The Logic of State Surveillance

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Abstract

All states adopt systems to surveil political activists. How do they decide whom to watch and why? We study the logic of state surveillance using the first complete individual-level database of those monitored by a state – 152,000 Italians born between 1816 and 1932, encompassing both democratic and authoritarian regimes. We focus on education: exploiting a discontinuous expansion in primary schooling in municipalities above a population threshold, we show that cohorts exposed to more years of school experienced an uptick in surveillance. The effect is largest for working classes, whom were monitored for longer periods, subjected to harsher measures, and disproportionately targeted when affiliated with communist ideologies. Yet treated cohorts did not become more politically active, suggesting that surveillance rose not in response to actual mobilization, but due to the state fearing working class empowerment. These findings reveal how states prioritize surveilling educated individuals who combine mobilization potential with revolutionary grievances, potentially generating inequalities in the capacity of different groups to influence political change.

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"For twenty years, we must stop this brain from functioning." – Michele Ingrano', Prosecutor in the trial against Antonio Gramsci, 1928.

1 Introduction

States worldwide use systems of surveillance to monitor the political activity of their citizens. In China, a growing apparatus follows activists' daily activities (Qin, Strömberg and Wu, 2017), and in the United States the government monitors immigrants' and students' political behavior (The Guardian, June 18 2025). While surveillance might help maintain security, it may also infringe on individual rights and be used to suppress democratic movements (Zhuravskaya, Petrova and Enikolopov, 2020; Andirin et al., 2022). A recent literature has examined the consequences of surveillance, studying how it impacts dissent and trust (Xu, 2021; Hager and Krakowski, 2022; Beraja et al., 2023; Lichter, Löffler and Siegloch, 2021).

Instead, the determinants of surveillance – which types of people states choose to watch and why – remain largely unexplored, partly due to states' reluctance to share intelligence data on those they track. Understanding the logic of state surveillance is important to learn about a fundamental strategy of state control and to understand the direction of political change: if certain groups face more scrutiny, the type of change they advocate for might be less likely to take place.

In this paper, we study states' strategies of surveillance using the first dataset ever assembled on whom a state watched: we scrape newly declassified data with biographical details on individuals surveilled by the Italian state. The data span democratic and autocratic regimes, encompass a range of political movements, from anarchism and communism to fascism, and include detailed information on the surveilled, offering a unique window to observe state choices across contexts, including, unlike previous work, in democratic settings.

Our theoretical point of departure is the idea that, given limited resources, states target individuals most likely to threaten their stability. We propose an hypothesis about the profile of such individuals by drawing on two strands of literature: one linking education

to greater capacity for political mobilization (Almond, Verba et al., 1963; Verba et al., 1993; Glaeser, Ponzetto and Shleifer, 2007), and another identifying subaltern groups – those socially or economically excluded – as most likely to seek radical change (Marx and Engels, 1848; Gramsci, 1975; Fanon, 1963; Gurr, 1970; Scott, 1985).[1] Historically, revolutions – from the Russian to the French, to the Haitian – have been led by subaltern groups seeking to overthrow existing regimes. Qualitatively, surveillance reports from the Italian archives frequently describe individuals from working class backgrounds – the relevant dimension of subalternity in our context – but with above-average education as subjects of concern. Building on these two literatures, we hypothesize that states prioritize surveillance of individuals who combine the tools to mobilize (via education) with the grievances to seek revolutionary change (via subalternity), as they are perceived as particularly threatening.

Offering evidence for this hypothesis is challenging. In addition to the lack of data on the identities of those monitored, identifying the causal effect of education or social status on the likelihood of being surveilled is complicated because individuals' family background, cognitive ability, or group affiliations may affect both their social and educational attainment and their likelihood of being monitored.

We address the endogeneity challenge by taking advantage of an exogenous shock to education: a reform assigning an additional two years of primary schooling – third and fourth grade – to pupils in municipalities with population above 4,000. Because other laws also generate discontinuities at the same threshold, we add a temporal dimension: cohorts are treated only if they had not yet completed second grade when the reform was implemented. Treatment is therefore determined by birth year, rather than by the calendar

[1]We use "subaltern" following Gramsci, who defined them as social groups beneath the hegemonic power structure and deprived of the means to voice or exit their oppression. While he primarily refers to class, Gramsci's concept extends to other marginalized identities such as ethnicity, culture, or religion (Gramsci, 1975, pp. 2286–2288). In other settings, as during the Chinese Cultural Revolution, landowners and other elites were the subaltern ("black") classes (Yongyi et al., 2011).

year of enactment or the timing of subsequent monitoring. Our identification combines two sources of exogenous variation in a differences-in-discontinuities (DDRD) framework (Grembi, Nannicini and Troiano, 2016), comparing cohorts that completed second grade before versus after the reform in municipalities just below versus above the population cutoff.

We first document that the reform was effective: using newly collected data on literacy and a cross-sectional regression discontinuity design, we show that municipalities affected by the additional schooling experienced lower illiteracy rates, consistent with historical accounts that credit the reform with expanding education.

Our main result considers whether more education attracts state scrutiny. In municipalities and cohorts exposed to two additional years of schooling, there were 3.5 more individuals surveilled per 100,000 inhabitants, corresponding to a 64% increase from a 5.5 baseline. This effect was absent in pre-reform cohorts, consistent with the parallel-trends assumption. In addition, when a later reform removed the 4,000-inhabitant education discontinuity and standardized the law across municipalities, the effect disappeared, providing additional evidence that the schooling shock – not other factors – drove the increase in surveillance. The result holds across specifications, and is absent at placebo cutoffs and cohorts.

We next ask who bore the brunt of the additional surveillance. Using each surveilled person's profession, we assign them to one of three social classes – working, middle, or upper – and create three corresponding outcomes that count surveillance in each class. We find that the post-reform increase was driven by surveillance of the working class. Because the reform likely reduced the working class's share of the population, this figure may be a lower bound on the effect of the reform on this group. Consistent with education triggering state fear of working class empowerment, treated cohorts, and particularly working class individuals, were more likely to be monitored for longer periods, subjected to harsher forms of control, and have more information collected on them. Also, the education reform led to higher surveillance of working classes with socialist and communist affiliations – ideologies tied to the demands of working classes.

This logic of surveillance is similar across regimes and settings: leveraging our extended period, we show that both democratic and autocratic regimes monitored educated working classes at higher rates; that differences in state capacity affect the intensity but not the targets of surveillance; and that targeting patterns remain consistent after technological innovations in surveillance, underscoring the relevance of our findings in an era of mass information gathering from online sources.

Having shown that education triggers surveillance of the working classes, our third set of analyses investigates whether the increase in surveillance was driven by increased political mobilization resulting from greater access to education. In our setting, evidence suggests that this force plays a limited role. We combine existing and newly collected data to examine five measures of political activism, ranging from low to high cost actions: voter turnout in national elections, records of notable political figures (scraped from Wikidata), data on protests and strikes (scraped from historical newspapers), the universe of partisans (scraped from an online repository), and episodes of banditry documented by Lecce, Ogliari and Orlando (2022). Across measures, we find no evidence that the education reform increased political participation.

There are two possible interpretations for this null result: that in this context education did not increase political participation, or that education-triggered surveillance depressed political engagement. While we cannot provide causal estimates to discern between the two, descriptive evidence is inconsistent with the chilling effects of surveillance interpretation: we observe no differences in political engagement between areas with high and low surveillance, or among cohorts that were never surveilled. Additionally, surveillance is unlikely to be used to target non-radical and widespread forms of political engagement, such as voting in national elections, which is unaffected by education. Instead, that state-provided education did not increase political activism aligns with recent work suggesting that education may not always promote dissent at large, particularly when designed to transmit official narratives (Cantoni et al., 2017; Paglayan, 2024). We argue that it would not be irrational for the state to increase

surveillance in absence of an uptick in participation: even if education had the potential to empower only a small number of revolutionary entrepreneurs, those could generate ripple effects large enough to justify widespread preventive surveillance (De Mesquita, 2010).

Finally, we rule out alternative mechanisms operating at the municipality–cohort level that could independently trigger surveillance.[2] First, we test whether the reform produced better-educated policemen who disproportionately surveilled members of their own cohort. We do so by replicating our analysis on surveilled who migrated to other municipalities, where they would not be monitored by former classmates. The presence of effects on this sample also indicates that the treatment operates through a portable asset – human capital. Second, we replicate our analysis on non-migrants and again find consistent results. This rules out migration as the main driver and addresses the possibility that education-induced moves to larger, more activist cities could explain the findings. Third, we test whether an increase in the number of state officials (teachers) acting as informants could explain the findings. Using a later reform that discontinuously increased the number of state officials in another set of municipalities, without affecting education, we find no associated rise in surveillance. Finally, we show that teachers were not targeted at higher rates or in greater numbers post-reform, making it unlikely that state concern over their potential role as political instigators explains our results. Other alternative explanations are discussed in Section 8.

This study builds on several strands of research. A literature across social sciences has examined how states repress political dissent focusing on overt forms of repression, such as arrests, killings, and censorship (e.g., Besley and Persson (2009); Enikolopov, Petrova and Zhuravskaya (2011); King, Pan and Roberts (2013); Hassan, Mattingly and Nugent (2022); Wen (2025)). A smaller and more recent literature has turned to examine *covert* repression – surveillance. Surveillance differs fundamentally from overt repression: instead of being a public, performative deterrent, explicitly silencing dissent, it is a covert, information-

[2]Changes induced by the reform that only affect municipalities discontinuously, but not cohorts, do not constitute alternative explanations, since our strategy compares individuals within the same city, but across different cohorts.

gathering exercise of state capacity that sidesteps reputational costs, but that demands substantial bureaucratic capacity. Therefore, limited resources are the main constraint for covert action. While previous work on surveillance has focused on its consequences, this paper shifts the focus to its determinants, investigating the strategic factors shaping whom the state chooses to monitor. Before this, Davenport (2005)'s seminal study offered correlational evidence that black neighborhoods were targeted at higher rates in the US. We exploit more than a century of archival data spanning multiple regimes and ideologies to show that states target groups most capable of undermining regime stability – newly educated members of subaltern groups.[3]

This paper also relates to a foundational area of inquiry for social science, studying who participates in politics (Almond, Verba et al., 1963; Glaeser, Ponzetto and Shleifer, 2007) and the conditions under which revolutions can take place (Kuran, 1989; Acemoglu and Robinson, 2001; Bueno de Mesquita and Smith, 2017). Studies have identified subalternity – defined by class, income, race, religion, or other factors (Marx and Engels, 1848; Gramsci, 1975; Fanon, 1963; Gurr, 1970; Scott, 1985) – as a key predictor of revolutions. We build on this tradition of inquiry to argue that states are particularly concerned with individuals having both high mobilization potential (through education) and high propensity for revolt (through grievances induced by subalternity).

By linking education to surveillance, our study also speaks to research on education and state control. Studies show that state-led education can foster a range of political reactions, from greater participation (Sondheimer and Green, 2010; Marshall, 2016), to greater obedience (Cantoni et al., 2017; Paglayan, 2024). Our findings indicate that it can also spark a reaction from the state: fear of unrest. In this sense, this study offers a nuanced picture of states' trade-offs in offering schooling, as in (Bourguignon and Verdier, 2000).

Our findings have significant implications for the understanding of state strategic choices and of political change. They reveal an important pattern: as subaltern groups gain

[3]Section A discusses this literature and our theoretical framework in more detail.

education and political voice, they also face disproportionate surveillance compared to their equally mobilized but higher-status counterparts. This asymmetry can undermine political agency, potentially muting the demands of society's most vulnerable members, precisely when their political participation increases.

2 Origins, Institutions, and Targets of Surveillance

2.1 The Surveillance System in Italy

Historical Evolution: State surveillance in Italy traces its origins to the Bourbon and Piedmont-Sardinia kingdoms, both of which adopted Napoleonic-style centralized political police. Following the uprisings of 1820–21 and 1848, these regimes created their first secret services, relying on spies, informants, and the monitoring of public spaces (Di Fiore, 2018). After unification, the Kingdom of Italy (1861–1946) inherited and expanded these traditions. The rise of socialist and anarchist movements in the late nineteenth century prompted the Interior Ministry to intensify surveillance and, in 1894, establish a national suspect registry by consolidating local files dating back to before unification (Tosatti, 1990, pp. 447–49). World War I further expanded state capacity through the introduction of wiretapping technologies. Under Mussolini, surveillance became more centralized and pervasive. Drawing inspiration from the Soviet Cheka, the regime created the OVRA (Organization for the Vigilance and Repression of Anti-Fascism) in 1926, expanding the use of informants and spies to monitor political dissidents (Leto, 1951, p.46) and sharply increasing the number of individuals placed under surveillance each year.

Institutional Architecture: Qualitative archival work, such as Dilemmi (2010)'s study of Verona's surveillance archives, sheds light on how individuals came under state scrutiny. Surveillance was most often triggered by participation in political parties, unions, or opposition movements, involvement in protests or strikes or subscribing to critical publications (Dilemmi,

2010, p. 215). In the Verona archives, nearly half of all files originated from the local Police Headquarters (43%). Others were initiated by higher institutional levels, including the central Ministry of Interior (18%) and the provincial Carabinieri (18%) (Dilemmi, 2010, p.206). Information on political activists also came, at times, from informants – often prisoners who reported on cellmates in exchange for reduced sentences (Ciconte, 2019, Ch. 9). With the rise of fascism, additional actors, such as regime officials and affiliated organizations, became increasingly active in initiating surveillance cases (Dilemmi, 2010, p.258).

Individuals under state monitoring were generally unaware of being surveilled. A significant share, however, were also subjected to special measures – some covert, others overt – that varied in severity. These included the *rubrica di frontiera*(monitoring movements across national borders, sometimes discreetly, 23% of cases), *diffida* (a formal warning to maintain good conduct, 5.5%), and *confino* (forced relocation, 30%). Surveillance files were closed when the individual was no longer deemed a threat. This could follow a period of inactivity, withdrawal from the activities that had raised suspicion, or broader political and social changes that shifted official priorities. Closure decisions typically reflected the belief that the person no longer endangered public order or regime stability.

2.2 Targets of Surveillance: Police Narratives

To qualitatively investigate which individual characteristics the state paid attention to, we collect a sample of verbatim police reports on the surveilled. Because no comprehensive digital archive of these narratives exists, we draw on the transcriptions by Ciancarini (2019), who documented the entirety of the dossiers from Civitavecchia (N=254), and by Carolini and Ecca (2015), who transcribed files on 1,000 antifascists. Neither set is random –their geographic focus and ideological mix differ from the broader population – but the narrative detail allows us to see which attributes surveillance officers flagged as noteworthy.

Qualitative evidence: Anecdotally, these reports often made explicit reference to both schooling and social class. For example:

> *"His profession is mechanical blacksmith [...] He enjoys a good reputation in the public. He has a bold and enterprising character. His education is above his status. Quick intelligence. Limited culture but above his status. He completed primary education."*

> *"Mechanic (...). The Prefecture judged him to enjoy a good reputation, of mild character, good manners, fairly intelligent, of decent education."*

When dealing with individuals openly in the opposition, the state regarded knowledge as so threatening that it sought to suppress not only its dissemination but also its production. This was the case for anti-Fascist politician Riccardo Bauer, who was kept under surveillance and, once imprisoned, was forbidden not only to write but even to read (AANPIA, Riccardo Bauer records). The regime's fear of knowledge generation is perhaps best captured in the case of one of the most prominent figures in our data, Communist Party founder Antonio Gramsci, at whose trial the prosecutor declared:

> *"For twenty years we must stop this brain from functioning."* [4]

Together with education, the state also collected information on the profession of the surveilled. As evidenced in the quotes and described in the data section, profession was recorded for 100% of the surveilled, highlighting how this aspect of individual identity was fundamental for the state, as important as documenting the ideology of the dissidents – another key piece of information systematically noted for each subject.

