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 and age threshold, we show that cohorts exposed to more years of school experienced an uptick in surveillance. The effect is largest for working classes, who 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, indicating that surveillance expanded not in reaction to increased mobilization, but as a preventive strategy rooted in fears of working-class empowerment. These findings reveal how states view educated yet excluded groups as politically threatening and prioritize their surveillance, potentially generating inequalities in groups' ability to influence political change.

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"For twenty years, we must stop this brain from functioning." – Michele Isgrò, 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 they see as threats to their stability: those with both the capacity to mobilize and to seek radical change. We draw on two literatures: 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 overturn existing regimes. Qualitatively, surveillance reports from the Italian archives frequently flag 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 literatures, we propose 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 that mandated two additional years of primary schooling (third and fourth) in municipalities with populations above 4,000. A simple regression discontinuity would be compounded, because other laws also change discretely at this cutoff. Instead, we exploit the interaction of the municipal cutoff with a cohort discontinuity: children who were 7 years old at implementation (i.e., had completed at most second grade) were exposed to

[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).

the reform, while older cohorts were not. Treatment is thus determined by birth year, not by the calendar year of enactment or the timing of subsequent surveillance. Our identification follows a difference-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 just above the cutoff. This design rules out confounders unless they vary both at the municipal and cohort threshold. For example, if municipalities above 4,000 received additional funds, and those were used to facilitate surveillance, these improvements would need to enhance surveillance specifically for the treated cohorts, not for all residents. While this design rules out a range of confounders, we also examine a host of plausible alternative explanations.

We start by documenting 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 thirty-eight pre-reform cohorts, consistent with the parallel-trends assumption. Importantly, when a later reform removed the 4,000-inhabitant education discontinuity and standardized the educational requirements 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 population and year of birth cutoffs.

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 increased attention to educated working classes persists after technological innovations in surveillance, underscoring the relevance of our findings based on historical data 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.

We propose two possible interpretations for this null result. The first is that, in this context, education did not increase political participation, consistent with recent research showing that education does not always foster dissent, particularly when designed to transmit official narratives (Cantoni et al., 2017; Paglayan, 2024). The second is that participation

was suppressed precisely by state preventive surveillance efforts – although, descriptively, we do not find differences in political engagement between cohorts exposed to surveillance and those who were not. While we cannot decisively adjudicate between these two possibilities, both underscore surveillance as a preventive tool, deployed to identify potential revolutionary entrepreneurs among groups perceived as threats to state stability.

Finally, we rule out alternative mechanisms operating at the municipality–cohort level that could independently trigger surveillance. 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. We also 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-

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.

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 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: The first surveillance files were written even before Italy existed: in the 1820s, the fragmented kingdoms of pre-Unitary Italy confronted waves of uprisings that culminated in the independence movements and revolutions of 1848. The largest states, the Kingdom of Sardinia–Piedmont and the Bourbon monarchy, responded by building Napoleonic-style political police: centralized offices that cultivated spies and informants and closely monitored public spaces (Di Fiore, 2018). After unification in 1861, the Kingdom of Italy inherited and expanded these practices. Rising socialist and anarchist mobilization in the late nineteenth century prompted the Interior Ministry to intensify oversight and, in 1894, to create a national registry of suspects by consolidating local dossiers that predated unification (Tosatti, 1990, pp. 447–49). World War I further expanded state capacity for surveillance through the introduction of wiretapping technologies. Under Mussolini, surveillance became more centralized and pervasive. Drawing inspiration from the Soviet Cheka, the Fascist 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. In absence of comprehensive digital archives of these narratives, 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 education 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."* [2]

Alongside education, the state systematically recorded each subject's profession. As shown by the archival excerpts and detailed in the data section, profession was recorded for 100% of individuals under surveillance, underscoring that this attribute was fundamental to the state, on par with documenting dissidents' ideology.

LLM classification: We analyze these narratives more systematically using a machine-learning pipeline to extract the individual traits of the surveilled. We first segment each biography into sentences and use an LLM (OpenAI GPT-4o; temperature=0, to maximize

[2] 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.

reproducibility) to flag sentences that describe a personal trait, characteristic, or aspect of personality. For the flagged sentences, we extract trait descriptors and cluster them by semantic similarity. We then assign human-readable labels to each cluster by prompting the LLM for concise descriptions (temperature =0). Out of the 25 categories produced by the model, we exclude singletons and merge overlapping descriptors into broader groups, obtaining twelve categories. The topic generation process is fully unsupervised – we do not provide the LLM with examples of typical traits of the surveilled. We include more details about this process in Section B.1.

Table 1a lists the twelve individual traits emerging from this pipeline. As expected, the police records detail subjects' ideology and political activity, as well as physical and behavioral traits helpful for identification and for communication on their actions (e.g., disobeying orders, rule breaking behavior). The state also emphasized individuals' leadership and communication skills, highlighting its concern for the mobilization capacity of the surveilled. The next most cited topic identified by the LLM is the education of the surveilled, including individuals' level of schooling, general culture, and intelligence. Socioeconomic characteristics – status, social background, upbringing, profession – follow suit.

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 A 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 B.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. Our empirical strategy leverages a discontinuity ending for those born after 1896, so the sample used in the main analysis 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. Surveillance 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 B.2 presents spatial variation in the number of individuals surveilled in 100,000 inhabitants: 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 show the number of surveilled by date of surveillance (Figure B.3a), and the full distribution of surveilled by date of birth, including those outside our study period (Figure B.3b).

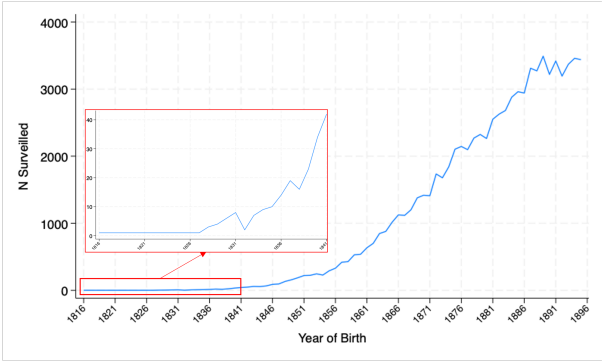
Class: The archive contains information on the profession of each individual surveilled, which we use to categorize subjects into upper, middle, and working class. Because the social meaning of professions changed over time (some occupations slid up or down the status ladder), we adopt the historical 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 to classify professions. First, we prompt a LLM classifier (GPT-4o, temperature 0.3)

