	[0.140 ; 1.824]	[-0.051 ; 1.649]
Robust p-value	0.0103	0.00306	0.0223	0.0654
Bandwidth(left)	0.204	0.210	0.221	0.226
Bandwidth(right)	0.445	0.588	0.591	0.514
Observations(left)	199	203	198	201
Observations(right)	267	330	275	253
Mean Outcome	1.147	1.217	1.164	1.132
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes
Panel B: Second order polynomial				
SRD Estimate	0.901**	1.176***	1.036**	0.852*
	(0.393)	(0.423)	(0.457)	(0.449)
Robust CI	[0.041 ; 1.678]	[0.274 ; 2.049]	[0.087 ; 2.003]	[-0.116 ; 1.758]
Robust p-value	0.0395	0.0103	0.0325	0.0859
Bandwidth(left)	0.256	0.250	0.300	0.305
Bandwidth(right)	0.634	0.785	0.771	0.650
Observations(left)	245	242	274	278
Observations(right)	340	389	318	285
Mean Outcome	1.144	1.143	1.057	1.042
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Bandwidth Sensitivity

	(1)	(2)	(3)	(4)
Panel A: Symmetric bandwidth (mserd)				
SRD Estimate	0.716*	0.919**	0.783*	0.355
	(0.388)	(0.384)	(0.407)	(0.469)
Robust CI	[-0.192 ; 1.533]	[0.051 ; 1.719]	[-0.106 ; 1.669]	[-0.842 ; 1.278]
Robust p-value	0.127	0.0376	0.0844	0.687
Bandwidth	0.244	0.332	0.311	0.230
Observations(left)	234	335	288	206
Observations(right)	156	205	166	128
Mean Outcome	0.998	0.937	0.882	0.978
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes
Panel B: CER-optimal (certwo)				
SRD Estimate	0.861**	1.098***	0.981**	0.900*
	(0.377)	(0.398)	(0.453)	(0.465)
Robust CI	[0.115 ; 1.662]	[0.277 ; 1.925]	[0.008 ; 1.902]	[-0.052 ; 1.858]
Robust p-value	0.0244	0.00881	0.0482	0.0638
Bandwidth(left)	0.144	0.161	0.159	0.137
Bandwidth(right)	0.269	0.341	0.495	0.321
Observations(left)	134	152	137	112
Observations(right)	169	210	249	169
Mean Outcome	1.159	1.185	1.236	1.169
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes
Panel C: No bandwidth restriction				
SRD Estimate	1.144***	0.992***	0.953***	0.837***
	(0.214)	(0.213)	(0.236)	(0.230)
Robust CI	[0.772 ; 1.906]	[0.724 ; 1.851]	[0.583 ; 1.826]	[0.566 ; 1.789]
Robust p-value	0.00000369	0.00000760	0.000146	0.000160
Bandwidth(left)	0.800	0.800	0.800	0.800
Bandwidth(right)	3.200	3.200	3.200	3.200
Observations(left)	555	555	528	528
Observations(right)	542	542	448	448
Mean Outcome	0.889	0.889	0.863	0.863
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Compulsory schooling and patenting across all municipalities

	(1)	(2)	(3)	(4)
SRD Estimate	0.324***	0.259***	0.277**	0.201*
	(0.102)	(0.0992)	(0.111)	(0.111)
Robust CI	[0.134 ; 0.587]	[0.073 ; 0.514]	[0.053 ; 0.553]	[-0.005 ; 0.496]
Robust p-value	0.00179	0.00902	0.0176	0.0545
Bandwidth(left)	0.209	0.209	0.207	0.194
Bandwidth(right)	1.246	1.901	1.539	1.146
Observations(left)	754	754	677	643
Observations(right)	4093	5154	3817	3140
Mean Outcome	0.459	0.444	0.455	0.478
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Count data models

	Zero-inflated NB				Zero-inflated Poisson			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SRD Estimate	3.463***	3.204***	1.763**	1.420*	2.526***	2.967***	2.056*	2.454**
	(1.184)	(0.960)	(0.887)	(0.778)	(0.928)	(1.010)	(1.104)	(0.988)
Geographical Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes	No	No	No	Yes
Observations	436	509	494	403	436	509	494	403
Mean of outcome	11.02	9.770	9.093	8.928	11.02	9.770	9.093	8.928