LLM classification: We analyze these narratives more systematically using a machine-learning pipeline. We start by segmenting each biography into individual sentences. We

[4] Closing speech to the Special Tribunal for the Defence of the State, Rome, 1928. In Antonio Gramsci: Pre-Prison Writings, ed. Richard Bellamy, trans. Virginia Cox, chap. XVIII, 181–183. Cambridge: Cambridge University Press, 1994.

then classify each sentence with a large language model (OpenAI GPT-4o, temperature 0 to enhance reproducibility) to select sentences that describes a personal trait, characteristic, or aspect of personality. For the sentences that do, we extract the relevant descriptors and group them into broader clusters of semantic similarity. Finally, we assign human-readable labels to each cluster by prompting a large language model to generate concise descriptions (OpenAI GPT-4o, temperature 0, to help generate concise yet natural descriptions, while also remaining replicable). We include more details about this process in Section C.1.

We present the ten most frequently mentioned individual traits in Table 1a, and report the full list in appendix (Table C.1). As expected, the police always noted information on the ideology and political activity of the surveilled, as well as physical and behavioral traits helpful for identification and descriptive purposes. They also emphasized individuals' leadership and communication skills, highlighting the concern of the state for the mobilization capacity of the surveilled. Education follows as the next most frequently mentioned trait, followed by moral and socioeconomic characteristics.

Both qualitative readings of the narratives and unsupervised text analysis highlight the importance of education and status in state discourse about surveillance targets, reflecting how authorities categorized and understood those they monitored.

3 Data

We rely on a range of historical, census, and administrative data partly collected for this project. Below, we outline the key outcomes and regressors. In Section B in the Appendix, we discuss information on additional historical data we collected and partly digitized.

Targets of Surveillance: Our primary outcome of interest is the identity and frequency of individuals placed under political surveillance by the state. To the best of our knowledge, this is the first study that brings detailed individual-level data on this dimension. We scrape newly released data on 152,434 individuals surveilled by the Italian State from the *Italian*

Central Political Surveillance Office (Casellario Politico Centrale), an archive maintained under the authority of the Ministry of Interior. Each entry lists the individual's name, birth date, birthplace, residence, political ideology, occupation, and years under surveillance (Figure C.1 shows an example of a record). After linking with the first population Census of 1861 and excluding individuals born outside of Italy, we obtain a sample of 102,535 individuals born from 1816 to 1929, and surveilled from 1869 to 1947. Finally, our empirical strategy leverages a discontinuity ending for those born after 1896, so the final analysis sample includes 64,915 individuals born across 80 cohorts in all 7,713 Italian municipalities in 1861, for a total of 617,040 observations.

In Figure 1b, we plot the number of surveilled in our study period by their date of birth – the time dimension used in our analyses. The number of individuals surveilled expanded steadily across cohorts, reflecting the state's growing capacity to monitor its population. Even for the earliest cohorts, where the curve appears flatter, the number of surveilled grows exponentially. Figure C.2 presents spatial variation in the number of individuals surveilled in 100,000 inhabitants during the period under study. At least one individual was surveilled in 85% of the municipalities, with a larger concentration of surveilled in Central Italy, an area that saw intense socialist activity. We also plot the number of surveilled by date of surveillance in Appendix (Figure C.3a), and the full distribution of surveilled by date of birth, including those outside our study period, in Figure C.3b.

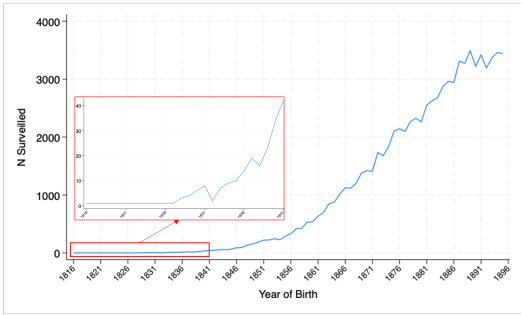
Class: The archive contains information on the profession of each individual surveilled, which we use to categorize subjects into upper, middle, and working class. Because the social meaning of professions changed over time (some occupations slid up or down the status ladder), we adopt the historical scheme developed by Labini (1972) for the 1881 census, which maps occupations to social classes as understood at the time. We then adopt an iterative process for accurate classification. First, we prompt a large-language-model classifier (OpenAI GPT-4o, temperature 0.3) with Labini's descriptions to tag every recorded

Figure 1: Descriptive statistics on the surveilled

(a) Police narratives: Individual traits

Cluster	N Biographies	Share
Ideological Traits	1036	82
Character Traits	661	53
Behavioral Traits	486	39
Leadership and Communication Traits	207	16
Educational Traits	136	11
Moral Traits	115	9
Socioeconomic Traits	96	8
Political Traits	88	7
Physical Traits	56	4
Religious Traits	54	4

(b) Number of Surveilled by birth date



Notes: (a) Reports the 10 most common individual traits of the surveilled highlighted in the 1,254 police-narratives identified through unsupervised ML. Full list in Table C.1. *(b)* Number of surveilled by date of birth in our sample. We zoom on earlier birth cohorts to reveal an exponential increase that later high counts otherwise flatten on the y-axis. Other descriptive figures are presented in Section C.

occupation. Second, we read a sample of classified data, correct classes attributed mistakenly, and feed the corrected list back into the classifier. More details about this process are included in Section B, SI. Figures C.4 and C.5 report, respectively, the most common jobs in each class and the prevalence of each class among surveilled, highlighting that working class individuals are the most surveilled on average.

We also use information in the files to code other characteristics of the surveilled: about half of those surveilled were communists and socialists (52%), followed by anti-Fascists (23%), anarchists (18%), and other ideologies (7%, Figure C.5). About 3% of the surveilled were women, their number continued increasing over time faster than for men (Figure C.6), and most of them were housewives (28%) and tailors (5%).

Measures of Political Engagement: A secondary outcome of interest in our study is the degree of political engagement in the population. We collect five different indicators of political activity, ranging from the least to the most costly actions. First, we gather data on the number of votes cast in each municipality and in each election from 1861 to 1913 from the *Electoral Historical Atlas* archive.

Second, we scrape data on all notable individuals born in Italy in our sample window

from WikiData, including information on their birth date and place, occupation, and other significant roles held. Using occupation and significant roles, we identify notable individuals who were involved in politics, ranging from members of parliament to activists and high-level religious figures.

Third, we move towards more widespread and costlier forms of political action by scraping news items on the number of protests and strikes from the archives of one of the main Italian newspapers, *La Stampa*, the only including articles dating as far back as 1867. We identify the location of the event by writing an algorithm that searches the municipality name in the text of the article, and excluding municipalities with names having double meaning.

Our final two sources provide information on the most costly type of political action: armed resistance against the state. First, we scrape a new database including 19,341 partisans born until 1895 from Central Institute for Archives (Icar). Those were anti-fascist resistance fighters who took up arms against the Fascist regime. This dataset aggregates regional records assembled immediately after World War II to identify officially recognized partisans for compensation purposes. Because these lists were created by the state with the aim of financial compensation to families of the partisans, they are likely more accurate and conservative than typical combatant registries, listing individuals as partisans only when there was sufficient evidence to demonstrate their role. The data includes names, date and location of birth of the partisans.[5]

Finally, we obtained data on another costly form of political action: banditism. Bandits, or "briganti", were individuals who lived outside of the law and engaged in criminal acts such as raids, kidnappings, and thefts, targeting the wealthy and landowners. Their actions were

[5]It is not possible to match partisans and surveilled: partisans started their activity in the summer of 1943, after Mussolini was arrested, a temporary government was put in place in Rome, while Northern Italy was under German occupation. Files of partisans might be present in the German archives, but the Italian secret police largely halted its surveillance activity in this period, as is visible in Figure C.3a.

often a form of insurgency against the newly unified Italian state. Banditry was especially prevalent in southern Italy, and in parts of central Italy, where social inequalities were severe and many viewed bandits as "primitive rebels" rather than simple criminals (Ciconte, 2011; Hobsbawm, 1971). We use data collected by Lecce, Ogliari and Orlando (2022) on the municipal level number of bandits from 1860 to 1870.

Population and other covariates: Our treatment is defined based on population data, which we gather from the Historical Censuses made available by the Italian Institute of Statistic. Our year of reference is 1861, the first Census in Italian history.[6] We also digitize new data on the share of illiterates at the municipal level from the 1911 Census, the first including municipal level data on education. We collect and partly digitize a number of other historical data which we discuss in greater details in Section B, SI.

4 Empirical Strategy

The Education Reform: Our treatment of interest is a shock in the number of years of primary schooling generated by the first education reform of the Italian Kingdom. The Casati Law, enacted across unified Italy in 1861,[7] introduced free elementary schooling for children aged six and older and made the first two years of schooling compulsory.

Although the law applied universally, towns with more than 4,000 inhabitants—judged to have sufficient administrative and financial capacity to enforce the requirement—had to extend compulsory schooling by an additional two years, covering ages eight to ten (n. 3725/1859, art. 319–321). Consequently, children who were seven or younger at the time of enactment (e.g., born in 1854 or later) and living in larger municipalities experienced a

[6]Municipalities in province of Udine started being part of Italy only in 1871, after the start of our treatment, and are therefore excluded from the main sample and included in a robustness.

[7]The law was initially approved in 1859 in the Kingdom of Sardinia, but its implementation was delayed due to the unification war. Before this time, no laws requiring compulsory education had been implemented (Sotgiu, 1984).

positive education shock compared to their peers born just before or in smaller towns.

This discontinuity ended in 1904, when the Orlando Law extended compulsory schooling to age twelve in all municipalities regardless of population size. Our analysis therefore focuses on cohorts born before 1896.

Identification Strategy, DDRD Design: We estimate the effect of the education reform on surveillance using a difference-in-discontinuities (DDRD) design (Grembi, Nannicini and Troiano, 2016). This design combines two exogenous sources of variation: (i) a cross-sectional at the 4,000-inhabitant cutoff, above which municipalities were required to extend compulsory schooling, and (ii) a temporal discontinuity between cohorts who were seven years old or younger in 1861 (and therefore required to remain in school longer) and those who had already completed second grade at the time of the reform. Because treatment is assigned by birth year, it is unrelated to the calendar year of either the law's enactment or later surveillance activities: individuals could be monitored at any point in their adult lives.

Formally, for municipality i and cohort t we estimate:

$$\begin{aligned} Surveilled_{it} = \beta\ (Post \times Above4000_{it}) + \lambda(f(Pop1861) \times Post_{it}) \\ + \eta\ (f(Pop1861) \times Post \times Above4000_{it}) + \alpha_i + \theta_t + \kappa_{r \times t} + \epsilon_{it} \end{aligned} \tag{1}$$

where $Surveilled_{it}$ is the population share of individuals surveilled in each municipality–cohort, $Post$ is an indicator equal to 1 for cohorts born after 1854 (seven or younger in 1861), $Above4000$ is a dummy for municipalities above the threshold, and $Pop1861$ is the running variable.

All regressions include municipality fixed effects, cohort fixed effects, and reign–cohort fixed effects.[8] These absorb differences across cohorts, fixed differences in municipality characteristics, and time-varying regional variation.

[8]We use reign rather than region fixed effects because the sample begins before national unification was complete, but we present robustness to using regions.

The coefficient of interest is β, capturing the causal impact of the reform at the discontinuity. We further control flexibly for population size on either side of the threshold, to isolate the effect of the reform itself. Our baseline specification uses a first-order polynomial, with second-order polynomials used for robustness. Standard errors are clustered at the municipal level. Concretely, we compare children born in the same year who lived in similar towns differing only in whether they crossed the 4,000-inhabitant threshold—and thus had access to two rather than four years of compulsory schooling—and we difference this contrast against slightly older cohorts who were already too old to be affected by the reform.

We do not employ a simple regression discontinuity design because other laws also changed discontinuously at the 4,000-inhabitant threshold.[9]

Identifying Assumptions: We provide evidence in favor of the validity of this approach in multiple ways. First, the manipulation test reveals no evidence of sorting in the running variable (Figure D.1). Second, we rely on pre-treatment covariates to test the credibility of the continuity in potential outcomes assumption. We observe a high level of balance in covariates across the discontinuity threshold, with none of the differences in means reaching statistical significance (Table D.1). The analysis section also implements a series of tests at placebo discontinuities, both in municipal population and date of birth. Finally, we present evidence consistent with the lack of pre-trends in the event-study in the results section.

Alternative Strategy, RD Design: The difference-in-discontinuities strategy requires a demanding set of data, including individual-level information on date and place of birth. For many of our secondary analyses, this level of detail is unavailable as we can rely only on municipal-level or municipal-year variation. In these cases, we use a simpler regression discontinuity design (RDD) that exploits only the population threshold and controls for reign fixed effects:

$$Y_{it} = \mu Above4000_i + \nu f(Pop1861)_i + \xi f(Pop1861) \times Above_i + \rho_r + \pi i \qquad (2)$$

[9]See, for example, Art. 15, Law n. 2248/1865 or R.D. January 26th, 1896, n. 20.

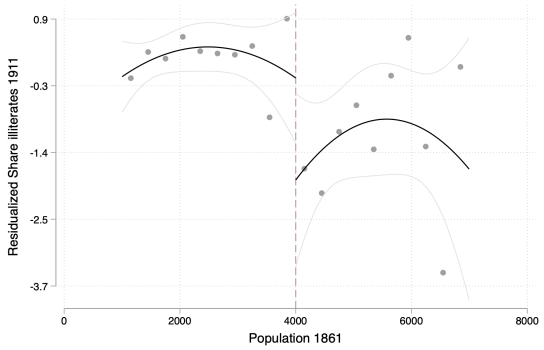
When year is present, we also include year fixed effects and show year-by-year estimates. While we provide evidence in line with continuity of potential outcomes (Table D.1) and absence of sorting (Figure D.1), this specification is less precise than the DDRD, since other laws also introduced discontinuities at 4,000 inhabitants. In particular, Art. 15 of Law n. 2248/1865 allowed municipalities above the threshold to create fractions of the municipality, and the Royal Decree of January 26, 1896, n. 20 applied different provisions for customs to municipalities above the threshold. Given that the 4,000-inhabitant cutoff was not unique to the education reform, the DDRD is our preferred identification strategy, and we use the RDD only when data limitations preclude adopting the DDRD. Results from the latter should therefore be interpreted with more caution.

Effect of the Reform on Literacy: We exploit this alternative identification strategy to provide evidence consistent with the idea that the Casati reform effectively expanded education, by assessing its long-run impact on literacy. We digitize the first available municipal-level illiteracy data from the 1911 Census, forty years after enactment and before later schooling reforms and estimate a cross-sectional regression discontinuity design. Treated municipalities exhibit from 1.6 to 1.8 percentage points lower illiteracy rates—a 4.8 to 5 % reduction from baseline levels (Table 2b).

While we measure the impact of the reform on an easily measurable outcome like literacy, it is plausible that the reform had broader impacts on the education of pupils than teaching them how to read and write. While the first two years of schooling focused primarily on literacy, mathematics, and religion, the additional two years introduced new subjects – geography, history, physical and natural sciences, and literature. Thus, beyond reducing illiteracy, the reform likely broadened their intellectual exposure, potentially inspiring students to read outside the prescribed curriculum or to continue their studies beyond the elementary level.