Figure 1: Descriptive statistics on the surveilled

(a) Police narratives: Individual traits

Cluster	N	Share	Description
Ideology	1137	94.4%	Ideology, political conduct, partisan activity
Character	915	75.9%	Attitudes toward authority, family, society
Subversivity	404	33.5%	Subversive behavior, obedience, instigator, aggressiveness
Leadership	307	25.5%	Organizational skills, protest promoter, recognized influence, ability to mobilize others, good speaker
Education	157	13.0%	Intelligence level, cultural knowledge, education
Socioeconomic	99	8.2%	Background, upbringing, financial conditions, profession, roles held
Activism	13	1.1%	Level of involvement, activism
Demographic	19	1.6%	Age, physical appearance
Social	11	0.9%	Friendship and familial networks

(b) Surveilled by birth date



Notes: *(a)* Most common individual traits of the surveilled identified by unsupervised LLM in 1,254 police-narratives. *(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.

with Labini's descriptions to tag every recorded occupation. Second, we read a sample of 500 classified jobs, correct classes attributed incorrectly, and feed the corrected list back into the classifier. At the next iteration, we ask the LLM to produce a confidence score for each classified profession, and review cases with low scores. More details about this process are included in Section A, SI. Figures B.4 and B.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 B.5). About 3% of those surveilled were women. Their numbers grew over time at a faster rate than men's (Figure B.6), and most worked as housewives (28%) or tailors (5%).

Measures of Political Engagement: A secondary outcome of interest is the degree of political engagement in the population. To measure it, we compile five indicators of political

activity, ordered 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[3]. 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 code notable individuals who were involved in politics, ranging from members of parliament to activists. 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 capture the most costly form of political engagement: armed resistance against the state. First, we scrape a new database including 19,341 partisans born until 1895 from Central Institute for Archives (Icar). Partisans were anti-fascist resistance fighters against the Fascist regime. This dataset aggregates regional records assembled immediately after World War II to identify 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 the 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.[4] Finally, we obtained data on another costly form of political action: banditism. Bandits ("briganti") were individuals who lived outside of the law and engaged in criminal

[3]We were unable to obtain information of votes by party at the municipality level. See Section A for more details.

[4]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 B.3a.

acts such as raids, kidnappings, and thefts, targeting the wealthy and landowners. Their actions were 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. Many view 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.[5] 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 A, 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,[6] 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

[5]Municipalities in the regions of Friuli Venezia Giulia, Trentino Alto Adige, in the provinces of Belluno, Frosinone, Latina, Padova, Rome, Rovigo, Treviso, Trieste, Udine, Venezia, Viterbo, started being part of Italy only after the start of our treatment, and are therefore excluded from the main sample and included in a robustness.

[6]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).

extend compulsory schooling by an additional two years, covering ages eight to ten (n. 3725/1859, art. 319–321).

Around this time, other laws applied above the same population cutoff. In particular, Law n. 2248/1865, Art. 15, allowed municipalities above the threshold to create fractions of the municipality, and the Royal Decree n. 20/1896 applied different provisions for customs to municipalities above the threshold. To account for this, we leverage also the temporal component of the education reform: children who were seven or younger at the time of the law enactment (e.g., those born in 1854 or later) and living in larger municipalities experienced a 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. To avoid contamination from this reform, we restrict our analyses to cohorts born before 1896, who would have completed schooling before the new law took effect.

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 discontinuity at the 4,000-inhabitant cutoff, above which municipalities extended compulsory schooling by two years, and (ii) a temporal discontinuity between cohorts who were seven years old or younger in 1861, and therefore remained in school longer. Because treatment is assigned by birth year, treatment effects are 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), *Above*4000 is a dummy for municipalities above the threshold, and *Pop*1861 is the running variable. All regressions include municipality fixed effects, cohort fixed effects, and reign–cohort fixed effects. We use reign rather than region fixed effects because the sample begins before unification, but we present robustness using regions.

The coefficient of interest is β, capturing the average causal effect of the reform at the discontinuity. We control flexibly for population size on either side of the threshold (λ), and of the temporal discontinuity (η) to isolate the effect of the reform itself. 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 we difference this contrast against slightly older cohorts who were already too old to be affected by the reform. This design rules out confounders that vary only at the municipal or cohort threshold – for example, if municipalities above 4,000 had higher ex-ante capacity, or received more funds after the reform, this is not an issue for identification, which relies also on the difference across cohorts within the same city. While this design rules out several confounders, we also examine plausible alternative explanations in Section 8.

Identifying Assumptions: We provide several pieces of evidence to support the validity of this approach. First, we find no evidence of sorting: a manipulation test shows that the density of the running variable is constant across the cutoff (Figure C.1, p = 0.796). Second, we assess the credibility of the continuity-in-potential-outcomes assumption by showing that pre-treatment covariates are balanced across the discontinuity threshold (Table **??**). Third, there should be no interaction between treatment and the confounding policies (municipal fractions and customs policy, Assumption 3 in Grembi, Nannicini and Troiano (2016)). This assumption would be violated if, for example, cohorts just below and above the population

threshold reacted to the creation of municipal fractions or to the customs policy in different ways that impact surveillance. We show, however, that there are null effects of the 4,000 cutoff on surveillance for cohorts not affected by the education reform but affected by the creation of municipal fractions and the customs policy (Section 5, Placebos). Finally, as for the difference-in-differences design, there should be no pre-existing differences in surveillance between treatment and control municipalities before the onset of the education reform after controlling for fixed effects. We present evidence supporting the absence of pre-trends in the event-study analysis (Figure 3a).

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 often unavailable as we can rely only on municipal-level or municipal-year variation. When only municipality-level data are present, in a minority of cases, we use a simple regression discontinuity design (RDD) that exploits only the population threshold and controls for reign fixed effects:

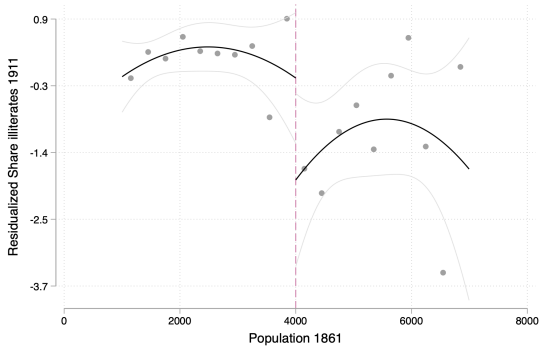
$$Y_{it} = \mu Above4000_i + \nu f(Pop1861)_i + \xi f(Pop1861) \times Above_i + \rho_r + \pi i \tag{2}$$

When year is present, as in most of the secondary analyses, we also include year fixed effects and show year-by-year RDD estimates. While we provide evidence in line with continuity of potential outcomes (Table **??**) and absence of sorting (Figure C.1), this specification is less precise than the DDRD, since other laws also introduced discontinuities at 4,000 inhabitants. 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 strategy 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

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.

education, by assessing its long-run impact on literacy. To do so, 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 between 1.6 and 1.8 percentage points lower illiteracy rates, corresponding to a 4.8–5% reduction from baseline levels (Table 2b). These coefficients are statistically significant only considering conventional (not robust) p-values.