Note: The dependent variable is the absolute number of patents. In columns 1-4 we estimate zero-inflated negative binomial models. In columns 5-8 we estimate zero-inflated Poisson models. To account for potential overdispersion and excess zeros in patent counts, we include population in 1911 as an inflate variable to predict the structural zeros in the data. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Winsorized outcome

	(1)	(2)	(3)	(4)
SRD Estimate	0.771^{***}	0.782^{***}	0.806^{***}	0.753^{**}
	(0.299)	(0.285)	(0.300)	(0.323)
Robust CI	[0.084 ; 1.387]	[0.164 ; 1.430]	[0.097 ; 1.440]	[-0.016 ; 1.417]
Robust p-value	0.0270	0.0136	0.0248	0.0553
Bandwidth(left)	0.205	0.229	0.224	0.194
Bandwidth(right)	0.348	0.433	0.491	0.390
Observations(left)	199	221	199	175
Observations(right)	215	264	247	202
Mean Outcome	0.985	1.009	0.996	0.970
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants, winsorized at the 1st and 99th percentile. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: RD estimates excluding the Kingdom of the Two Sicilies

	(1)	(2)	(3)	(4)
SRD Estimate	1.800^{***}	1.970^{***}	1.580^{***}	1.599^{**}
	(0.541)	(0.555)	(0.603)	(0.716)
Robust CI	[0.597 ; 3.057]	[0.857 ; 3.314]	[0.257 ; 3.009]	[0.087 ; 3.334]
Robust p-value	0.00361	0.000876	0.0200	0.0389
Bandwidth(left)	0.178	0.218	0.230	0.170
Bandwidth(right)	0.629	0.647	0.816	0.617
Observations(left)	71	82	72	55
Observations(right)	266	271	251	205
Mean Outcome	1.669	1.639	1.657	1.772
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: RD estimate excluding municipalities with universities

	(1)	(2)	(3)	(4)
SRD Estimate	1.018**	1.103***	1.171***	0.988**
	(0.406)	(0.389)	(0.413)	(0.440)
Robust CI	[0.074 ; 1.816]	[0.153 ; 1.877]	[0.314 ; 2.107]	[-0.038 ; 1.863]
Robust p-value	0.0334	0.0210	0.00814	0.0600
Bandwidth(left)	0.218	0.216	0.200	0.171
Bandwidth(right)	0.481	0.621	0.698	0.518
Observations(left)	207	205	173	149
Observations(right)	277	324	285	244
Mean Outcome	1.029	1.056	1.058	1.070
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of patents per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Compulsory schooling and trademarks

	(1)	(2)	(3)	(4)
SRD Estimate	0.785***	0.845***	0.798***	0.740***
	(0.238)	(0.238)	(0.264)	(0.260)
Robust CI	[0.292 ; 1.299]	[0.323 ; 1.338]	[0.229 ; 1.347]	[0.214 ; 1.310]
Robust p-value	0.00197	0.00134	0.00571	0.00645
Bandwidth(left)	0.0904	0.0933	0.0959	0.0947
Bandwidth(right)	0.397	0.486	0.493	0.392
Observations(left)	72	75	69	68
Observations(right)	247	292	247	203
Mean Outcome	0.631	0.626	0.620	0.603
Geographical Controls	No	Yes	Yes	Yes
Schooling Controls	No	No	Yes	Yes
Pre-unification States FE	No	No	No	Yes

Note: The dependent variable is number of trademarks per 10,000 inhabitants. Geographical controls include altitude, temperature and precipitation. Schooling controls include schools per capita, expenditure per student, and the student-teacher ratio. Conventional standard errors are reported in parenthesis. Robust CI and robust *p-value* refer to confidence intervals and *p-value* under robust inference as described in Cattaneo et al. (2020a). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Figures

Figure A.1: Ministerial Directive 30 April 1877

74 75

A. **Anno 1876-77. Provincia di**

Numero d'ordine	COMUNE	POPOLAZIONE		TOTALE						SCUOLE da istituire	SEDE della nuova scuola	INSEGNANTI da nominarsi	*Osservazioni*
		dell' intero Comune	che può usufruire delle scuole esistenti	degli In-segnanti attuali	nelle classi superiori		inferiori						
					maschili	femminili	maschili	femminili	miste				
1	2	3	4	5	6	7	[illegible]	9	10	11	12	13	14

Note: The figure shows the data that the Ministry of Education required local authorities to collect in order to assess in which municipalities the law on compulsory schooling could be enforced. Source: "Sull'obbligo della istruzione elementare nel regno d'Italia: attuazione della legge 15 luglio 1877".