Figure 2: Education Reform Impact on Illiteracy (RDD)

(a) RD Plot



(b) RD estimates

	(1)	(2)
	Share illiterates	
Above4000	-1.760*	-1.585**
	(0.976)	(0.744)
Observations	7,123	7,123
Bandwidth	2084	2567
N Left	5916	5916
N Right	1207	1207
Robust P-val	0.190	0.125
Conventional P-val	0.071	0.033
Fixed effects	Region	Province
Mean DV	33.21	33.21

Notes: (a) RDD results (Equation 2) of the impact of the 4,000 population cutoff at which the education reform applied on municipal-level illiteracy rates in 1911. We consider residualized illiteracy rates after accounting for province fixed effects. *(b)* RDD coefficients with region and province fixed effects. Standard errors are clustered at the municipal level.

5 Effect of the Education Reform on Surveillance

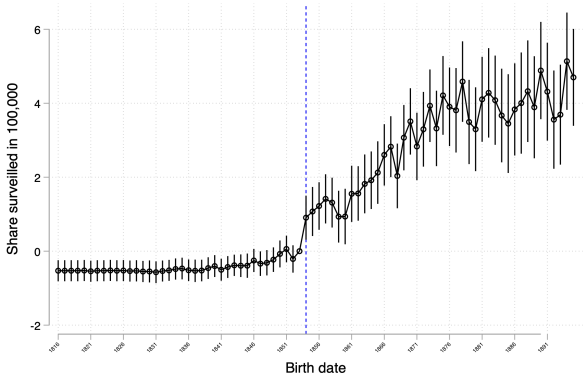
Our main question is whether increased schooling led to a higher probability of a municipality-cohort being targeted by state surveillance. In Table 3b, we present our main results. Municipality-cohorts exposed to the education reform experienced 3.5-3.7 more individual surveilled in 100,000 inhabitants compared to control group municipalities. This effect is large when we consider that the average number of surveilled across cohort exposed to the reform in municipalities just below the 4,000 cutoff is 5.5 per 100,000 inhabitants, meaning that the education reform increases surveillance by 64%. Assuming 50 cohorts were alive at the same time per municipality[10], the number of surveilled in a municipality would pass from 27.5 to 47.5 in 10,000 inhabitants per municipality. Effects are similar in magnitude across specifications that do and do not include fixed effects, in line with the possibility that we are isolating the impact of a quasi-random treatment. We present this analysis as an event study in Figure 3a. In the forty cohorts preceding the first cohort treated (1854), there was no significant difference in surveillance between municipality-cohorts that will later be exposed to the reform and those that will not. The unusually long pre-treatment period

[10]Based on life expectancy in 1910 in Italy (Caselli, 1990).

in our setting provides stronger support for the parallel trends assumption than is typically possible in difference-in-differences designs.

Figure 3: Effect of the Education Reform on Surveilled in 100,000

(a) Event Study



(b) Difference-in-Discontinuity

	(1)	(2)	(3)
	Share Surveilled in 100,000		
Post × Above4000	3.729***	3.605***	3.515***
	(0.359)	(0.350)	(0.318)
Observations	617,040	617,040	617,040
Birth FE	Y	-	-
Municipality FE	N	Y	Y
Reign x Birth FE	N	N	Y
Mean DV	5.499	5.499	5.499

Notes: *(a)* Event-study and *(b)* difference-in-discontinuity estimates (Equation 1). *Above*4000 is= 1 for municipalities> 4,000 inhabitants and *Post* is= 1 for cohorts affected by the reform (born>=1854). The reference cohort is 1853. Regressions include municipality, birth-year, and reign×birth-year fixed effects. We report the mean of the dependent variable for cities with population just below 4,000 after the reform within the RDD optimal bandwidth. Standard errors are clustered at the municipality level.

Beginning with the 1854 cohort, we observe a stark increase in surveillance, corresponding to an additional 0.4 individuals surveilled per 100,000 in the first year after the reform's implementation. This effect grows over time, reaching an additional 4 individuals surveilled per 100,000. The increasing impact of the reform over time is consistent with the state's gradual expansion of its capacity to enforce mandatory education, and the broader uptake of educational resources, as schooling became less of an elite privilege. While detailed time-varying data on the expansion of schooling services in this period is absent, preventing us to provide direct evidence on when and where education became more inclusive, we account for fine-grained location fixed effects and time-varying regional trends to account, for example, for time-varying underdevelopment in different regions of Italy.

End of the discontinuity: What happens when the discontinuity in education disappears? As discussed in Section 4, compulsory schooling was extended to all municipalities from

1904, eliminating a distinction between places with more and less than 4,000 inhabitants. If the increase in surveillance was produced by the education reform, we should see it disappearing when the distinction between cities is eliminated. In the extended event study in Figure E.1, we see that surveillance decreases exactly for individuals born from 1896 or later in municipalities above 4,000 inhabitants, now exposed to the same education requirements as other municipalities. As for the main result, the difference between previously treated and control municipalities does not disappear immediately: places that had built school capital over time maintain an advantage in effectively providing education compared to newly treated locations, resulting in a slowly decreasing impact on surveillance. Over time, the impact converges to zero, with surveillance rates equalizing across municipalities on either side of the original discontinuity.

Robustness: We start by considering robustness to alternative specifications in Table E.1. First, we account for variation coming from calendar year effects by controlling for the rate of surveillance in different periods of Italian history. We create a vector of controls, one for each decade in the calendar years of surveillance, measuring the number of individuals from each birth cohort who started being surveilled during that decade. Results are similar in magnitude and significance adding this vector of controls (column 1). Second, we show that results are robust to estimating a staggered DDRD to account for the reforms' later implementation in areas annexed to the Kingdom of Italy after 1861, which are excluded from the main analysis (column 2). Column 3 shows that replacing reign (defined pre-treatment) with region (post-treatment) times birth-year fixed effects does not affect our results. We relax functional form assumptions in column 4, showing that results are consistent using a Poisson specification. Columns 5 and 6 consider higher-order polynomials.

Table E.2 tests robustness to alternative transformations of the dependent variable. Using a dummy equal to one if any individual is surveilled in a municipality-cohort, we observe that the education reform increases the likelihood of surveillance a 28% (column 1).

By contrast, considering the absolute count yields a null effect (column 2), which is consistent with a highly zero-inflated and right-skewed distribution. In fact, by trimming the upper tail via winsorization (column 3) or modeling proportional changes via a log transformation (column 4), we are able to recover positive and significant effects.

In Table E.3, we replicate our results using different designs. We start from a less restrictive difference-in-differences (DID) which leverages the entire distribution and not only municipalities close to the cutoff (columns 4-6). We use, instead, a more restrictive model flexibly controlling for both population and for birth year, such that we consider observations in the proximity of both the geographic and the temporal discontinuity, in the spirit of a "double" difference-in-discontinuity design (columns 7-9). Finally, we remove the interaction with Post from the flexible controls to estimate a regression discontinuity version of Equation 1 in columns 1-3, and present a RD plot in Figure E.2. Our findings are robust across these designs.

Placebos: We present two sets of placebo analyses to rule out the possibility that other factors cause the jump we observe at the education reform discontinuity. First, we consider effects at other population thresholds. We zoom into a window of 10,000 around each population threshold, both above and below 4000. While we observe similar results around the 4,000 discontinuity, we find null effects for all other thresholds in Tables E.4 and E.5. This confirms that our findings are not driven by a general pattern of increased surveillance in larger towns, or by other effects observable at round population numbers.

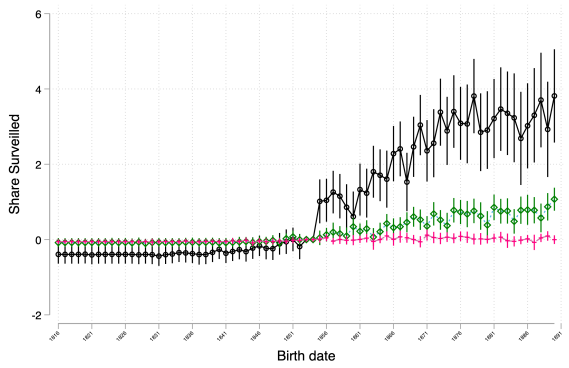
Second, we repeat the same exercise for cohort of birth and ask whether being born in another cohort unexposed to the reform is likely to trigger the same effect by replacing our *Post* indicator with a set of placebo cohorts. Findings presented in Table E.6 rule out the importance of non-treated cohorts in explaining the effect we find on surveillance.

6 Surveillance of the Working Class

The education reform led to increased surveillance among treated cohorts. To assess whether this effect is connected to state concerns over the education of marginalized populations, we classify the professions of surveilled individuals into distinct social classes, as detailed in the Data section and illustrated in Figure C.4. We then use the same specification as in Equation 1 on three distinct dependent variables: surveilled from the working, middle, and upper class. The results, presented in event-study (Figure 4a) and table format (Table 4b), indicate that the increased surveillance following the reform is primarily driven by the working class. The increase in surveillance for this group is large: there are 2.9 more individuals surveilled in 100,000 – a 60% increase with respect to municipalities just below the 4,000 the cutoff after the reform. We observe a positive but smaller effect for the middle (beta = 0.57, column 2) and upper classes (beta = 0.07, column 3). The last row reports the p-value from a one-sided test of the null hypothesis that the effect on the working class is weaker than that on the other class. We reject the null for both comparisons, indicating a significantly larger effect for the working class. The differential increase in surveillance for this social group is also visible in the event study plot. Coefficients remain consistent (if anything, larger) when we use a binary indicator for the presence of any surveilled individuals in that social class (Table F.1): for the working class, the increase is 0.24 percentage points, while for the middle and upper classes the increase is 0.04 and 0.004 percentage points respectively.

Figure 4: Effect of the Education Reform on Surveillance by Class

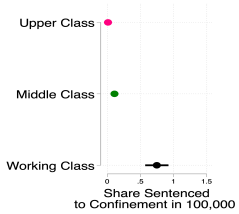
(a) Event Study



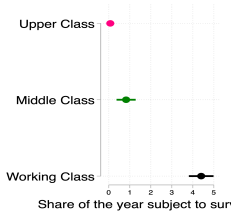
(b) Difference-in-discontinuity

	(1)	(2)	(3)
	Share Surveilled in 100,000		
	Working class	Middle class	Upper class
Post × Above4000	2.871***	0.574***	0.0690***
	(0.281)	(0.0544)	(0.0196)
Observations	617,040	617,040	617,040
Municipality FE	Y	Y	Y
Reign x Birth FE	Y	Y	Y
Mean DV	4.767	0.558	0.118
H_0: Working Class <		0	0

(c) Severity



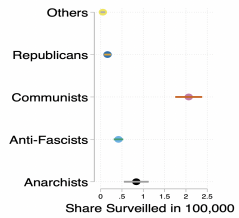
(d) Duration



(e) Information



(f) Ideology



Notes: *(a)*: DDRD eventstudy using as dependent variables surveilled in the working (black), middle (green) and upper (pink) class. *(b):* DDRD coefficients (Equation 1). We report the mean of the dependent variable for municipalities just$< 4,000$ inhabitants after the reform. The last row reports the p-value from a one-sided test of the null hypothesis that the effect on the working class is statistically lower than that of the other class. *(c-e)*: DDRD coefficients from separate regressions using as dependent variable the severity, duration, and volume of information collected on surveilled from each class. *(f)*: DDRD coefficients from separate regressions using as dependent variable surveillance of individuals from different ideologies. All regressions include municipality and reign times birth fixed effects. Standard errors are clustered at the municipal level in all regressions.

A potential concern with interpreting this analysis is that the number of individuals in the working class could itself be influenced by the education reform. However, given that the reform expanded access to literacy and likely improved the labor prospects of children born in low income families, it most likely reduced the overall proportion of individuals in the working class in the post-reform period. This would imply that our findings represent a lower bound of the true effect, as we cannot directly observe the fraction of the population moving to the middle class due to improved opportunities. Therefore, the observed effects

of surveillance in this analysis should be interpreted as a conservative estimate of the actual impact.

6.1 State Behavior Consistent with Threat Perception

If surveillance increases because the state fears that education equips subaltern groups with political skills, then we should observe not only higher surveillance rates but also more intensive forms of monitoring directed at these groups. We evaluate this prediction using three measures of surveillance intensity – the severity of control, the duration of surveillance, and the volume of information collected – and by examining whether surveillance disproportionately targeted certain ideological orientations.

Severity: Our first measure captures whether individuals were subjected to severe forms of surveillance, such as monitoring movements across borders, and political confinement, a costly tool used to restrict dissidents' movements and in some cases forcibly remove them from their communities. Table F.2, column 1, shows that municipality–cohorts affected by the education reform experienced higher rates of severe surveillance. Consistent with the state perceiving a threat and responding with harsher measures, the increase in severe control is concentrated among the working class (beta = 0.77, column 2) and less pronounced for other groups (beta = 0.11 and 0.01, columns 3-4).

Duration: The second measure considers how long individuals remained under surveillance. Duration provides a measure of the intensity and persistence of state attention: longer surveillance implies greater resource investment and a higher perception of threat. Given that our time dimension is a cohort, we express this duration in terms of how long a cohort was surveilled for in a municipality. On average, cohorts in control municipalities after the reform are surveilled 2.1 years. The reform significantly extends surveillance duration: treated cohorts are surveilled 5.2 years longer, a 2.5-fold increase (Table F.3, column 1). As with the severity of surveillance, this effect is most pronounced among the working class,

where surveillance duration increases by 4.4 (column 2), exceeding the increase observed for middle (beta = 0.82, column 3) and upper-class individuals (beta = 0.07, column 4).

Volume of Information: The third measure examines whether authorities collected extensive information on the surveilled, as indicated by the presence of a biographical file ("scheda biografica"). These files were maintained for all significant political figures or for individuals considered particularly dangerous. Descriptive statistics show that such files were more frequently recorded for working class individuals (81.17%) compared to middle-class (15%) and upper-class (2.81%) individuals. The remaining question is whether this pattern is enhanced by quasi-exogenous exposure to education. Table F.4, column 1, shows that treated cohorts were more likely to have a biographical file, with the share of surveilled individuals with biographical documentation increasing by 1.36 percentage points. This increase is driven by the working class (beta = 1.1, column 2), which experiences an increase larger than for any other class (0.23 and 0.03 for middle and upper class individuals, respectively, in columns 3 and 4). Across all three measures, the evidence shows that the state intensified surveillance following the education reform, adopting harsher methods and more intensive control practices, and that the sharpest increase was targeted at working-class individuals.

Ideology of Surveilled: We next examine whether ideological orientation shaped who was targeted. If surveillance indeed reflected fears of educated working classes, we should observe disproportionate monitoring of ideologies advocating on their behalf. Table F.5 presents surveillance rates by ideological affiliation. While all political groups experienced increased monitoring following the education reform, socialists and communists were surveilled at the highest rates. This pattern aligns with their ideological focus on addressing the demands and grievances of subaltern classes.

A final test considers whether targeting varied with the ideology of the governing party. Before Fascism, two parties alternated in power: the Historical Left, which supported secularism and centralization, and the Historical Right, which championed monarchical

authority, conservative values, and local autonomy. Neither party, however, represented the interests of the working classes, as both were elite projects dominated by landowners, intellectuals, and the bourgeoisie. Consistent with this, our results reveal little partisan heterogeneity: surveillance of the working class is statistically indistinguishable between Historical Left and Right (Table F.6), underscoring the exclusion of the working class from meaningful political representation.

7 Education and Political Engagement

Did the increase in surveillance following an expansion of education stem from an increase in political engagement? We test this possibility by measuring the impact of the education reform on political activism. To explore this possibility as extensively as is possible, we use and partly collect five different indicators of political engagement that range from low-cost high-frequency actions, like turning out to vote, to actions requiring substantial commitment and risk, such as taking up weapons against the state.