While we consider the impact of the reform on an outcome systematically measured for all municipalities, like literacy, it is plausible that the reform had broader impacts on the education of pupils than teaching them how to read and write. The first two years of schooling focused primarily on literacy, mathematics, and religion, whereas the additional two years introduced new subjects – history, geography, physical and natural sciences, and literature. Thus, beyond reducing illiteracy, the reform likely broadened students' intellectual exposure, potentially inspiring them to read outside the prescribed curriculum or to continue their studies beyond the elementary 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 per cohort in municipalities just below the 4,000 cutoff was 5.5 per 100,000 inhabitants, meaning that the education reform increased surveillance by 64%. Assuming 50 cohorts were alive at the same time per municipality,[7] the number of surveilled in the average treated municipality passed from 27.5 to 47.5 in 10,000 inhabitants. 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 thirty-eight cohorts preceding the first treated cohort, 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 in this setting provides stronger support for the parallel trends assumption than is typically possible in difference-in-differences designs. The pre-trend is slightly negative, driven by a modest positive effect in the four years preceding treatment among municipalities above 4,000. This pattern is consistent with some individuals becoming treated early as a result of grade repetition.[8]

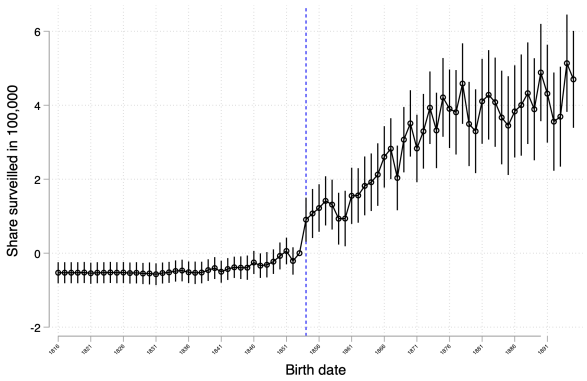
Beginning with the 1854 cohort, we observe a stark increase in surveillance, corresponding to an additional 0.4 individuals surveilled per 100,000 in the first treated cohort. This effect grows over time, reaching an additional 4 individuals surveilled per 100,000 by 1890. The increasing impact of the reform over time is consistent with the state's gradual expansion of its capacity to enforce mandatory education, and the broader uptake of educational

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

[8]For instance, a child born in 1853 who repeats second grade would re-take it once the reform is in place, thereby continuing their education beyond grade two and qualifying as treated.

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.

resources, as schooling became less of an elite privilege. While detailed time-varying data on the expansion of students' enrollment in schools in this period is absent, preventing us to provide direct evidence on when and where education became more inclusive, we include 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: In 1904 the population discontinuity ended: compulsory schooling was extended to all municipalities, eliminating differences between treatment and control areas. If the increase in surveillance was produced by the education reform, we should see it disappearing together with the differences between cities. The extended event study in Figure D.1 shows that the difference in surveillance for municipalities above 4,000 decreases precisely starting from 1896 – the birth-date of individuals who turned 8 years old when the new reform was enforced, now exposed to the same education requirements as

children in control municipalities.[9] 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 D.1. First, we control 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 when including calendar-year controls (column 1). Second, we include areas of Italy that were annexed to Italy after 1861, where the education reform was implemented later, and are thus excluded from the main analysis. Results are robust to estimating a staggered DDRD which accounts for the effective year of the reforms' implementation in later-annexed areas (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 D.2 tests robustness to alternative transformations of the dependent variable. Using a dummy equal to one if any individual was surveilled in a municipality-cohort, we find a 28% higher likelihood of surveillance following the education reform (column 1). By contrast, considering the absolute count yields a null effect (column 2), which is consistent

[9]The 1904 reform affected only children aged eight or younger (those born in 1896 or later, who would have been completing third grade) because an earlier reform, introduced prior to 1904, had already raised the requirement to third grade, thereby reducing the discontinuity for municipalities above 4,000 inhabitants from two additional years of schooling to just one.

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 observe again positive and significant effects.

In Table D.3, we replicate our results using different designs. We start from a less restrictive difference-in-differences (DID) which does not flexibly control for proximity to the population cutoff and leverages the entire distribution (columns 1-3). We also use a more restrictive model flexibly controlling for proximity to both the population and the year of birth discontinuities, such that we focus on observations across both the geographic and the temporal discontinuity, in the spirit of a "double" difference-in-discontinuity design (columns 4-6). Finally, we remove the interaction with Post from the flexible controls to estimate a regression discontinuity version of Equation 1 in columns 7-8. 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 below the 4,000 discontinuity. We find null effects for all the round placebo thresholds below the right cutoff in Figure D.2a.

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 Figure D.2b 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 driven by state concerns over the education of subaltern groups, we classify the professions of surveilled individuals into distinct social classes, as detailed in the Data section and illustrated in Figure B.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 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 (β=0.57, column 2) and upper classes (β=0.07, column 3). The last row reports the p-value from a t-test comparing working class to middle and upper class coefficients. Both p-values indicate that effects for the working class are significantly larger than for other classes. 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 the working class: we observe a 0.24 percentage points increase for working classes, as opposed to 0.04 and 0.004 for the middle and upper class respectively (Table E.1).

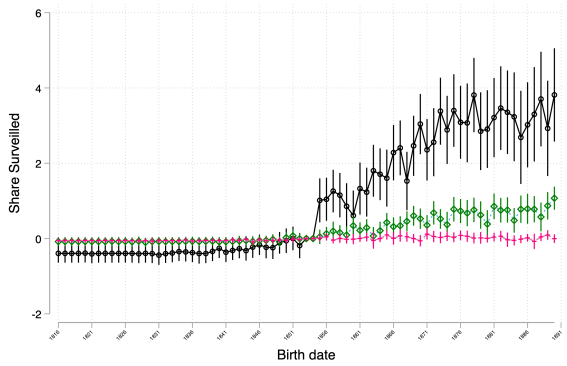
A potential concern with interpreting this analysis is that the number of individuals in the working class could itself be influenced by the education reform. However, given that the reform expanded access to literacy and likely improved the labor prospects of children born in low income families, it most likely reduced the overall share of working class individuals in the post-reform period. This would imply that our findings represent a lower bound of the 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,

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

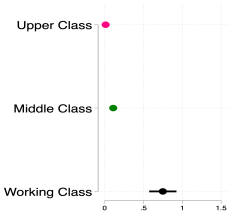
(a) Event Study



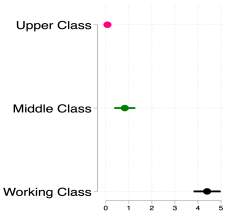
(b) Difference-in-discontinuity

	(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
Birth FE	Y	Y	Y
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