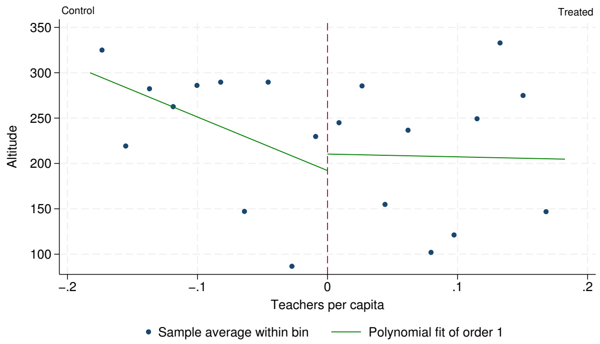
Figure A.2: Example of the data on the implementation of the Coppino Law

Numero d'ordine	COMUNE	Popolazione dell'intero comune	Popolazione che non può usufruire delle scuole esistenti	Totale degli insegnanti	Totale – Nelle classi inferiori	Totale – Nelle classi superiori	Insegnanti da nominarsi	Comuni nei quali è da proclamarsi l'obbligo scolastico	Comuni nei quali non è da proclamarsi l'obbligo scolastico
1	Gerace	7257	»	8	7	1	»	1	»
2	Agnana.	1195	»	2	2	»	»	1	»
3	Antonimina	1642	»	1	1	»	1	»	1
4	Ardore	5141	2669	4	3	1	2	»	1
5	Benestare	3173	»	3	3	»	1	»	1
6	Bianconovo.	1931	»	3	3	»	»	1	»
7	Bivongi.	2506	»	2	2	»	1	»	1
8	Bovalino	2644	»	4	4	»	»	1	»
9	Brancaleone	1323	»	2	2	»	»	1	»
10	Bruzzano	1409	558	2	2	»	»	1	»
11	Camini	1001	»	2	2	»	»	1	»
12	Canolo	2977	»	2	2	»	1	»	1
13	Caraffa	946	»	2	2	»	»	1	»
14	Careri	1095	»	3	3	»	»	1	»
15	Casignana	1139	»	2	2	»	»	1	»
16	Caulonia	10125	»	7	6	1	3	»	1
17	Ciminà	1845	»	2	2	»	»	1	»
18	Ferruzzano	1262	»	2	2	»	»	1	»
19	Gioiosa Jonica	8488	»	4	3	1	4	»	1
20	Grotteria.	5223	»	4	3	1	2	»	1
21	Mammola.	7804	»	5	4	1	3	»	1
22	Martone	1740	»	1	1	»	1	»	1
23	Monasterace	1174	»	2	2	»	»	1	»
24	Palizzi	2087	»	2	2	»	»	1	»
25	Pazzano	1457	»	2	2	»	»	1	»
26	Placanica.	1439	»	2	2	»	»	1	»
27	Plati	2368	»	2	2	»	1	»	1
28	Portigliola	1611	»	2	2	»	»	1	»
29	Precacore	585	»	2	2	»	»	1	»
30	Riace	1577	»	2	2	»	»	1	»

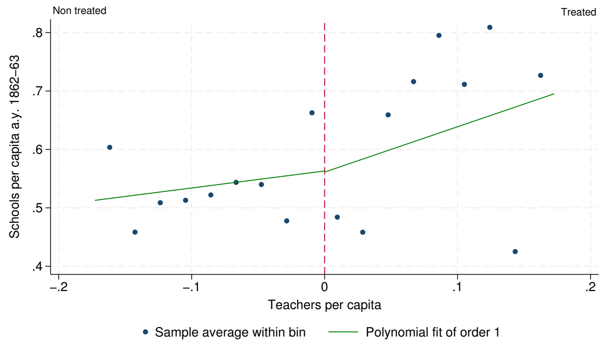
Note: The figure shows an example of our data on the implementation of the Coppino Law. Source: "Sull'obbligo della istruzione elementare nel regno d'Italia: attuazione della legge 15 luglio 1877".