Voter mobilization: We examine the impact of the reform on voting in national elections using municipality–election year level data, beginning with the first election in unified Italy in 1861. The first treated cohorts became eligible to vote in 1880, when individuals born in 1854—those first affected by the reform—reached the voting age of 25. In this initial year of treatment, only a small share of the electorate had received the additional schooling. Because treatment exposure then increases gradually as more cohorts enter the electorate, the time discontinuity is less informative in this setting. For this reason, our main analysis presents RD coefficients for the impact of the 4,000 inhabitant cutoff (Equation 2) estimated separately for each election year, allowing us to trace the evolution of the effect over time independently of the pre-1880 period. For completeness, we also report DDRD estimates in appendix. Our analysis ends in 1913, the last election year before new cohorts across both sides of the population threshold had attained similar levels of education under subsequent

reforms.

We start by presenting evidence that, before the first affected cohort starts voting, municipalities above the 4,000-population threshold voted at about the same rate as those below (Figure G.5). We then consider the impact of the education reform after the first cohort gained voting rights.

We observe no differences in voting in national elections in municipalities-election years affected by the education reform. This is true both considering the year-by-year RD coefficients (Figure 5, panel(a)) and the overall difference between the pre and post period in a DDRD framework using different transformation of the dependent variable – absolute number, log, population share (Table G.1).

Notable people involved in politics: Our second measure of political engagement is the number of individuals involved in any type of political role, which we scrape from Wikidata. Given that we have individuals' date of birth, this data allows us to replicate the same municipality-cohort specification as in our main analysis on surveillance. We present the DDRD event study in Figure 5, panel (b). Before the education reform, municipalities above 4,000 inhabitants had the same rate of participation in politics as municipalities below the threshold. After the reform entered in effect, there was still no difference between municipalities – if anything we see a slight negative impact of the reform. The null effect is confirmed in Table G.2, where we also show the results for all notable figures (not only in politics). Also with the full DDRD specification, the reform had no significant effect on the share of individuals involved in politics according to Wikipedia, nor in any other occupation.

Protests and strikes: Third, we consider a form of activism that is more costly than voting and more widespread than holding a political position: participation in protests and strikes. In this case, the data is at the municipality-year level: we do not have individuals' birth date, nor a pre-period before the reform. Because we cannot exploit the temporal component, we rely only on the cross-sectional regression discontinuity design at 4,000

inhabitants. Consistently with the previous results, our findings indicate no significant increase in politically motivated activities in municipalities above the 4,000 discontinuity (Figure 5, panel (d) and Table G.3).

Partisans and Bandits: Finally, we consider two extremely intensive forms of political activism: participating in the resistance movement and engaging in banditry. Partisans were guerrilla fighters resisting Fascist and Nazi forces through sabotage and combat. Bandits, often seen as rebels rather than criminals, carried out raids and kidnappings, particularly in southern and central Italy, where social inequalities were severe (Ciconte, 2011). Both were armed forms of action involving a high risk of death. The data on partisans includes date and location of birth, allowing us to run the same DDRD specification as in the main analyses. When we use the share of partisans (as for our main specification on surveillance), we find a null effect of the education reform on the likelihood of engaging in the resistance movement (Table G.4, column 1). Effects are mixed with an alternative dependent variable: they become negative considering the total number of partisans. Figure 5, panel (c), instead, shows a clear null effect on the number of bandits.[11]

Does political engagement drop because of surveillance? We have examined an extended array of political engagement measures, differing on multiple dimensions, from how widespread they are in the population to how costly it is to engage in them. Across those measures, we have found no evidence that education increased political engagement. Could this be the result of a state strategy to chill participation through preventive surveillance?

On one hand, it was shown that totalitarian surveillance in East Germany and authoritarian control in China depressed political engagement (Hager and Krakowski, 2022; Xu, 2021). On the other hand, in a setting where the state was not authoritarian and repression was not large-scale, and where only the most engaged individuals were likely aware of being under police scrutiny, participation might have suffered less from increased state surveillance. While

[11]The cross-sectional nature of the data prevents us from running a DDRD.

we cannot rule out that chilling effects are why we observe no increase in political activism, our descriptive evidence is not consistent with this possibility. We consider two pieces of evidence.

First, we repeat these analyses by splitting the sample for cohorts that were and were not surveilled. For this analysis, we rely on the only data varying at the cohort level - holding political positions as documented in Wikidata. If surveillance had a systematic impact on political participation, we would observe *more* political figures in cohorts subject to the education reform but where surveillance was absent. Instead, we observe again a null effect on the number of political figures in these cohorts, and an effect that is not differential from that on other cohorts exposed to surveillance (Table G.2).

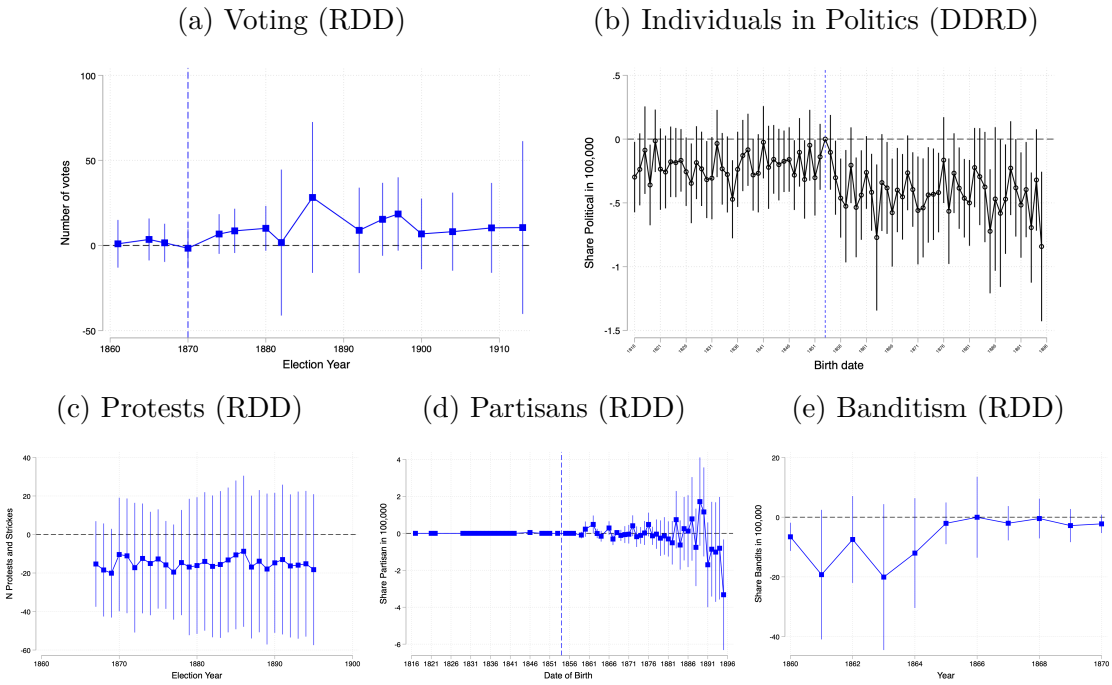
Second, we run a heterogeneity analyses for each indicator of political engagement by whether the level of surveillance was high (Table G.5) or whether no individuals were surveilled from untreated cohorts in a municipality (Table G.6). Across analyses, we find no differential impact of high surveillance on any metric of political activism. While both these analyses are not causal – in the first case, there might be endogenous reasons why certain cohorts are not surveilled, and the second analysis is a simple heterogeneity – all results point to the same conclusion of no differential impact of the education reform on political engagement depending on surveillance.

These findings align with the possibility that, outside of contexts where the state is highly repressive and individuals expect to be subject of tight surveillance, as in totalitarian regimes, surveillance may not have a large impact engagement at least not to a degree detectable in our analyses.

Why did the education reform impact surveillance but not political engagement?

The presence of effects on surveillance but not on political activism could appear as a contradiction: why did the state mobilize more resources if there was no real threat to respond to? From the state's perspective, even a small number of empowered members of subaltern groups might pose enough risk to justify increased surveillance of many of

Figure 5: Effect of the education reform on political engagement



Notes: *(a, c-e)* RDD coefficients (Equation 2) in each available year for being above the 4,000 population discontinuity. We consider different outcomes and time dimensions: the number of votes cast by election year *(a)*, the winsorized number of protests in each calendar year *(c)*, the population share of partisans by date of birth *(d)* and the share of bandits by calendar year *(e)*. *(b)* DDRD event study (Equation 1) of the effect of the education reform on the share of individuals in politics in 100,000 inhabitants in a municipality-cohort. Standard errors are clustered at the municipal level in all analyses.

them, as revolutionary entrepreneurs can have wide-ranging consequences for states' stability (De Mesquita, 2010). Therefore, even if education did not trigger widespread mobilization, the state's calculus of targeting this category at higher rates might still be correct.

The lack of evidence of increased political engagement following the reform thus supports the possibility that heightened surveillance was not primarily driven by increased political mobilization but rather by state concerns over newly educated, potentially influential working class citizens.

7.1 Additional Heterogeneities

Democracy and Dictatorship: A substantively important question is whether the strategic use of state surveillance we uncover in these analyses varies across regime types. To answer this question, we leverage our unique setting, which allows us to observe state behavior across both democratic (1869-1921) and fascist governments (1922-1947). Table H.1 presents results separately for these two periods. The findings indicate similar patterns across both regimes. Under democracy (columns 1-4), the likelihood of surveillance increases by a factor of 1.54, while under fascism (columns 5-8), it increases by a factor of 1.33. In both cases, the effect is largest among the working class. The consistent patterns across regimes suggest that educated yet economically marginalized individuals remain a primary focus of state surveillance, regardless of the governing ideology.

State Capacity: Another potential factor influencing surveillance intensity is state capacity. To assess this, we digitize new data on postal offices in the Kingdom of Italy, sourced from an 1861 official postal bulletin, as a proxy for state presence. Columns (1) and (2) in Table H.2 show that surveillance is more prevalent in areas with postal offices, but the effect remains significant even in their absence. Second, columns (3)-(5) examines geographical variation in treatment effects, revealing that while the impact of education reform on surveillance is stronger in Central Italy, similar patterns are observed across all regions. These results indicate that, although greater state capacity facilitates surveillance, it is not a prerequisite for the patterns documented here.

Technology: The technology employed for surveillance can also influence its intensity. In 1916, Italian military units began utilizing telephone wiretapping as a tool for intelligence collection. Column (6) of Table H.2 shows that this innovation did not correspond to a statistically significant increase in the start of surveillance within the same municipality-cohort.

Pre-Reform Surveillance Levels: Finally, the analysis considers whether the increase in surveillance is driven by newly monitored areas or by heightened scrutiny in already highly surveilled regions. Table H.3 shows that both channels contribute to the observed effects. Treatment effects are larger in areas with higher pre-reform surveillance, possibly due to spillover from surveilling more individuals, or network-driven mobilization. However, the effect remains significant even when accounting for pre-reform surveillance levels.

8 Alternative Explanations

Other municipal-level changes induced by the education reform:

The main analysis shows that surveillance responds to schooling itself, not to prior attributes (family background, genetic endowments) causing both education and surveillance. It is, however, possible that the education reform impacted other local conditions that might have triggered surveillance independently on state activation to target educated working classes. For instance, the increase in surveillance could result from increased education levels among local institutions, such as the police, or among the general population. A better-educated police force might be more effective at monitoring individuals. Notice, however, that for such a mechanism to account for the jump in surveillance observed for the first post-reform cohort, two conditions must hold: (i) the rise in surveillance would have to be driven specifically by the first cohort of newly educated policemen or citizens; and (ii) these actors would need to selectively target individuals from their own age cohort, since our results are driven by the age of those surveilled rather than by the timing of surveillance.[12]

We empirically rule out this class of alternative explanations by running our analyses on individuals who relocated away from their municipality of origin. Because these individuals are no longer exposed to changes in municipal-level characteristics, any local institutional

[12]In the case of law enforcement specifically, officers were typically assigned based on institutional needs rather than place of origin, making it unlikely that they would serve in their city of origin. See, for example, Arma dei Carabinieri, Chapter XII – Transfers.

or compositional shifts –such as changes in the education level of local officials or residents– should not affect their likelihood of being surveilled. As shown in columns (4)–(6) of Table I.1, the results for this subsample closely mirror those reported in Table 3b, suggesting that migration does not attenuate the treatment effect.

This finding does more than rule out a confounding channel: it reinforces our interpretation of the mechanism. The persistence of the effect among movers indicates that the treatment operates through a portable individual characteristic –namely, human capital– rather than through fixed local attributes.

Migration of educated individuals: Conversely, we also show that migrants do not drive our findings. This could be a concern if we consider that educated individuals might be more likely to migrate to larger cities for better opportunities, and there they might encounter heightened political activism. In this scenario, increased surveillance could stem from education-driven migration, as individuals with higher education gravitate toward politically active urban centers. To rule out this possibility, we limit the analysis to those who remained in their birthplace. Results in Table I.1 confirm our primary findings even among non-migrants: treated municipality-cohorts experience an increase in the number of surveilled individuals to about 0.78 times the baseline –a result consistent with those shown in Table 3b. Therefore, while education may indeed have influenced migration and increased exposure to activism, this does not appear to be the main driver of our results.

Teachers as state agents: One possibility is that teachers, as part of the bureaucratic system, may have acted as informers for the state, monitoring former students and report on their political activity. While this possibility is already ruled out by the first test on migrants, and it could not explain the increase in the first treated cohort, we investigate this possibility further. If state bureaucrats were used as informants, we would expect that also other reforms increasing the size of the bureaucracy would lead to a similar surge in surveillance. To test this, we examine the impact of the Crispi Law of 1891, which required municipalities with over 5,000 residents to expand their welfare boards, thereby substantially

increasing the administrative workforce. Results in Table I.2 reveal that this expansion had no measurable effect on the level of surveillance. This indirect test reinforces the historical accounts, suggesting that an increase in the number of bureaucrats alone did not necessarily lead to heightened state surveillance.[13]

School records: A related story might be that the reform generated more school records, making it easier for the police to identify and track exposed cohorts. However, first, the 4,000 discontinuity does not expand enrolment – which was compulsory – but merely lengthened primary schooling by two years, so record coverage did not jump at the reform cutoff. Second, our outcome measures surveillance at any age, often decades after pupils left elementary school; what a child did in third rather than second grade is unlikely to inform political policing when that same person is 40. Hence the reform's effect is unlikely to be a source of significantly better archival access alone.

Teachers as instigators: Another possibility is that the state perceives teachers as dangerous instigators of radical ideologies. If this were the case, we would observe that the increase in surveillance is driven by a surge in surveillance of teachers, who become a larger occupational group in treated areas. First, teachers constituted a negligible fraction of those surveilled pre-reform (0.00005 per 100,000 inhabitants), making it improbable that their surveillance alone drives the treatment effect. Second, although the number of teachers in the population likely increases post-reform, the absolute number of teachers who are surveilled does not increase (Table I.3, column 1), implying that the surveillance rate of teachers must have declined, or at most remained constant. Finally, the share of teachers among those surveilled increases (column 2), but by an amount that is similar to or smaller than that observed for other professions with comparable baseline surveillance rates (columns

[13]In addition, historical records indicate that informants were generally individuals in dire personal situations and working as informants to attenuate personal problems, such as prisoners seeking reduced sentences by reporting on their jail mates (Ciconte, 2011). Instead, teachers held financially stable and high-status positions, far from the situations of captivity that motivated informants.