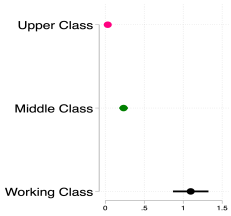
(c) Severity



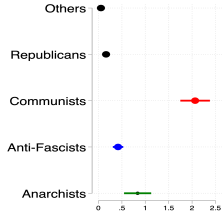
(d) Duration



(e) Information



(f) Ideology



Notes: (a): DDRD eventstudy using as dependent variables the population share of 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 bottom 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 population share of individuals under severe forms of surveillance, the average number of years a municipality-cohort spends under surveillance, and the population share of individuals with a high volume of information collected on them in the working (black), middle (green) and upper (pink) class. *(f)*: DDRD coefficients from separate regressions using as dependent variable the population share of surveilled from each ideology. All regressions include municipality and reign times birth-year fixed effects. Standard errors are clustered at the municipal level in all regressions.

and the volume of information collected – and by examining whether surveillance disproportionately targeted certain ideological orientations.

Severity: Our first measure captures whether treated cohorts were subjected to the more intensive forms of surveillance described in the Data section (monitoring movements across borders, notice of holding a good conduct, and political confinement), a set of tool used to restrict dissidents' movements and in some cases forcibly remove them from their communities. Table E.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 (Figure 4c and Table E.2, β=0.77, column 2) and less pronounced for other groups (β= 0.11 and 0.01, columns 3-4).

One could wonder if the increase in surveillance for educated working classes is entirely the result of the state switching to overt forms of control as a strategy to advertise state monitoring among these groups. However, the increase in surveillance is also present when we consider surveillance that does not use intensive methods of control, indicating an overall uptick in control of subaltern groups by the state (Table E.3).

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, consistent with 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 for 2.1 years. The reform significantly extends surveillance duration: treated cohorts are surveilled 5.2 years longer, a 2.5-fold increase (Table E.4, column 1). As with the severity of surveillance, this effect is most pronounced among the working class, where surveillance duration increases by 4.4 years (Figure 4d and Table E.4, column 2), exceeding the increase observed for middle (β=0.82, column 3) and

upper-class individuals (β= 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 the middle (15%) and upper-classes (2.81%). Was this pattern further enhanced by quasi-exogenous exposure to education? Table E.5, column 1, shows that treated cohorts were twice as likely to have a biographical file, with the share of surveilled individuals with biographical documentation increasing by 1.36 percentage points from a mean of 1.17. This increase is driven by the working class (Figure 4e and Table E.5, β= 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 reflected state concern for educated working classes, we should observe disproportionate monitoring of ideologies advocating on their behalf. Table E.6 and Figure 4f present the effect of the education reform on surveillance 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 E.7), underscoring the exclusion of the working class from meaningful political representation.

6.2 Additional Heterogeneities

Democracy and Dictatorship: A substantively important question is whether the strategic use of state surveillance we document in these analyses varies across regime types. To answer, we leverage our extended time period which spans both democratic governments (1869–1921) and the fascist regime (1922–1947). Table E.8 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 subaltern individuals remain a primary focus of state surveillance, regardless of the governing ideology.

State Capacity: State capacity is another potential mediator of surveillance whose impact is ambiguous. If state resources are extremely large, targeting might become unnecessary. If too limited, the state might not be able to conduct preventive surveillance, being forced to only monitor those that represent an obvious threat. To assess if state capacity is a mediator of our findings, we digitize new data on postal offices in the Kingdom of Italy, sourced from an 1861 official postal bulletin – a proxy for state presence. Columns (1) and (2) in Table E.9 show that the targeting of educated groups 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 the 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 targeting individuals for surveillance, it is not a prerequisite for the patterns documented – at least at the levels of capacity present in Italy at this time.

Technology: The technology employed for surveillance could also influence states' strategies. We evaluate this possibility by leveraging the fact that in 1916 Italian military units began utilizing telephone wiretapping as a tool for intelligence gathering. Column (6) of Table E.9 shows no statistically significant difference in the rate of surveillance in response to the education reform after the introduction of wiretapping. An innovation enhancing state capacity to collect intelligence did not change the strategic choice of the state to target educated subjects.

Pre-Reform Surveillance Levels: We consider whether the increase in surveillance is driven by newly monitored areas or by heightened scrutiny in already highly surveilled regions. Table E.10 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.

7 Education and Political Engagement

Did the increase in surveillance following an expansion in 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 extensively, 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. Since each data source varies at a different level of aggregation, we adopt distinct empirical strategies tailored to each measure. In every case, our preferred specification is the most restrictive one available, but we also report all results using the minimal common denominator – a cross-sectional RDD.

Voter mobilization: We begin by examining the impact of the education reform on voting in national elections, starting with the first election in unified Italy in 1861. The first treated cohorts became eligible to vote in 1880, when individuals born in 1854, the first affected by the reform, reached the voting age of 25. In this initial treatment year, only a small share of the electorate had received the additional schooling. Because election data is at the municipality-election level rather than at the cohort level, and because treatment intensity increases gradually as more cohorts entered the electorate, the time discontinuity is less informative in this context. We therefore estimate yearly RD coefficients for the effect of the 4,000-inhabitant cutoff (Equation 2) in each election year, which allows us to trace the evolution of the reform's impact over time independently of pre-1880 differences. For completeness, we also report DDRD estimates in the appendix, which yield a consistent picture. Our analysis concludes in 1913, the last election year before later reforms equalized educational exposure across the population threshold.

We start by showing that, before the first treated cohort started voting, municipalities above the 4,000-population threshold voted at about the same rate as those below (Figure F.3), in line with the parallel trends assumption. We then consider the impact of the education reform after the first cohort gained voting rights. We still observe no differences in voting 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 effect of the population discontinuity in the post-period (Figure F.1 and Table F.1, columns 1-2), as well as estimating the DDRD using different transformations of the dependent variable (Table F.1, columns 3-4). The education reform had a robust null effect on voting in national elections.