Figure A.3: Balancing of pre-determined covariates

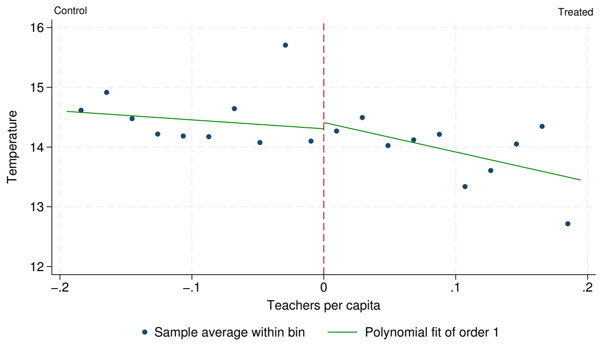
(a) Altitude



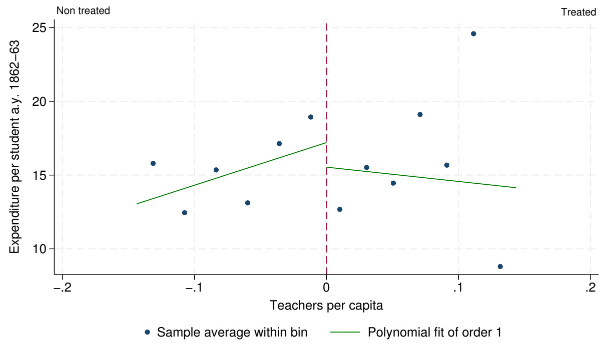
(d) Schools per capita



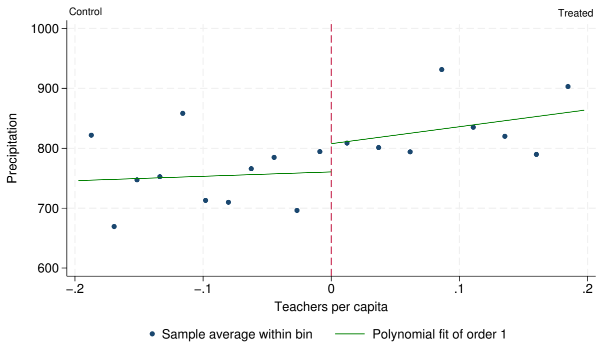
(b) Temperature



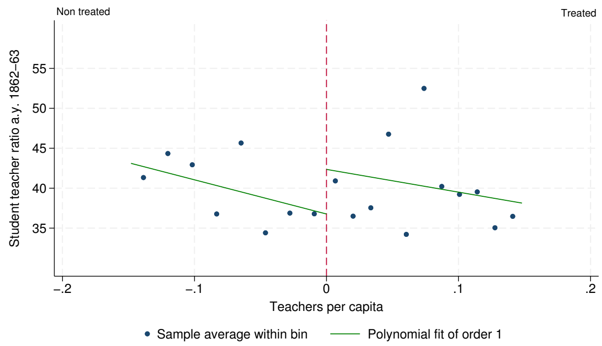
(e) Expenditure per student



(c) Precipitation



(f) Student-teacher ratio



Note: The figures shows the regression discontinuity plots for the estimates reported in Table A2.

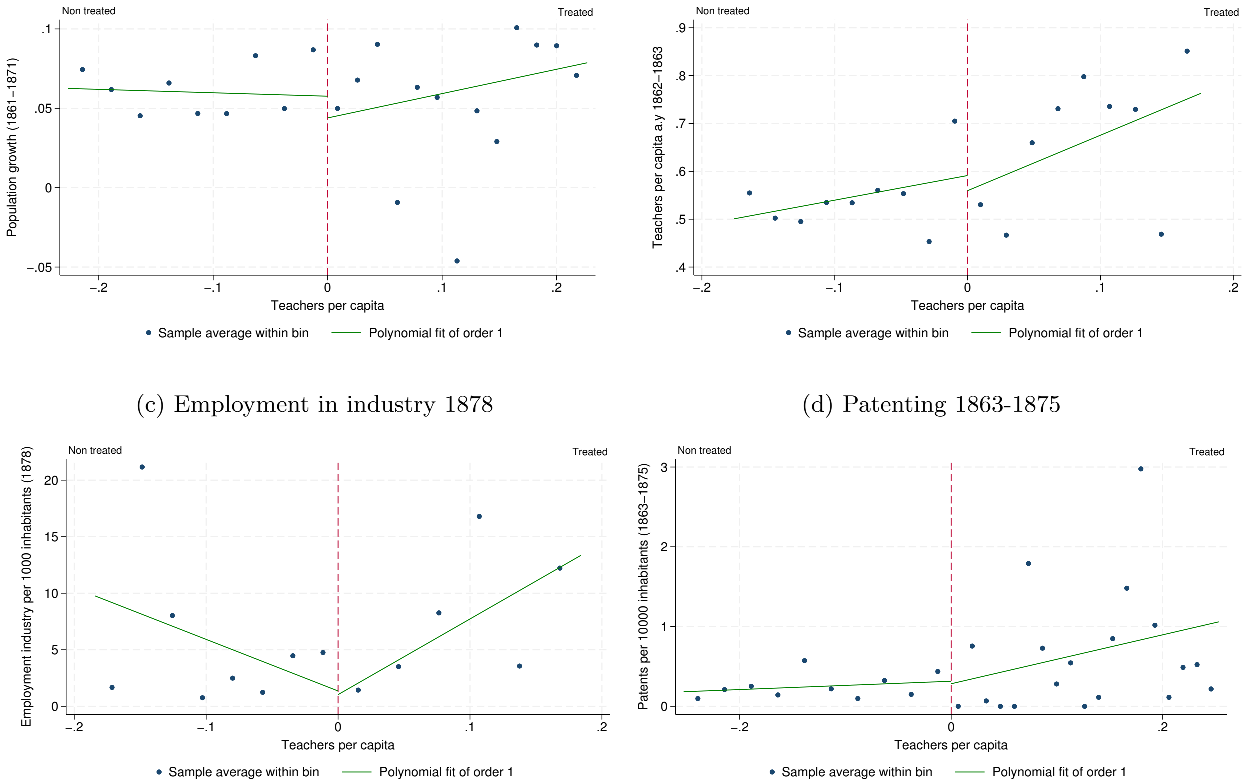
Figure A.4: Balancing of additional pre-treatment characteristics