3–5, Table I.3), despite the post-reform growth in the share of teachers in the population. These facts point to the same conclusion: teachers were not the group the police singled out after the reform.

Job prospects: A potential alternative story behind the observed effects on the working class can be found in the downstream consequences of surveillance itself. Surveillance may reduce individuals' job prospects, thereby pushing them to select into working class occupations. Table I.4 examines the main results by age at the start of surveillance and shows that the effect is present even among individuals who began being surveilled as adults or late in life, after their careers had been defined. This suggests that the impact of the reform on surveillance cannot be explained by early-career sorting into the working class.

Substitution with the Church: One effect of the education reform is that schools began to take on the church's role in education, as before the reform, churches were largely responsible for occupying and educating children. However, since our analysis centers on rebellion against the state, and the reform positioned the state as the provider of education, it is implausible that state-led schooling would inspire greater rebellion against itself. In fact, research by Paglayan (2024) shows that states typically design education reforms to cultivate loyalty and compliance among citizens. Therefore, if a substitution from church to state oversight occurred, as seems likely, we would expect the opposite effect: with increased exposure to state-run education, surveillance should have decreased, not increased.

9 Conclusions

Our study sheds new light on the strategic considerations states make when deciding whom to subject to political surveillance. Our findings reveal that cohorts exposed to additional primary education were subjected to increased state surveillance, particularly when they belonged to working class backgrounds. This effect is present across democratic and authoritarian regimes, and independently on levels of state capacity. We find limited evidence that

an increase in political mobilization can explain these effects, consistent with surveillance spurring from state heightened concern rather than from an effectively higher threat. Our findings support a view in which states perceive educated but socio-economically subaltern individuals as particularly threatening to regime stability, regardless of whether these individuals engage in overt political activism.

To what extent do historical findings on surveillance remain relevant in the age of digital monitoring? Specifically, do states still face resource constraints that force them to choose whom to surveil, or have digital tools eliminated the need for selectivity, enabling widespread and indiscriminate targeting? On one hand, social media and facial recognition technologies have made it easier for states to gather information on individuals. On the other hand, technological advancements also provide activists with new ways to evade scrutiny, such as encrypted communication and an abundance of alternative channels for correspondence. Moreover, while digital tools have expanded surveillance capabilities, collecting, storing, and analyzing vast amounts of data remains costly, particularly when it requires transforming unstructured information into actionable intelligence. In authoritarian contexts, where dissidents are acutely aware of surveillance risks, large datasets may yield little useful intelligence, as activists take steps to avoid leaving digital traces of their actions. As a result, states may still need to rely on traditional intelligence-gathering methods. Considering these factors, we argue that it remains useful to think of states as resource-constrained actors who must make strategic choices about whom to monitor, indicating that our findings based on historical data – the only type of intelligence data on surveilled likely to be shared by states – might still yield insights to understand our time of digital surveillance.

While this study shed light on the logic of state surveillance, most facets of this importat tool of state control remain understudied. For example, resources allocation for surveillance might vary by target: large numbers of newly educated workers may be monitored lightly to gauge their intentions, whereas a handful of elites might be followed intensively to detect concrete plots. Future research could also chart how covert and overt surveillance evolve

over a movement's life cycle, and how different agencies share—or weaponize—intelligence. Finally, extending the analysis to other contexts would allow scholars to consider alternative dimensions of subalternity beyond class, offering a richer understanding of the strategies of state surveillance.

This research offers a new perspective on the enduring challenges faced by states in balancing security with democratic inclusivity. As states around the world continue to adopt increasingly sophisticated surveillance technologies, our findings emphasize the need to ensure that these tools do not disproportionately target or disenfranchise already vulnerable populations.

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SUPPLEMENTARY INFORMATION

— For Online Publication —

A Theoretical Framework

A.1 Overt and Covert Repression

In his seminal work, Davenport (2005) distinguishes between two forms of repression: overt and covert. Overt repression includes visible state actions such as political arrests, protest crackdowns, censorship, disappearances, and assassinations of political opponents. Covert repression, by contrast, operates largely out of sight and without actively repressing dissent, and includes activities such as surveillance, infiltration, and information-gathering on dissidents. While both forms serve the state's objective of controlling oppositions, they entail distinct logics, costs, and risks (Andirin et al., 2022), which in turn shape who becomes a target.

The literature on overt repression is extensive. Scholars have identified a range of conditions under which states are more likely to deploy visible repressive measures[14] and have documented how certain groups suffer more targeting, such as ethnic minorities (Rozenas, 2020; Wen, 2025) or the enemies of regime allies (Klor, Saiegh and Satyanath, 2016). Other studies have examined the effects of repressive strategies on persuasion (Enikolopov, Petrova and Zhuravskaya, 2011; Rozenas and Zhukov, 2019), collective action (Morris and Shadmehr, 2024), wages (Hsieh et al., 2011), and military obedience (Rozenas, Talibova and Zhukov, 2024).

tunity structure (Rozenas and Zhukov, 2019), the strength of social networks (Siegel, 2011), the type of organizations involved (Sullivan, 2016; Garfias and Magaloni, 2018), the timing of mobilization (Truex, 2019), or the psychological responses of dissidents (Young,

[14](Besley and Persson, 2009; Pierskalla, 2010; King, Pan and Roberts, 2013; Shadmehr, 2014; Ritter and Conrad, 2016; Truex, 2019; Blaydes, 2018; Guriev and Treisman, 2020; González et al., 2025)

2019; Nugent, 2020).

By contrast, the literature on covert repression is far more limited. Existing studies on surveillance have focused on the actors conducting the monitoring – police, secret services, intelligence apparatus – and have examined the consequences of surveillance on protest (Beraja et al., 2023; Hager and Krakowski, 2022), imprisonment (Gohdes, 2020), public goods (Xu, 2021), and long-run trust (Lichter, Löffler and Siegloch, 2021). Recent work also highlights how international pressures can induce regimes to shift from overt repression to surveillance (Andirin et al., 2022).

Yet, we still lack studies that examine the logic of surveillance as a dependent variable, and which can rely on data identifying who is monitored. An important exception is Davenport (2005), who analyzes U.S. surveillance of the Republic of New Africa and shows correlational evidence that racial composition shaped the likelihood of being targeted. Our paper builds on this seminal contribution and extends it in several ways. Empirically, we examine a vastly larger population of surveilled across multiple organizations, governments, and ideological orientations, spanning a much longer period. We also move from the neighborhood to the individual level and from correlations to causal estimates. Theoretically, and importantly for this section, we advance a new hypothesis to explain how states decide whom to target with surveillance.

Surveillance differs in important respects from overt repression. It is typically not designed to send a public signal or deter through visible punishment; rather, it serves to gather information while remaining hidden from public view. Because it is generally undisclosed (except for unintentional leaks), surveillance carries minimal reputational costs for the state, either domestically or internationally (Andirin et al., 2022), and it avoids the risk of domestic backlash that often accompanies visible repression. At the same time, surveillance is costly: it demands significant bureaucratic capacity to monitor, record, and process information, often across multiple layers of state administration and intelligence. Thus, unlike overt repression, which is constrained by audience costs and potential political

blowback, covert repression is primarily constrained by resource allocation: the state must decide whom it is worth mobilizing costly surveillance apparatuses to monitor. This allocation problem lies at the center of our study.

A.2 The Logic of State Surveillance

Surveillance demands substantial resources: bureaucratic capacity to gather information, intelligence to distinguish critical signals from noise, and analytical tools to interpret patterns. Given these constraints, states are likely to prioritize monitoring individuals who pose the greatest threat to regime stability. But who is perceived as a threat? We argue that this depends on the intersection of grievances and mobilization capacity.

This hypothesis builds on two strands of literature. First, research on political participation shows that education can equip individuals with political awareness, organizational skills, and the capacity for collective action (Almond, Verba et al., 1963; Verba et al., 1993; Sondheimer and Green, 2010). Importantly, this does not imply that education uniformly increases political activity (Marshall, 2016; Paglayan, 2024), only that some actors may acquire mobilizing resources through education. Second, scholarship on revolutionary movements highlights that disruptive political change often emerges from subaltern groups—economically or socially marginalized populations with grievances against the status quo and the potential for revolutionary leadership based on class (Marx and Engels, 1848; Gramsci, 1975), ethnicity (Fanon, 1963), or other social markers (Gurr, 1970; Hobsbawm, 1971; Skocpol, 1979; Scott, 1985). We use subalternity following Gramsci's conception: social groups positioned beneath the hegemonic power structure and deprived of the means to voice or exit their oppression (Gramsci, 1975, pp. 2286-2288). In our context, class is the principal dimension of subalternity, though the logic may apply to other markers of marginalization depending on the context. Historical evidence from Italian police archives supports this logic: as we show in the next section, surveillance agents frequently reported focusing on individuals from low-income backgrounds who nonetheless held above-average education.

Importantly, while education tends to increase political participation generally, its combination with economic marginalization may be particularly threatening from the state's perspective. High education equips individuals with the tools to mobilize; low economic standing gives them grievances that make radical action more appealing. Thus, while wealthy and educated individuals may prefer reformist participation (having more to lose), educated individuals in low socioeconomic positions may be more prone to revolutionary mobilization. This leads to our central theoretical expectation: states are likely to direct surveillance resources disproportionately towards politically empowered individuals who are both educated and in a subaltern category, as these actors pose the greatest potential threat to regime stability.

B Additional Details on Data

Social Classification: Each record of the surveilled individuals report the profession. To classify them into district social classes, we refer to Labini (1972), whose work provides a framework for classifying occupations in 1881 into classes, reflecting the societal norms of that era, as social class may change over time. The author divides the society into 4 big groups as follows Labini, 1972, p.105:

- Upper Class (Upper and Middle Bourgeoisie)
  - Owners, Entrepreneurs, Executives, Professionals
- Urban Middle Class (Small Bourgeoisie Not Directly Engaged in Production)
  - Private and Public Employees, Merchants
- Special Categories (Military, Clergy and Others)
- Rural Middle Class (Small Bourgeoisie Directly Engaged in Production
  - Direct Cultivators, Artisans, Transport and Specialized Services

- Working Class
  - Wage Earners in Agriculture, Industry, Construction, Commerce, Transport and Services

Following this scheme, we employed a large-language-model-based classifier (OpenAI GPT-4o, temperature 0.3) to classify each recorded occupation. First, we asked the model to code a sample of 500 jobs (out of 14,780) that we had coded manually. After reviewing the output, we provided the corrected sample back to the model along with a prompt for coding. To improve reliability, we asked the model to assign a confidence score (0–100) to each classification, then we manually reviewed all cases with low scores.

Voting behavior: Data on the voting behavior of the Italian population was obtained from the Electoral Historical Atlas Archive, which includes the results of all general elections held in Italy from 1861 to 2008. For the period between 1861 and 1913, which is the focus of our analysis, the archive provides records of the total number of votes for each candidate at the municipal level. However, there is no available information on the total number of eligible voters. In our analysis, we constructed a panel dataset at the municipality level, where the key variables are the number of votes cast per 100,000 inhabitants and the absolute number of votes cast.

Wikidata: The information on notable individuals known for their roles in politics was sourced from Wikidata. We scraped records for individuals born in Italy between 1816 and 1930. For each individual, we collected data on their birth date, municipality of birth, occupation, and other significant roles held, when available. Based on the last two fields, we identified notable individuals involved in politics. We classified as political those occupations that identify individuals as activists, partisans, bureaucrats, politicians, titled nobility, or religious figures (bishops, archibishops, vicar, popes).

Protests: Information on location and timing of protests was sourced from articles published by the Italian newspaper *La Stampa*, the only including articles dating as far back as 1867. We scraped all articles published between 1867 and 1940 using the keywords *protesta* (protest) and *sciopero* (strike). For each article, we extracted and stored key information such as the title, abstract, publication date, and references to specific locations. We assigned each protest to the location mentioned in the article and then used this data to construct a panel dataset. To ensure accuracy, we excluded cities whose names are also common nouns, such as Porte ("doors"), or Ora ("hour"). For a subset of these common words, we manually reviewed the articles and confirmed that the mentions referred to the common noun, rather than the municipality. Of all the municipalities in our sample, only 0.5% had names that are common words, and of these, only 10 had populations greater than 4,000. These cities were excluded from the protests analysis to avoid misclassification.

Postal Offices: We digitized data on the presence of postal offices per municipality in Italy in 1861. The list comes from the official postal bulletin published by Institute of postal studies "Aldo Cecchi".

Geographical and climatic variables: We collect and digitize data on elevation and monthly climate conditions. Elevation data are taken from the 1991 Census, while climate data come from WorldClim. The latter include solar radiation, minimum, mean, and maximum temperature, precipitation, water vapor pressure, wind speed, and an index summarizing 19 "bioclimatic" variables. The climate variables refer to October averages over the period 1970–2000. We successfully matched all but 269 municipalities in our sample.

B.1 Education Reforms

Our main analysis considers the first education reform of Unified Italy, the Casati Law enacted in 1861. In 1877, another education reform was passed, the Coppino Law. This law introduced new curricula for civic education, imposed penalties for failure to comply

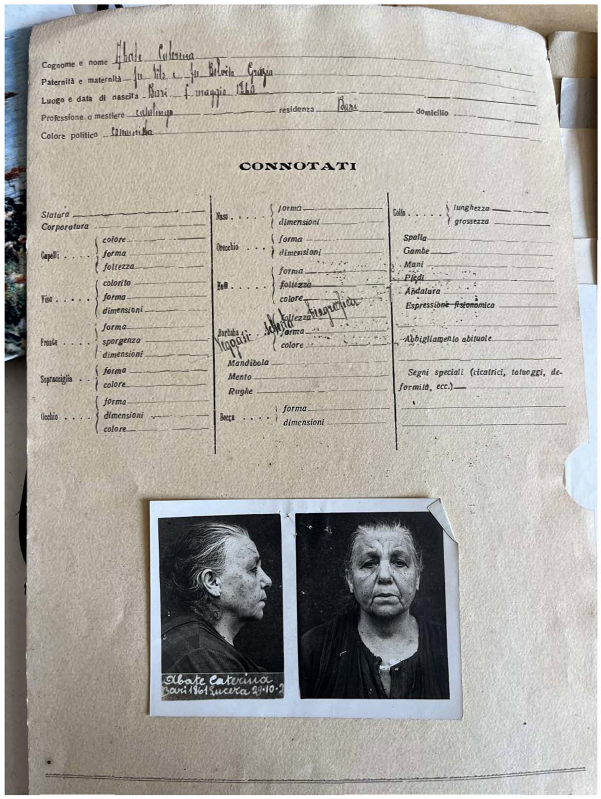
with mandatory education, and further extended compulsory schooling by an additional year in municipalities below 4,000 inhabitants. This discontinuity entails that our treatment becomes one (rather than two) additional year of education for individuals that were seven years old or younger in 1877 (e.g. born in 1870).

The discontinuity in education disappears, instead, in 1890 with the Orlando Law, which extended mandatory education until age 12 to all municipalities, independently on their population.

B.2 A Non-Education (Placebo) Reform:

The Crispi Law mandated that local governments assume responsibility for administering public welfare institutions such as orphanages, shelters, and other charitable organizations. The law established local welfare boards which were tasked with overseeing welfare programs to ensure that aid reached vulnerable groups. These boards were subject to rules: in municipalities with populations exceeding 5,000, the boards were required to have eight members, while smaller municipalities were only required to have three. This provision aimed to ensure that larger populations received more robust administrative support.