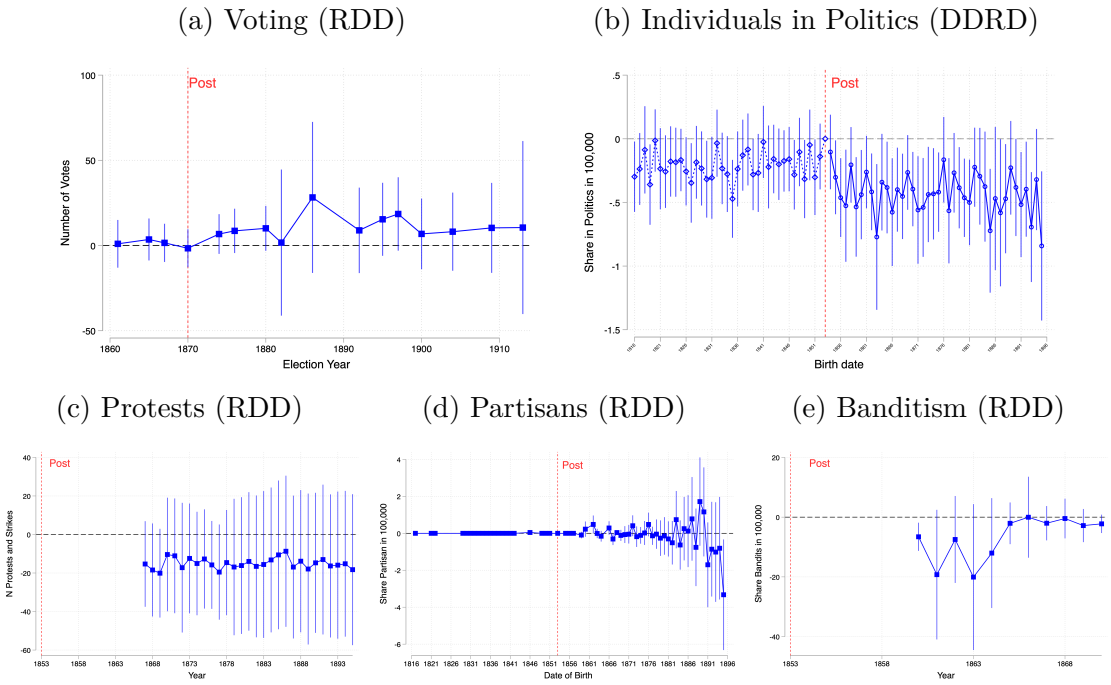
Notable people involved in politics: Our second measure of political engagement is the number of individuals who held political roles, which we scrape from Wikidata. Given that in this case we observe individuals' date of birth and we have sufficient observations both before and after the education reform, we can replicate the same municipality–cohort specification as in our main surveillance analysis. The DDRD event study in Figure 5, panel (b), shows that, before the reform, individuals in municipalities above and below the 4,000-inhabitant threshold held political positions at similar rates. After the reform, this pattern remains unchanged – if anything, the estimates suggest a slight negative effect on political engagement. Table F.2 confirms the finding both for individuals in politics and for other notable figures using the same specification as our main analysis.

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 and we do not have a pre-period before the reform, as data is only available from 1867. Because we cannot exploit the temporal component, we present regression discontinuity coefficients for the impact of the 4,000 inhabitants discontinuity in each year. Consistently with the previous results, our findings indicate no significant increase in protests and strikes in municipalities above the 4,000 discontinuity in each year (Figure 5, panel (d)), and overall (Figure F.1), independently on transformations of the dependent variable (Table F.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 groups whose political activity involved a high risk of death.

The data on partisans includes their date and location of birth, but too few of them

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.

were born pre-education reform to meaningfully exploit the time variation.[10] Therefore, also in this case, we present RDD coefficients in each year. We find a null effect of the education reform on the population share of members of the resistance movement (Figure 5, panel (d), and Table F.4, column). Results are similar in a DDRD design (columns 3-4) and considering a RDD with year fixed effects (Figure F.1).

We consider bandits in Figure 5, panel (e): also in this case, RDD coefficients plotted in each available calendar year show a null effect of the education reform on the number of bandits, results confirmed in Figure F.1e, with year fixed effects.

[10]While the number of observations in the pre-period is not zero, partisans started operating in 1943, so those born pre-reform were 90 years old when the resistance started.

Interpreting the absence of effects on political engagement We have examined a wide range of political engagement outcomes, and the evidence consistently shows that exposure to additional primary education did not translate into higher levels of political participation. This finding may appear to stand in tension with the increase in surveillance we document. If education did not foster greater political activism, why would the state invest additional resources in monitoring these individuals?

We propose two interpretations. The first is that political participation did not increase precisely because the state's preventive surveillance strategy succeeded in deterring it. Under this account, increased surveillance produced a chilling effect on political mobilization. The second possibility is that participation did not increase in the first place, independently of surveillance, meaning that state surveillance was not a response to an actual increase in activism. This would be consistent with recent studies showing that the content of education matters to determine whether this leads to more activism or more obedience with the state.

Our evidence cannot adjudicate decisively between these two interpretations. Descriptive exercises, however, provide little support for the chilling-effect interpretation: we find no differences in political engagement between cohorts that were and were not subject to surveillance pre-reform (Table F.6) and that were subject to high or low levels of surveillance (Table F.5). While these analyses suggest that surveillance does not appear to explain the absence of greater political engagement, they are endogenous and partial.

If, instead, education itself did not trigger higher participation, does this imply that the state's heightened surveillance was irrational? Identifying future political leaders ex ante is notoriously difficult, and the potential payoffs to early detection are extremely high. From the state's perspective, even a small number of empowered members of subaltern groups could pose enough risk to justify increased surveillance of many of 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.

8 Alternative Explanations

Other municipal-level changes induced by the education reform: The education reform may have influenced other aspects of treated municipalities: they might receive more funds, or gain better capacity. For these changes to represent alternative explanations for our findings, they should vary discontinuously not only by municipality, but also by cohort. For example, the reform could have increased education levels among local institutions, such as the police, or in the general population. A better-educated police force might be more effective at monitoring individuals, increasing surveillance. For such a mechanism to account for the jump in surveillance we observe, (i) the increase in surveillance would need to be driven specifically by the newly educated cohort of policemen or citizens; and (ii) these actors would need to selectively target individuals from their own age cohort, since our results are tied to the age of those surveilled rather than the timing of surveillance.[11]

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 or compositional shifts – such as changes in the education level of local institutions or residents– should not affect their likelihood of being surveilled. As shown in columns (4)–(6) of Table G.1, the results for this subsample closely mirror those reported in Table 3b, suggesting that migration does not attenuate the treatment effect. The finding also replicates if we consider individuals moving exclusively to control municipalities, where any local changes produced by the education reform would be absent (columns (7)-(9)).

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

[11]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.

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 G.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 reporting on their political activity. While this possibility is unlikely already in light of the first test on surveilled who migrated away from their municipality, we investigate it 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. We consider difference-in-discontinuity results at the municipality-year of surveillance level in Table G.2. The analysis shows that this expansion in state agents 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.[12]

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 below and above the threshold, it merely lengthened primary schooling by two years, such that record keeping on pupils did not jump at the reform cutoff. Second, our outcome measures surveillance at any age, often decades after pupils left elementary school. For this force to determine our findings, what a child did in third rather than second grade would need to better inform political policing when the same person is 40, and even if this person migrated away from the hometown where this initial record was kept.