(a) Population growth

(b) Teacher per capita 1863

(c) Employment in industry 1878

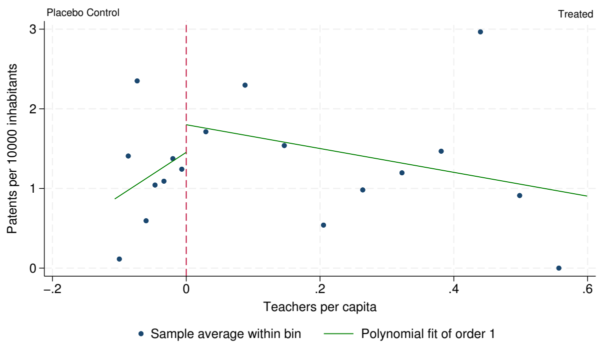
(d) Patenting 1863-1875



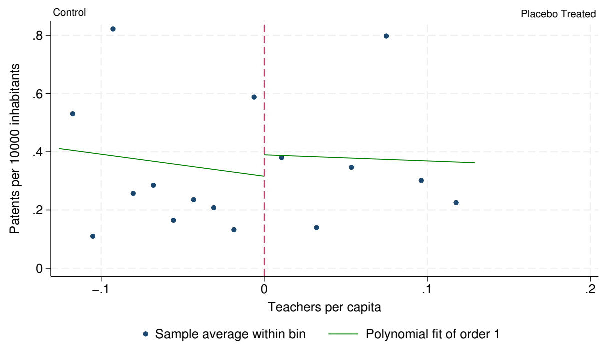
Note: The figures shows the regression discontinuity plots for the estimates reported in Table A3.

Figure A.5: Regression discontinuity plots at artificial (placebo) cutoffs

(a)



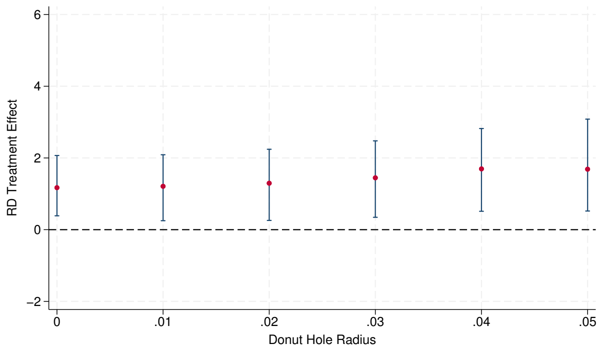
(b)