Figure C.1: Example of a record of a surveilled



C Descriptives

C.1 Topic Modeling

To uncover the individual traits that were considered important enough to be recorded in the police records on surveilled individuals, we analyze the dossiers collected by Ciancarini (2019) and Carolini and Ecca (2015), for a total of 1,254. While unsupervised topic modeling, like Latent Dirichlet Allocation (LDA) models, is useful for uncovering recurrent themes in text files, it is not well suited to our specific case. Topic models rely on patterns of frequent co-occurrence. In the biographies we have, the co-occurrence are dominated by the bureaucratic and political vocabulary of surveillance (e.g., judicial and legal process). By contrast, descriptions of individual personality traits tend to be highly heterogeneous in wording,

Figure C.2: Number of surveilled by municipality of birth

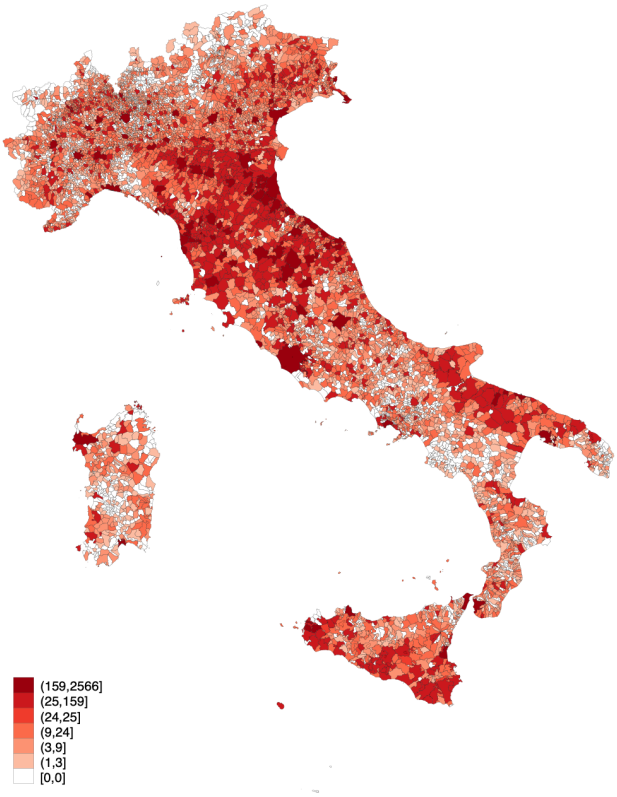
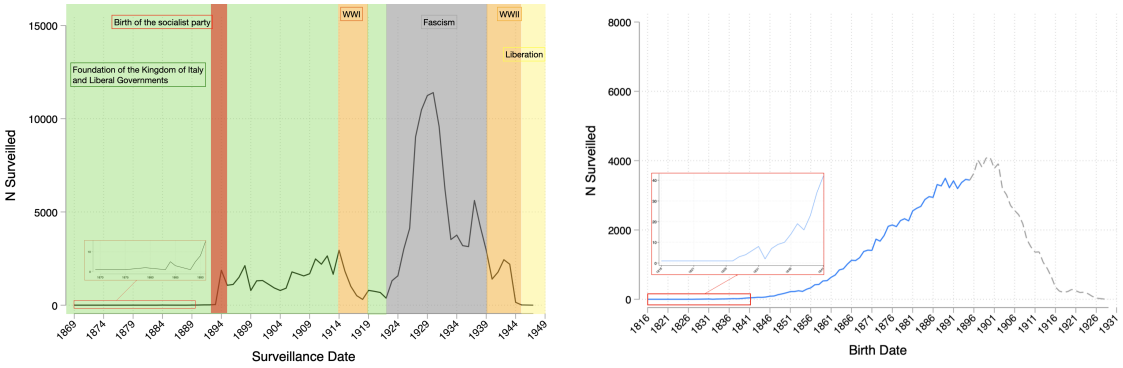


Figure C.3: Number of surveilled

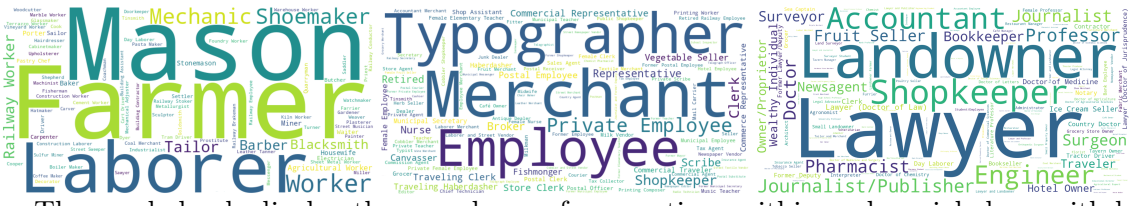


(a) by date of surveillance (b) by date of birth

Notes: (Left) Number of individuals surveilled by the year their file was opened. There are two peaks in surveillance, in correspondence with the founding of the socialist party and with the start of Fascism. The number of surveilled before the first peak is above zero (mean = 7.75). (Right) Number of individuals surveilled by year of birth. The blue solid line shows those born before 1896 (our sample), and the gray dashed line those born after. As discussed in Section **??**, the Orlando law in 1904 raised the compulsory schooling age to twelve in every municipality, removing the previous discontinuity between cities above and below 4,000 inhabitants. Their number decreases because the data on surveillance stops with the end of Fascism, such that younger cohorts are not observed in this data unless they were active during Fascism.

Figure C.4: Jobs distribution within classes

(a) Working Class (b) Middle Class (c) Upper Class



Notes: The word clouds display the prevalence of occupations within each social class, with larger and bolder text representing occupations employing more workers.

Figure C.5: Number of surveilled by class and political ideology

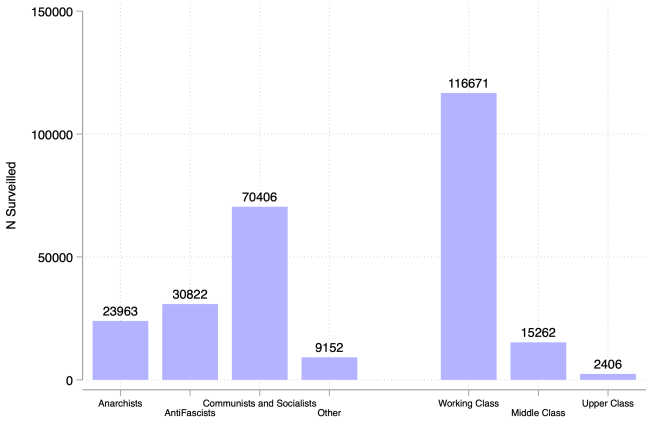
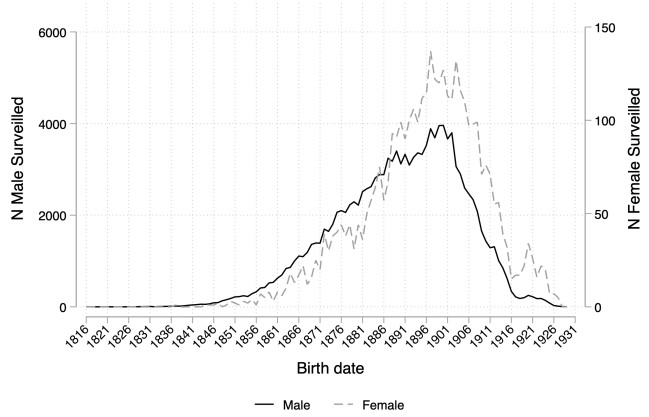


Figure C.6: Number of surveilled by gender



Notes: The figure displays the number of individuals surveilled by birth year and gender. In solid black, the number of male surveilled by date of birth (left axis). In dashed gray, the number of female surveilled by date of birth (right axis).

and often embedded in narrative phrases rather than repeated technical terms. As a result, they do not cluster into coherent latent topics in the same way that institutional or political categories do. For this reason, dictionary-based, embedding, or supervised approaches are more appropriate for extracting information about how individuals' personal characteristics are portrayed in these reports.

To address this issue, we developed a pipeline to process the raw records and extract the relevant traits. First, each biography was split into sentences using spaCy and each was then classified with a large language model (OpenAI GPT-4o, temperature 0) to determine

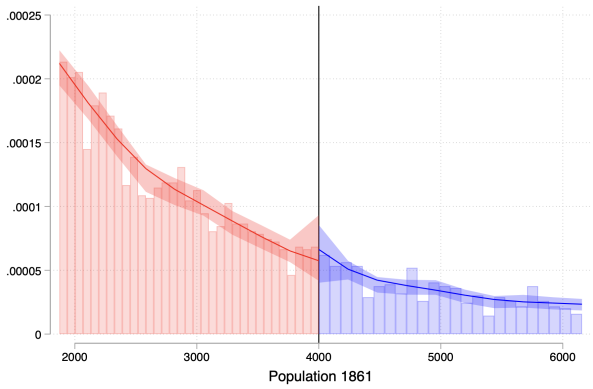
whether they described a personal trait, characteristic, or aspect of personality. For those that did, the trait descriptors were extracted and normalized through lemmatization to reduce variation across synonymous expression. Given the high heterogeneity of the resulting list, we grouped traits into broader clusters using a sentence-transformer model and agglomerative clustering to capture semantic similarity. To focus on meaningful themes, we retained only those clusters appearing in at least five biographies. Finally, a second large language model (OpenAI GPT-4o, temperature) was used to assign human-readable labels to each cluster. This two-step approach makes it possible to capture how police institutions framed individual traits systematically. Table C.1 reports the full list of labels.

Table C.1: Police Records: Topics by cluster

Cluster	N Sentences	N Biographies
Ideological Traits	1075	1036
Character Traits	668	661
Behavioral Traits	497	486
Leadership and Communication Traits	210	207
Educational Traits	136	136
Moral Traits	115	115
Socioeconomic Traits	96	95
Political Traits	91	88
Physical Traits	57	56
Religious Traits	55	54
Psychological Traits	36	36
Criminal Traits	35	35
Gender Traits	24	24
Professional Traits	21	21
Cultural Traits	20	20
Military Traits	19	19
Familial Traits	18	18
National Traits	14	14
Artistic Traits	14	14
Geographical Traits	8	8

D Identifying Assumptions

Figure D.1: Manipulation Testing Plot



Notes: The figure plots the manipulation test at the 4,000 inhabitants cutoff. P-value: 0.796.

Table D.1: Balance Table of the RD sample in fixed and post-treatment outcomes

	Mean T	Mean C	Diff	Std Diff
Minimum Elevation	182.184	159.508	-12.486	-0.095
	(171.413)	(167.527)	(64.936)	
Maximum Elevation	828.189	802.665	-162.232	-0.028
	(641.521)	(641.042)	(245.545)	
Solar radiation (kJ/m^2 day)	10,727.552	10,878.235	-448.329	0.083
	(1,248.738)	(1,327.522)	(490.641)	
Minimum Temperature (°C)	9.942	10.273	-0.477	0.092
	(2.588)	(2.521)	(0.981)	
Maximum Temperature (°C)	17.947	18.381	-0.203	0.125
	(2.475)	(2.436)	(0.941)	
Water Vapor Pressure (kPa)	1.248	1.276	-0.044	0.111
	(0.180)	(0.177)	(0.068)	
Bioclimatic Variables	21.184	21.473	0.026	0.099
	(2.131)	(2.012)	(0.797)	
Precipitation (mm)	98.726	94.551	-6.775	-0.092
	(32.828)	(31.115)	(12.285)	
Average Temperature (°C)	13.944	14.327	-0.341	0.111
	(2.469)	(2.416)	(0.937)	
Wind Speed (m/s)	2.136	2.226	0.144	0.075
	(0.848)	(0.839)	(0.323)	
Observations	915	632	1,547	

Notes: The table reports the mean for the control municipalities (column 1), for treated municipalities (column 2), differences in mean (column 3) and standardized differences (column 4) of a series of geographical and climate outcomes. The analysis control for the population level in 1861 and an interaction term between the population level and a dummy variable *Above4000*, that takes value 1 for municipalities with more than 4,000 inhabitants in the 1861 census. The sample is restricted to municipalities with population between 2,613 and 6,613.

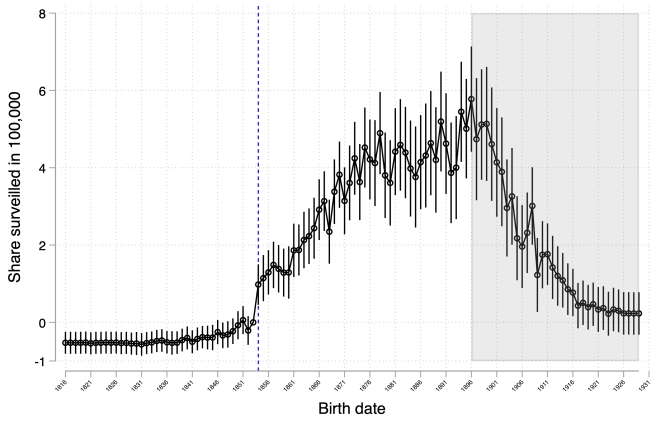
E Robustness to the Main Result

Table E.1: Robustness: functional forms

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Surveilled in 100,000					
	Control	Staggered Adoption	Region FE	Poisson	Order 1	Order 2
Post × Above4000	3.335*	3.275***	3.211***	1.251***	3.515***	3.173***
	(1.980)	(0.284)	(0.312)	(0.0123)	(0.318)	(0.425)
Observations	617,040	701,600	617,040	425,520	617,040	617,040
Municipality FE	Y	Y	Y	Y	Y	Y
Birth FE	-	-	-	Y	Y	Y
Reign x Birth FE	Y	Y	-	-	Y	Y
Region x Birth FE	-	-	Y	-	-	-
Mean DV	5.499	4.830	5.499	5.499	5.499	5.499

Notes: Robustness to DDRD results as in Equation 1. Column (1) controls for the number of individuals in each cohort who begin surveillance in a given decade. Specifically, for each decade from 1869 to 1947, we construct a variable equal to the number of individuals from that cohort who start surveillance during that decade. Column (2) accounts for places that were annexed to the Kingdom of Italy after 1861 and that implemented the Casati Law following their annexation. The staggered adoption is as follows: Friuli Venezia Giulia, Veneto, and the province of Mantova enacted the law in 1866 when they were annexed to the Kingdom of Italy; Lazio followed in 1870. The rest of Italy adopted the reform in 1861. We exclude Trieste, which was annexed in 1954. Column (3) introduces region times birth-year fixed effects instead of reign times birth-year. Column (4) considers a poisson specification. Columns (5) -(6) consider first and second-order polynomials for the interaction of population, treatment, and post. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Figure E.1: Effect of the Education Reform, during and after the Discontinuity Applies on Surveilled in 100,000 (Extended Event Study)



Notes: Event study estimates from Equation 1 as in Figure 3a for each year of birth from 1816 to 1929 (rather than stopping at cohort 1896). From cohort 1896, the education reform applies to all municipalities and the discontinuity stops applying. The reference cohort is 1853 (one year before treatment). Regressions include municipality and reign × birth-year fixed effects; standard errors are clustered at the municipality level.