Teachers as instigators: An opposite possibility is that the state perceived 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 were surveilled did not increase (Table G.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 3–5, Table G.3), despite the post-reform growth in the share of teachers in the population.

[12]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.

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 have reduced individuals' job prospects, thereby pushing them to select into working class occupations. Table G.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 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 unlikely 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

This study focuses on the strategic considerations states make when deciding whom to subject to political surveillance. Our findings reveal that states are more likely to target individuals who display potential markers of political activism – educated individuals –, and particularly so when they belonged to a subaltern category deemed potentially dangerous – in this setting, the working class. This logic is observable 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 Additional Details on Data

Social Class Classification: Each record of the surveilled individuals reports their profession. To categorize profession into 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. The author divides society into 4 large groups as described in Table A.1.

Table A.1: Professions and Social Class correspondence as in Labini, 1972, p.105

Class	**Description**	**Professions**
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, 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 employ a large-language-model-based classifier (OpenAI GPT-4o, temperature 0.3) to classify each recorded occupation. We use temperature 0.3 to allow the model limited flexibility in extrapolating beyond exact job titles, since our categorization scheme offers broad occupations rather than a one-to-one full mapping of profession to class. This low but nonzero setting helps the model generalize appropriately while maintaining consistency and reproducibility in the classifications.

First, we feed a sample of 500 jobs and asked the model to map professions into classes based on the framework in Labini (1972) on a sample of 500 jobs (out of 14,780) that we

had previously coded manually. After reviewing the output of this first classification, we provided the corrected sample back to the model along with a prompt for coding the entire sample of professions. Additionally, to improve reliability, we asked the model to assign a confidence score (0–100) to each classification. After this second round of classification, we manually reviewed all entries with confidence score below 10, and correct them if needed. Finally, we define *Upper* as the top class in Labini, *Middle* as including urban middle and special categories, and rural middle and working as $Working$ class.[13]

Voting behavior: Data on the voting behavior of the Italian population was gathered from the *Electoral Historical Atlas* archive, which includes municipal-level results for all national elections held in Italy from 1861 onward. Specifically, the number of votes by each candidate at the first round and at the run-off. We focus on elections up to 1913, after which the electorate included cohorts no longer exposed to discontinuous variation in education (those born after 1896, for whom the 4,000 cutoff did not apply anymore). We build a municipality–election year panel and merge it with 1861 population data, which we use to define treatment status. Because the voter data report the number of votes but not the number of eligible voters, we normalize votes by the 1861 population, the last population Census before elections began.

The archive contains information for the party only of the winning candidate in each constituency. Thus, the information at our disposal allows us only to find the number of votes for the winning party in the constituency rather than the number of votes by party per municipality. As the winning party is determined by the number of votes at the constituency level and not at the municipal level, we are unable to use this data to study the effect of the education policy on ideology within municipality.

[13]Moving the rural middle class to the middle class returns very similar findings, available upon request.

Wikidata: We gathered information on notable individuals known for their roles in politics from Wikidata. We scraped records for all individuals born in Italy between 1816 and 1930 that have a Wikipedia profile for any reason. For each individual, we collected date and municipality of birth, occupation, and other significant roles held. 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, members of parliament and other institutions, titled nobility.

Protests: Information on location and timing of protests was sourced from articles published by the Italian newspaper *La Stampa*, the only news archive 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 event to the year of the article's publication and to the municipalities mentioned. To ensure accuracy, we excluded municipalities whose names are also common nouns, such as Porte ("doors") or Ora ("hour"). For a small subset of such cases, we manually reviewed the articles mentioning them and confirmed that the mentions referred to the common noun rather than the municipality. Using this information, we constructed a municipality-year panel dataset and merged it with 1861 population data. Of all the municipalities in our sample, only 0.5% had names that are common nouns, and of these, only 10 had populations greater than 4,000. These municipalities were excluded from the protests analysis to avoid misclassification.

Postal Offices: We digitized data on the presence of postal offices per municipality in Italy from the 1861 official postal bulletin published by Institute of postal studies "Aldo Cecchi". The bulletin reports the type and location of each postal office in that year. We merged this information with our main panel dataset based on municipality information.

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

A.1 Education Reforms

Our main analysis considers the first education reform of Unified Italy, the Casati Law enacted in 1861, which made the first two years of elementary schooling compulsory. Town with more than 4,000 inhabitants had to increase compulsory schooling by an additional two years. This discontinuity ended in 1904, when the Orlando Law extended compulsory schooling to age 12 in all municipalities, regardless of population level. To avoid contamination from this reform, we restrict our analyses to cohorts born before 1896, who would have completed compulsory schooling before the new law took effect. An earlier reform, introduced prior to 1904, had already raised the requirement to three years, thereby reducing the discontinuity for municipalities above 4,000 inhabitants from two additional years of schooling to just one. Consequently, the 1904 reform affected only children aged 8 or younger (i.e. those born in 1896 or later, who would have been completing third grade).

A.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 more than 5,000, the boards were required to have eight members, while smaller municipalities were only required to have three.