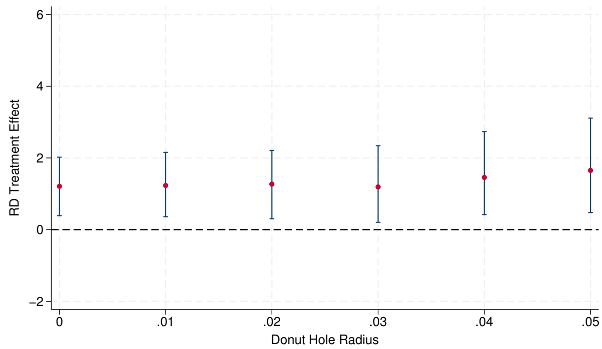
Note: The figure shows the regression discontinuity plots for estimates at placebo cutoffs. The green lines show the linear polynomial fit for the regression discontinuity estimate with no controls and employing a uniform kernel. The dots. show the observed average values within bin. Plots obtained using rdplot command from Cattaneo et al. (2020a)

Figure A.6: Donut-Hole Approach

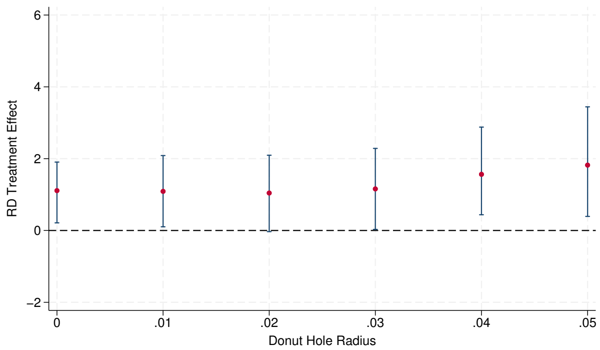
(a) SRD with no controls



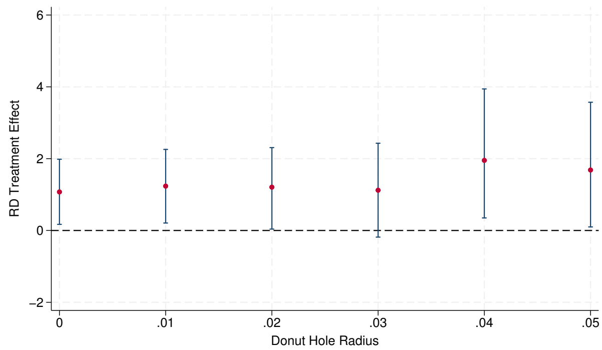
(b) SRD including geographical controls



(c) SRD including geographical controls and pre-existing conditions in education



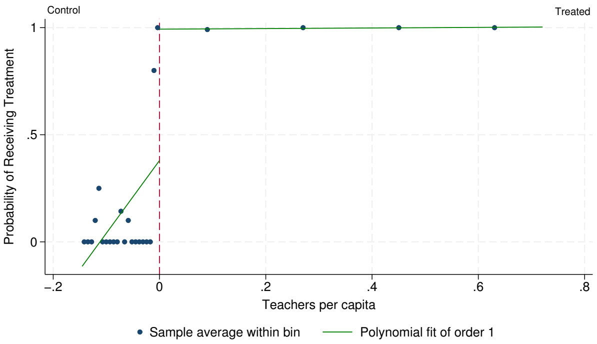
(d) SRD including geographical controls, pre-existing conditions in education and pre-unification States FE



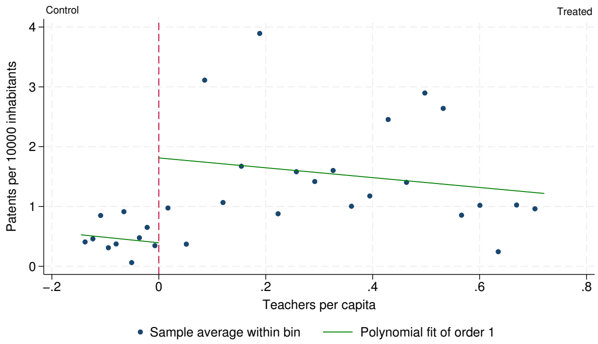
Note: The figure shows the regression discontinuity estimates and robust confidence intervals for the donut-hole approach. Each panel displays, for reference, the baseline RD estimate and robust confidence intervals.

Figure A.7: Regression discontinuity plots Fuzzy RDD

(a) First Stage (Fuzzy RDD)



(b) Reduced Form (Fuzzy RDD)



Note: Panel (a) displays the conditional probability of receiving the treatment, while panel (b) displays the regression discontinuity plot of patents per 10,000 inhabitants for the fuzzy RD design. The green lines show the linear polynomial fit for the regression discontinuity estimate with no controls and employing a uniform kernel. The dots show the observed average values within bin. Plots obtained using rdplot command from Cattaneo et al. (2020a).