Table E.2: Robustness: Alternative Dependent Variables

	(1)	(2)	(3)	(4)
	Surveilled			
	Any	N	Winsor	Ln
Post × Above4000	0.276***	0.00788	0.367***	0.208***
	(0.0145)	(0.200)	(0.0307)	(0.0342)
Observations	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	0.00320	0.160	0.147	0.0980

Notes: Robustness to DDRD results as in Equation 1 where we vary the definition of the dependent variable. Column (1) considers a dummy equal to 1 if a city has at least one surveilled born in that year; column (2) the absolute number of surveilled; column (3) the winsorized number of surveilled; column (4) the natural logarithm of the number of surveilled. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Table E.3: Effect of the education reform, Double DDRD, RD and DiD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Share Surveilled in 100,000					
		RD			DD			Double DDRD	
Above4000	3.729***	3.729***	3.831***						
	(0.359)	(0.359)	(0.365)						
Post × Above4000				3.128***	3.013***	2.435***	3.729***	3.605***	3.514***
				(0.258)	(0.251)	(0.216)	(0.359)	(0.350)	(0.318)
Observations	617,040	617,040	617,040	617,040	617,040	617,040	617,040	617,040	617,040
Effective Observations	323,946	323,946	323,946						
Municipality FE	-	-	-	N	Y	Y	N	Y	Y
Birth FE	N	Y	-	N	Y	-	Y	Y	-
Reign x Birth FE	N	N	Y	N	N	Y	N	N	Y
Mean DV	5.499	5.499	5.499	5.499	5.499	5.499	5.499	5.499	5.499

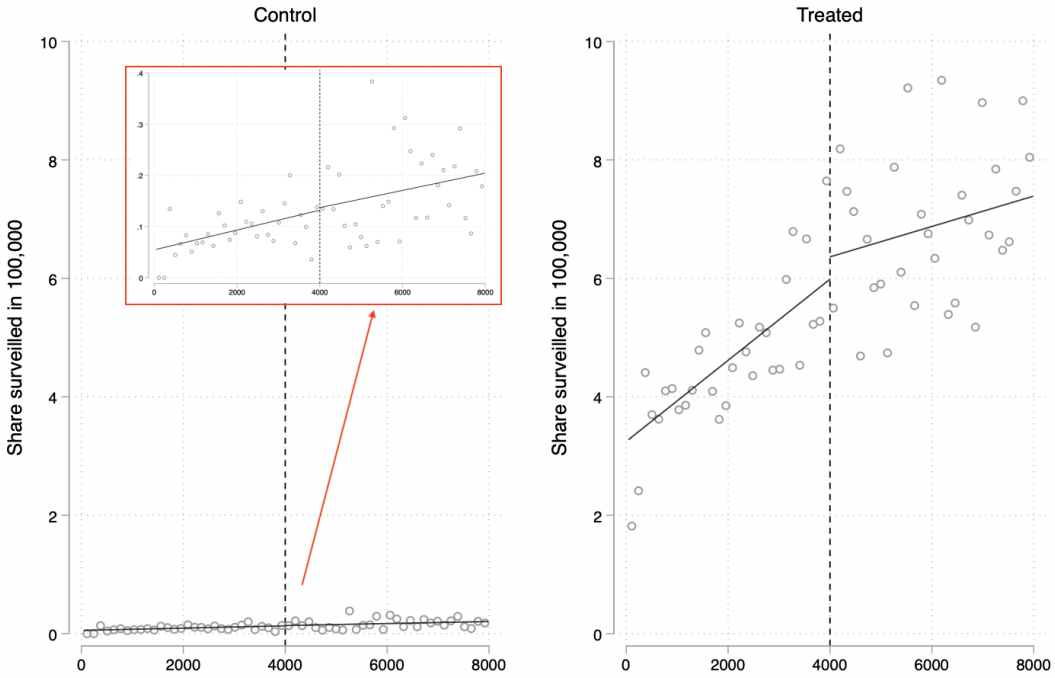
Notes: Columns (1)-(3) report the RD Calonico, Cattaneo and Titiunik (2015) results showing the effect of being above the 4,000 population discontinuity on the number of individuals surveilled. Columns (4)-(6) report the DiD results of the effect of the reform on the number of surveilled per 100,000 inhabitants in a municipality-cohort. We do not control flexibly for population around the cutoff as in the main specification in Equation 1. Columns (7)-(9) show the "Double" Difference-in-Discontinuity results as in Equation 1 with additional controls for the flexible impact of treatment across cohorts, allowing to isolate the effect of a possible jump caused by cohorts being exposed longer to the reform. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Table E.4: Placebo population discontinuity (below threshold)

	(1)	(2)	(3)
	Above 1000	Above 2000	Above 3000
Post × Above	0.841	1.266	-0.937
	(0.566)	(1.280)	(3.627)
Observations	565,506	565,506	565,506
Municipality FE	Y	Y	Y
Reign x Birth FE	Y	Y	Y
Mean DV	4.047	3.751	4.419
Population cutoff	1000	2000	3000

Notes: DDRD results as in Equation 1 but in municipalities with alternative (placebo) population cutoffs below the 4,000 inhabitants in 1861 (all control units). We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Figure E.2: Effect of the education reform (RDD)



Notes: RD Calonico, Cattaneo and Titiunik (2015) results showing the effect of being above the 4,000 population discontinuity on the number of individuals surveilled. We subset the analyses for cohorts unexposed (left) and exposed (right) to treatment. We adopt the same y-axis for comparison, but zoom into the (lack of a) discontinuity in control cohorts to show the pattern across the threshold.

Table E.5: Placebo population discontinuity (above threshold)

	(1) Correct Cutoff	(2) Above 6000	(3) Above 8000	(4) Above 10000	(5) Above 12000	(6) Above 14000	(7) Above 16000	(8) Above 18000	(9) Above 20000	(10) Above 25000	(11) Above 30000
Post × Above	2.708***	0.230	-3.294	1.305	7.074	-18.74	-24.29	47.90	22.60	-48.37	-28.35
	(0.840)	(2.867)	(3.295)	(4.592)	(12.11)	(16.86)	(19.54)	(35.67)	(21.42)	(42.99)	(27.14)
Observations	593,520	83,920	83,920	58,960	31,920	21,518	14,558	10,238	7,920	2,880	1,877
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Window	0-10k	4k-14k	4k-14k	5k-15k	7k-17k	9k-19k	11k-21k	13k-23k	15k-25k	20k-30k	25k-25k

Notes: DDRD results as in Equation 1 but in municipalities with alternative (placebo) population cutoffs above the 4,000 inhabitants in 1861 (all treated units). In all regressions, we consider a window of 10,000 inhabitants around the cutoff. For the correct cutoff (4,000 inhabitants), we use observations from 0 to 10,000 inhabitants. For placebo cutoffs, we consider 10,000 inhabitants windows around the placebo cutoffs, but including only treated units (e.g., municipalities above 4,000 inhabitants). We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level. Standard errors are clustered at the municipal level.

Table E.6: Placebo Cohorts

	(1)	(2)	(3)	(4)	(5)
	Share Surveilled in 100,000				
	Treat: 1826	Treat: 1831	Treat: 1836	Treat: 1841	Treat: 1846
Post × Above4000	1.432	0.314	-9.819	-32.51	-39.24
	(6.485)	(8.484)	(15.92)	(22.97)	(35.04)
Observations	300,807	300,807	300,807	300,807	300,807
City FE	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y

Notes: DDRD results as in Equation 1 but for alternative (placebo) cohorts that are not treated. We consider cohorts between a certain threshold and that threshold plus 10. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

F Additional Analyses on Mechanism

Table F.1: Effect of the Education Reform on Surveillance, by Class (Dummy)

	(1)	(2)	(3)
	Any Surveilled		
	Working class	Middle class	Upper class
Post × Above4000	0.239***	0.0415***	0.00357
	(0.0142)	(0.0114)	(0.00371)
Observations	617,040	617,040	617,040
Municipality FE	Y	Y	Y
Reign x Birth FE	Y	Y	Y
Mean DV	0.108	0.0153	0.00340
H_0: Working class <		0	0

Notes: DDRD results as in Equation 1 where the dependent variable is a dummy = 1 if a municipality has at least one surveilled born in that year who is from a given class. The We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform. Standard errors are clustered at the municipal level.

Table F.2: Effect of the Education Reform on Political Confinement, by class

	(1)	(2)	(3)	(4)
	Share Sentenced to Confinment in 100,000			
	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	0.867***	0.748***	0.108***	0.0107
	(0.0969)	(0.0897)	(0.0187)	(0.00849)
Observations	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	1.598	1.441	0.131	0.0284
H_0: Working Class <			0	0

Notes: DDRD results as in Equation 1 where the dependent variable is the number of individuals sentenced to political confinement (*confino*) in 100,000 inhabitants by class. We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform. Standard errors are clustered at the municipal level.

Table F.3: Effect of the Education Reform on Duration of Surveillance, by class

	(1)	(2)	(3)	(4)
	Share of the year subject to surveillance			
	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	5.185***	4.397***	0.824***	0.0714
	(0.317)	(0.300)	(0.233)	(0.0805)
Observations	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	2.086	1.737	0.287	0.0639
Mean days surveilled	761.5	634	104.6	23.32
H_0: Working Class <			0	0

Notes: DDRD results as in Equation 1 where the dependent variable is the number of years a municipality-cohort spends under surveillance. Column (1) shows the results for the entire sample; Columns(2)-(4) by class. We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform. We translate this share in average number of days in the bottom of the table. Standard errors are clustered at the municipal level.

Table F.4: Effect of the Education Reform on quantity of information collected, by class

	(1)	(2)	(3)	(4)
	Share Surveilled with biographical file in 100,000			
	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	1.361***	1.096***	0.234***	0.0308**
	(0.134)	(0.116)	(0.0282)	(0.0129)
Observations	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	1.169	0.935	0.196	0.0441
H_0: Working Class <			0	0

Notes: DDRD results as in Equation 1 where the dependent variable is the number of individuals surveilled with recorded biographical information in 100,000 inhabitants. Column (1) shows the results for the whole sample; columns (2)-(4) by class. We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform. Standard errors are clustered at the municipal level.

Table F.5: Effect of the Education Reform on Surveillance, by ideology

	(1)	(2)	(3)	(4)	(5)
	Share Surveilled in 100,000				
	Anarchists	Anti-Fascists	Communists and Socialists	Republicans	Others
Post × Above4000	0.836***	0.418***	2.065***	0.162***	0.0526*
	(0.148)	(0.0568)	(0.161)	(0.0410)	(0.0285)
Observations	617,040	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y
Mean DV	1.169	0.875	3.303	0.138	0.204

Notes: DDRD results as in Equation 1 subsetting by ideology of the surveilled. The category *Others* is a residual group that includes surveilled individuals whose ideology is recorded as anti-nationalists, syndicalists, masons, and other similar affiliations. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Table F.6: Effect of the Education Reform on Surveillance, by governing party

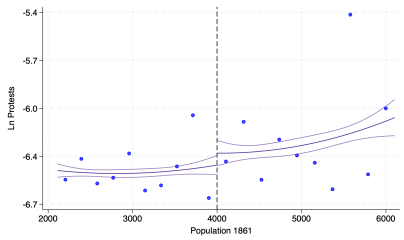
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share Surveilled Historical Right				Share Surveilled Historical Left			
	Overall	Working Class	Middle Class	Upper Class	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	0.186***	0.149***	0.0301***	0.00655*	0.763***	0.656***	0.105***	0.00222
	(0.0415)	(0.0390)	(0.00905)	(0.00386)	(0.0884)	(0.0773)	(0.0213)	(0.0113)
Observations	617,040	617,040	617,040	617,040	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	0.284	0.253	0.0287	0.00430	0.751	0.623	0.102	0.0317
H_0: Working Class <			0.00144	0.000126			0.00144	0.000126

Notes: DDRD results as in Equation 1 subsetting by governing party at the start of surveillance. Columns (1)-(4) focus on individuals who begin surveillance under the historical right (1862–1876; 1896; 1906; 1910); columns (5)-(8) on individuals who begin surveillance under the historical left (1877–1895; 1899–1905; 1899-1905; 1907–1909; 1911–1914). Individuals who begin surveillance under coalition governments are not included. We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform . Standard errors are clustered at the municipality level. Results for individuals who begin surveillance during the fascist period is reported in columns (5)-(8) in Table H.1.

G Additional Analyses on Individual Political Engagement

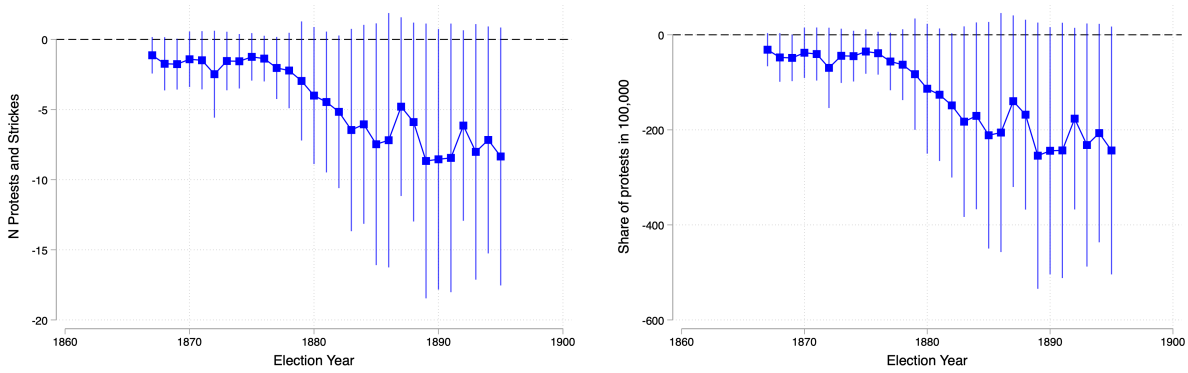
Figure G.1: Effect of the education reform on political engagement (RDD)

(a) Protests (b) Partisans (c) Banditism



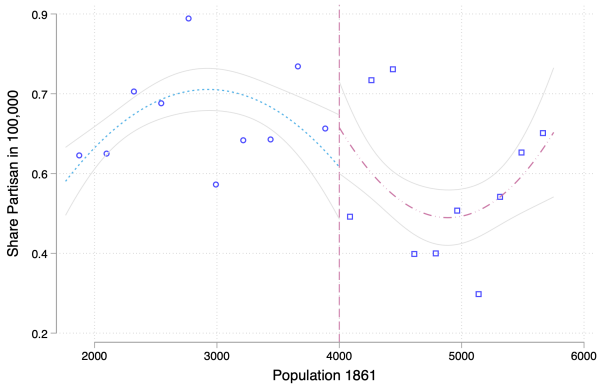
[width=0.33]figures/partisan˙rdplot.png[width=0.33]figures/briganti˙rdplot.

Figure G.2: Effect of the 4,000 cutoff on Protests (RD), by year



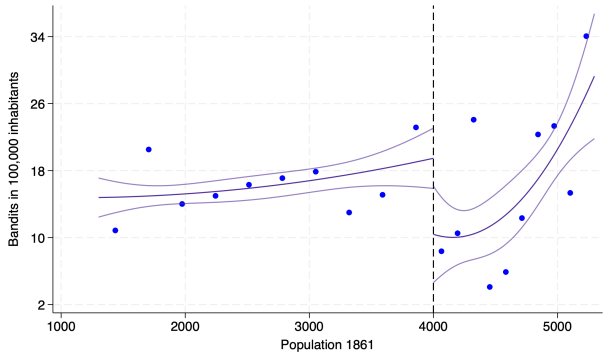
Notes: RD (Calonico, Cattaneo and Titiunik, 2015) results showing the effect of being above the 4,000 population discontinuity on the number of protests (left) and the share of protests in 100,000 (right). All regressions include region fixed effects. Standard errors are clustered at the municipal level.