B Descriptives

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

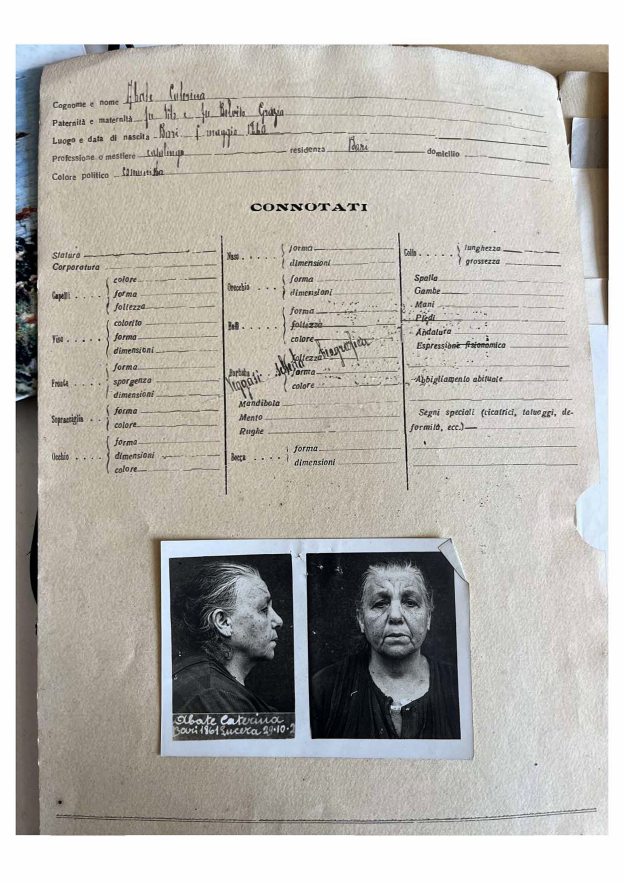


Figure B.2: Number of surveilled by municipality of birth

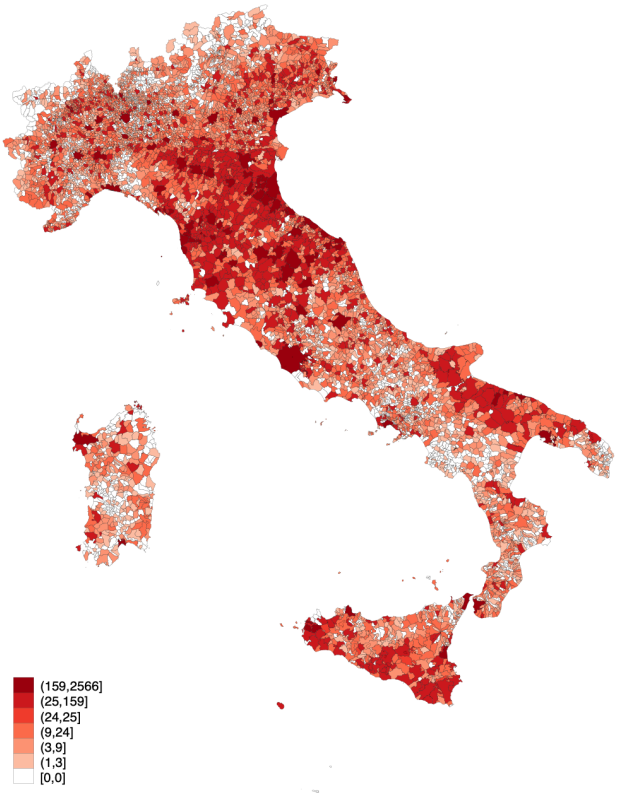
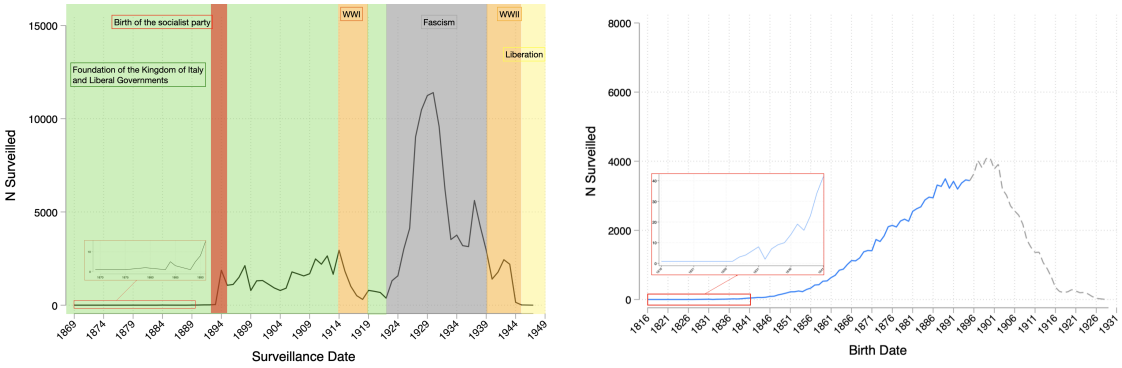


Figure B.3: Number of surveilled

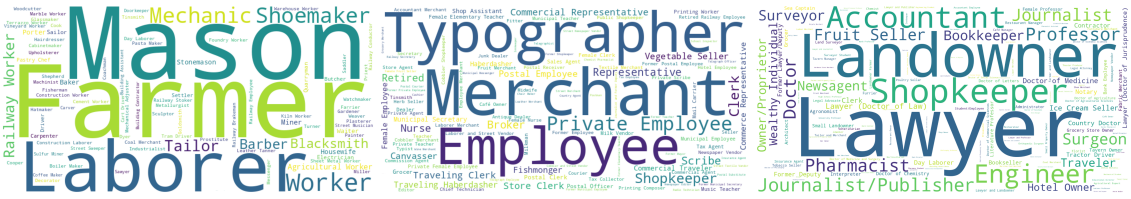


(a) by date of surveillance (b) by date of birth

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 A.1, 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 B.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.

B.1 LLM Classification

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

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

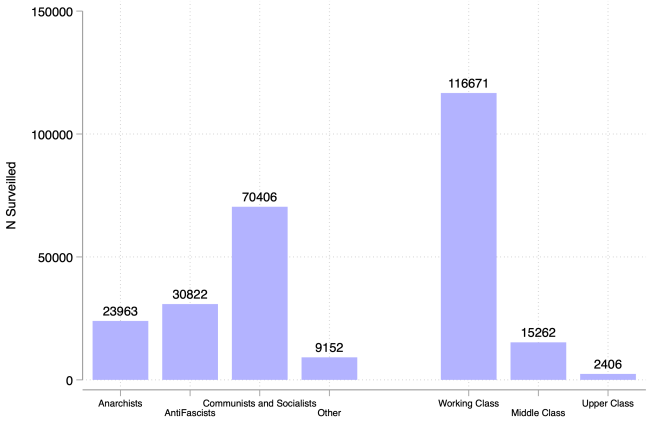
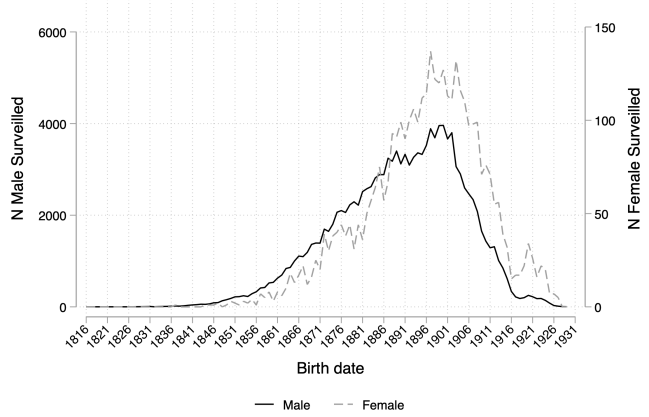


Figure B.6: Number of surveilled by gender



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).