Figure G.3: Effect of the education reform on partisans (RDD)



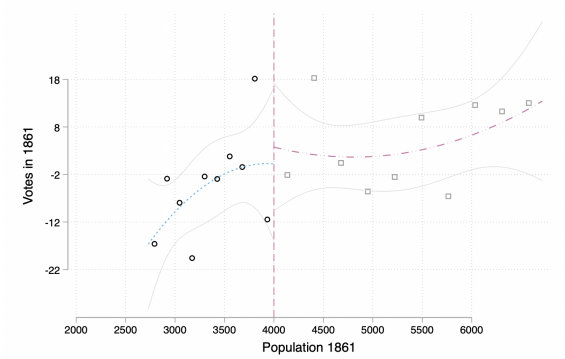
Notes: RD results showing the effect of being above the 4,000 population discontinuity on the number of partisan.The regression includes region fixed effects. Standard errors are clustered at the municipal level.

Figure G.4: Effect of the education reform on banditry (RDD)



Notes: Effect of being in populations above the 4,000 cutoff on the share of bandits in 100,000 inhabitants. The regression includes region fixed effects. Standard errors are clustered at the municipal level.

Figure G.5: Effect of the 4,000 cutoff on voter mobilization in the 1861 national election (RDD)



Notes: RD Calonico, Cattaneo and Titiunik (2015) results showing the effect of being above the 4,000 population discontinuity on the absolute number of votes cast in each election year. Individuals who were 7 years or younger at the time of the reform voted for the first time in 1880. Standard errors are clustered at the municipal level.

Table G.1: Effect of the education reform on votes in national elections (RDD and DDRD)

	(1)	(2)	(3)	(4)
	N votes	Votes over 1861 pop	N votes	Votes over 1861 pop
Above4000 (RDD)	11.997	3.363		
	(11.094)	(4.729)		
Post × Treat (DDRD)			-28.04	-42.19***
			(125.4)	(6.724)
Observations	189,422	189,422	224,529	224,529
Bandwidth	3470.96	921.56		
N Left	148538	148538		
N Right	40884	40884		
Robust P-val	0.180	0.830		
Election Year FE	Y	Y	Y	Y
Reign FE	Y	Y		
Mean DV	157.7	106.2	157.7	106.2
Municipality FE			Y	Y
Reign x Election Year FE			Y	Y

Notes: RD Calonico, Cattaneo and Titiunik (2015) (columns 1-2) and DDRD (columns 3-4) results showing the effect of being above the 4,000 population discontinuity on votes in national elections. We use the absolute (column 1-3), and 1861 population share (column 2-4) vote count. In column 1-2, we subset the analyses to election years from 1880 onward, when the first treated cohort gained the right to vote (Figure G.5 shows there is no effect in the pre-period). Here, we control for region and election year fixed effects. In columns 3-4, we use the same DDRD specification as in the main analysis (Equation 1), but with election-year as time variable. We report the mean of the dependent variable for cities with population just below 4,000 in 1861. Standard errors are clustered at the municipal level.

Table G.2: Effect of the Education Reform on notable individuals from Wikidata

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		Non Politics		Politics		Non Religious Figures	
	Share	Total	Share	Total	Share	Total	Share	Total
Post × Above4000	-0.294*	-0.0869**	-0.170**	0.0134***	-0.236***	0.00345	-0.184**	-0.000292
	(0.159)	(0.0361)	(0.0790)	(0.00256)	(0.0860)	(0.00398)	(0.0805)	(0.00310)
Observations	328,480	328,480	328,480	328,480	328,480	328,480	328,480	328,480
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	0.800	0.0192	0.261	0.00590	0.305	0.00710	0.271	0.00620

Notes: DDRD results as in Equation 1 where the dependent variable is the number of notable individuals included in Wikidata per 100,000 inhabitants. Columns 1-2 report the effect on all notable individuals. Columns 3-4 focus on non-political occupations. Columns 5-6 consider political occupations, while columns 7-8 exclude religious figures (see Section B for variable construction). We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Table G.3: Effect of the 4,000 cutoff on Protests (RD)

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Protests in 100,000		Winsorized		Ln Protests	
Above4000	-106.0	-120.0*	-13.37	-15.64	0.0798	0.0495
	(65.50)	(71.35)	(14.94)	(15.98)	(0.189)	(0.198)
Observations	291,609	291,609	291,609	291,609	294,872	294,872
Bandwidth	1450.32	2448.45	1549.55	2803.30	2108.38	4050.57
N Left	256055	256055	256055	256055	259318	259318
N Right	35554	35554	35554	35554	35554	35554
Robust P-val	0.160	0.046	0.386	0.186	0.794	0.772
Mean DV	109.299	109.299	20.827	20.827	-6.462	-6.462
Polynomial	1	2	1	2	1	2

Notes: RD (Calonico, Cattaneo and Titiunik, 2015) results showing the effect of being above the 4,000 population discontinuity on the number of protests. Columns (1)-(2) shows the effect for the number of protests in 100,000 inhabitants; columns (3)-(4) the winsorized number of protests; columns (5)-(6) the natural logarithm of the number of protests. We report the mean of the dependent variable in the sample. Standard errors are clustered at the municipal level.

Table G.4: Effect of the Education Reform on partisans activity (RDD and DDRD)

	(1)	(2)	(3)	(4)
	RDD		DDRD	
	Share	Total	Share	Total
Above4000	-0.087	-0.006		
	(0.273)	(0.010)		
Post × Above4000			-0.0973	-0.0723***
			(0.151)	(0.0258)
Observations	323,946	323,946	617,040	617,040
Bandwidth	1762.076	2051.634		
N Left	272412	272412		
N Right	51534	51534		
Robust P-val	0.931	0.719		
Conventional P-val	0.751	0.565		
Municipality FE			Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	5.425	0.0407	1.304	0.0407

Notes: RD (Calonico, Cattaneo and Titiunik, 2015) and DDRD results as in Equation 1 where the dependent variable is the share of partisans in 100,000, as in the main surveillance specification (columns 1-3) and the total number of partisans (columns 2-4). We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Table G.5: Effect of the Education Reform on notable individuals from Wikidata, by surveillance level before the reform

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		Non Politics		Politics	
	Share	Total	Share	Total	Share	Total
Post × Above4000	-0.301*	-0.0907**	-0.176**	0.0140***	-0.241***	0.00363
	(0.160)	(0.0386)	(0.0791)	(0.00267)	(0.0861)	(0.00421)
Post × Above4000 × HighSurveillance	0.0470	0.0254	0.0377	-0.00392	0.0360	-0.00126
	(0.0917)	(0.0173)	(0.0513)	(0.00318)	(0.0534)	(0.00361)
Observations	328,480	328,480	328,480	328,480	328,480	328,480
Municipality FE	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y
Mean DV	0.738	0.0183	0.241	0.00560	0.284	0.00690

Notes: DDRD results as in Equation 1 where the dependent variable is the number of notable individuals included in Wikidata per 100,000 inhabitants. Columns (1) and (4) report the effect on all notable individuals. Columns (2) and (5) focus on non-political occupations, while columns (3) and (6) consider political occupations (see Section B for variable construction). *HighSurveillance* takes value 1 if the median number of individuals surveilled from untreated cohorts in a municipality is larger than the median at the national level. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Table G.6: Effect on notable individuals from Wikidata, by no pre-reform surveillance

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		Non Politics		Politics	
	Share	Total	Share	Total	Share	Total
Post × Above4000	-0.144	-0.0949*	-0.135	0.0237***	-0.177*	0.0130**
	(0.172)	(0.0558)	(0.0834)	(0.00417)	(0.0909)	(0.00615)
Post × Above4000 × NoSurveillance	-0.202***	0.0108	-0.0479	-0.0138***	-0.0790**	-0.0128***
	(0.0722)	(0.0274)	(0.0343)	(0.00351)	(0.0374)	(0.00429)
Observations	328,480	328,480	328,480	328,480	328,480	328,480
Municipality FE	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y
Mean DV	1.164	0.0292	0.395	0.00930	0.449	0.0106

Notes: DDRD results as in Equation 1 where the dependent variable is the number of notable individuals included in Wikidata per 100,000 inhabitants. Columns (1) and (2) report the effect on all notable individuals. Columns (3) and (4) focus on non-political occupations, while columns (5) and (6) consider political occupations (see Section B for variable construction). *NoSurveillance* takes value 1 if there were no individuals surveilled from untreated cohorts in a municipality. We report the mean of the dependent variable for cities with population just below 4,000 in 1861. Standard errors are clustered at the municipal level.

Table G.7: Effect of the Education Reform on partisans activity, by pre-reform surveillance level

	(1)	(2)	(3)	(4)
	Partisans			
	Share	Total	Share	Total
Post × Above4000	-0.0731	-0.0754***	0.0617	-0.0669*
	(0.151)	(0.0274)	(0.185)	(0.0380)
Post × Above4000 × HighSurveillance	-0.158	0.0205		
	(0.123)	(0.0131)		
Post × Above4000 × NoSurveillance			-0.213*	-0.00728
			(0.117)	(0.0199)
Observations	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	1.119	0.0354	2.760	0.0842

Notes: DDRD results as in Equation 1 where the dependent variable is the share of partisans in 100,000, as in the main surveillance specification (columns 1-3) and the total number of partisans (columns 2-4). *HighSurveillance* takes value 1 if the median number of individuals surveilled from untreated cohorts in a municipality is larger than the median at the national level. *NoSurveillance* takes value 1 if there were no individuals surveilled from untreated cohorts in a municipality. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

H Additional Heterogeneities

Table H.1: Effect of the Education Reform on Surveillance, by Democracy and Dictatorship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share Surveilled before 1922				Share Surveilled after 1922			
	Overall	Working Class	Middle Class	Upper Class	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	1.831***	1.540***	0.267***	0.0240	1.683***	1.333***	0.307***	0.0433***
	(0.199)	(0.178)	(0.0344)	(0.0148)	(0.164)	(0.147)	(0.0317)	(0.0120)
Observations	617,040	617,040	617,040	617,040	470,493	470,493	470,493	470,493
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	2.019	1.696	0.252	0.0534	3.373	2.991	0.308	0.0676
H_0: Working Class <			0	0			0	0

Notes: DDRD results as in Equation 1 subsetting by start of surveillance. Columns (1)-(4) focus on individuals who begin surveillance before 1922; columns (5)-(8) on individuals who begin surveillance after 1922 –included. We report the mean of the dependent variable for municipalities just below 4,000 inhabitants in 1861 after the reform. Table F.6 reports the results by governing party before 1922.

Table H.2: Heterogeneity by state capacity and technology

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Surveilled in 100,000					Surveilled Post Wiretapping
			North	Center	South	Surveilled Pre Wiretapping
Post × Above4000	2.855***	2.866***	5.279***	8.502***	1.651***	0.177
	(0.359)	(0.335)	(0.521)	(1.094)	(0.243)	(0.170)
Post × Above4000 × Any Post Office	1.203***					
	(0.427)					
Post × Above4000 × N Post Offices		1.185***				
		(0.441)				
Observations	617,040	617,040	351,920	65,120	200,000	617,040
Municipality FE	Y	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	N	N	N	Y
Mean DV	5.077	5.077	7.211	10.97	2.590	1.742
Δ over mean			0.732	0.775	0.637	

Notes: Heterogeneity to DDRD results as in Equation 1. Columns (1) and (2) subset by the presence of a post office. *Any Post Office* is equal to 1 if there is at least one working post office in that municipality in 1861; *N Post Offices* is a continuous variable that corresponds to the number of working post offices in that municipality. Columns(3)-(5) subset by the geographical area a surveilled is born in. In columns (6) the dependent variable is the difference in share surveilled in 100,000 before and after the introduction of wiretapping technology. As wiretapping introduction in 1916 affected all cohorts, we examine the effect on differential surveillance rates, comparing the same cohorts before and after 1916 to isolate how this technological change interacted with the education reform. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Table H.3: Heterogeneity by pre-reform level of surveillance

	(1)	(2)
	Share Surveilled in 100,000	
Post × Above4000	2.892***	7.158***
	(0.336)	(0.566)
Post × Above4000#HighSurveillance	4.083***	
	(0.788)	
Post × Above4000#NoSurveillance		-4.895***
		(0.544)
Observations	617,040	617,040
Municipality FE	Y	Y
Reign x Birth FE	Y	Y
Mean DV	0.0824	0.0824

Notes: Heterogeneity to DDRD results as in Equation 1 by pre-reform levels of surveillance. *HighSurveillance* takes value 1 if a municipality has a high level of surveillance before the reform. A municipality is classified as having a high level of pre-reform surveillance if its average surveillance level before the reform is greater than or equal to the median pre-reform surveillance level. *NoSurveillance* takes value 1 if there were no individuals surveilled from untreated cohorts in a municipality. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

I Alternative Interpretations for the Main Result

Table I.1: Effect of the Education Reform on non-migrants and migrants

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Surveilled in 100,000					
	Stayers			Movers		
Post × Above4000	2.100***	2.036***	2.075***	1.479***	1.429***	1.433***
	(0.218)	(0.212)	(0.194)	(0.134)	(0.131)	(0.120)
Observations	617,040	617,040	617,040	617,040	617,040	617,040
Municipality FE	N	Y	Y	N	Y	Y
Reign x Birth FE	N	N	Y	N	N	Y
Mean DV	2.694	2.694	2.694	1.980	1.980	1.980

Notes: DDRD results as in Equation 1 subsetting by non-migrants, individuals for which the registered birth and residence locations are the same (columns 1-3) and migrants, for which the registered birth and residence locations differ (columns 4-6). We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Table I.2: Placebo Reform - More Bureaucrats but Not More Schools

	(1)	(2)	(3)
	Share Surveilled in 100.00		
Post × Above5000	8.896	8.894	8.894
	(5.454)	(5.454)	(5.454)
Observations	95,827	95,827	95,827
Municipality FE	N	Y	Y
Year FE	Y	Y	-
Reign x Year FE	N	N	Y
Mean DV	7.301	7.301	7.301

Notes: DDRD results as in Equation 1 of the effect of the Crispi reform enacted in 1890 in municipalities above 4,000 inhabitants on the share of surveilled in 100,000. The database is at the municipality and year of start of surveillance level. Post is a dummy equal to one after 1890. Municipalities with less than 4,000 inhabitants in 1861 are not included in the analysis. We report the mean of the dependent variable for cities with population just below 5,000 in 1861 after the reform. Standard errors are clustered at the municipal level.

Table I.3: Effect of the Education Reform, by different professions

	(1) N Teachers	(2) Share Teachers	(3) Share Shop Owners	(4) Share Lawyers	(5) Share Tailors
Post × Above4000	-0.000823	0.00474***	0.00376***	0.00700***	0.00720***
	(0.00263)	(0.000499)	(0.000382)	(0.000587)	(0.000535)
Observations	617,040	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y
Mean DV	0.00160	0.00140	0.00150	0.00130	0.00290

Notes: DDRD results as in Equation 1 on the share of surveilled in 100,000 by profession. Column (1) considers the absolute number of teachers; column (2) the share of teachers in 100,000; columns (3)-(5) the share of other professions (shop owners, lawyers and tailors) surveilled at similar rates than teachers pre-reform. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipality level.

Table I.4: Effect of the Education Reform, by age

	(1) (1) 0-17	(2) (2) 18-25	(3) (3) 26-35	(4) (4) 36-45	(5) (5) 45 and more
Post × Above4000	0.00261***	0.0635***	0.105***	0.106***	0.0743***
	(0.000896)	(0.00757)	(0.0107)	(0.0110)	(0.00926)
Observations	617,040	617,040	617,040	617,040	617,040
Municipality FE	Y	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y	Y
Mean DV	0.00250	0.0582	0.128	0.183	0.161

Notes: DDRD results as in Equation 1on the share of surveilled in 100,000 by age. We report the mean of the dependent variable for cities with population just below 4,000 in 1861 after the reform. Standard errors are clustered at the municipal level.