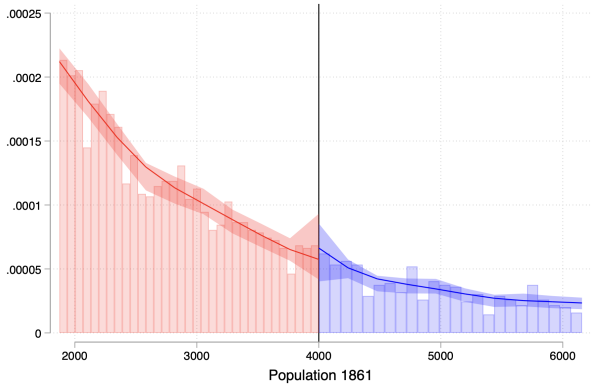
Latent Dirichlet Allocation (LDA) models, is useful for uncovering recurrent themes in text files, it is not well suited to our specific case. Topic models rely on patterns of frequent co-occurrence. In the biographies we have, the co-occurrence are dominated by the bureaucratic and political vocabulary of surveillance (e.g., judicial and legal process). By contrast, descriptions of individual personality traits tend to be highly heterogeneous in wording, 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. Finally, a second large language model (OpenAI GPT-4o, temperature 0) was used to assign human-readable labels to each cluster. We then refined these results through a manual consolidation process. We exclude categories referring to a single record, and merge thematically overlapping categories that captured similar concepts, obtaining 12 categories (Table 1a). The merging process was based on conceptual similarity among underlying traits. In particular, we combine work ethic and vocational descriptors into "Professional"; physical and demographic characteristics into "Demographic"; decision-making and communication abilities into "Leadership and Communication"; intellectual and cultural traits into "Education"; and contextual descriptors were grouped as "Socioeconomic". This two-step approach makes it possible to capture how police institutions framed individual traits systematically.

C Identifying Assumptions

Figure C.1: Manipulation Testing Plot

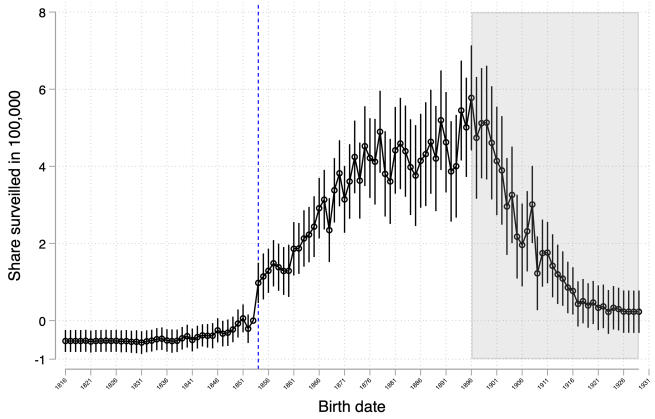


Notes: The figure plots the manipulation test at the 4,000 inhabitants cutoff (p-value: 0.796).

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D Robustness to the Main Result

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

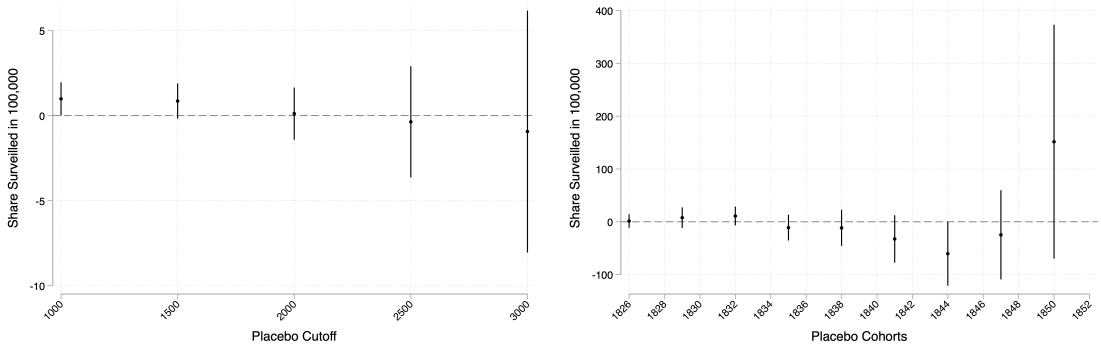


Notes: Event study estimates from Equation 1 as in Figure 3a for each year of birth from 1816 to 1929 (rather than stopping at cohort 1896). From cohort 1896, the education reform applies to all municipalities and the discontinuity stops applying. The reference cohort is 1853 (one year before treatment).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

Figure D.2: Placebo

(a) Population Cutoffs

(b) Cohorts Cutoffs



Notes:*(a)* DDRD results as in Equation 1 in municipalities at placebo population cutoffs below 4,000. We restrict the sample to municipalities with less than 4,000 inhabitants to avoid overlap with the actual treatment. Standard errors are clustered at the municipal level. *(b)* DDRD results as in Equation 1 but for alternative (placebo) cohorts born before the first treated cohort (1854). We consider treated cohorts between a certain placebo threshold and that threshold plus 10. Standard errors are clustered at the municipal level.

Table D.1: Robustness: functional forms

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Surveilled in 100,000					
	Calendar-year	Annexed later	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	-	-
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) includes areas that were annexed to the Kingdom of Italy after 1861 and that implemented the Casati Law following their annexation, using a staggered version of treatment (Friuli Venezia Giulia, Veneto, and the province of Mantova enacted the law in 1866; Lazio in 1870. 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.

Table D.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 in the top and bottom percentile; 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 D.3: Effect of the education reform, DiD, Double DDRD and RD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share Surveilled in 100,000							
		DiD			Double DDRD			RD
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)		
Above4000							3.729***	3.831***
							(0.359)	(0.365)
Observations	617,040	617,040	617,040	617,040	617,040	617,040	323,946	323,946
Municipality FE	N	Y	Y	N	Y	Y	-	-
Birth FE	Y	Y	-	Y	Y	-	Y	-
Reign x Birth FE	N	N	Y	N	N	Y	N	Y
Mean DV	5.499	5.499	5.499	5.499	5.499	5.499	5.499	5.499

Notes: Columns (1)-(3) report 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 (4)-(6) show a "double" DDRD results: in addition to the controls in Equation 1, we flexibly control for the impact of treatment across cohorts just-exposed and just-unexposed to the reform. Columns (7)-(8) report RD results showing the effect of being above the 4,000 population discontinuity on the number of individuals surveilled in the post-period only, flexibly controlling for the impact of population around the discontinuity. 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.

E Results on Working Classes

Table E.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 E.2: Effect of the Education Reform on Intensive Surveillance, by class

	(1)	(2)	(3)	(4)
	Share under Intensive Surveillance 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
Birth FE	Y	Y	Y	Y
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 under intensive forms of surveillance (*monitoring movements across borders, notice of good conduct, forced relocation*) 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 E.3: Effect of the Education Reform on Non Intensive Surveillance, by class

	(1)	(2)	(3)	(4)
	Share under Intensive Surveillance in 100,000			
	Overall	Working Class	Middle Class	Upper Class
Post × Above4000	2.711***	2.172***	0.482***	0.0577***
	(0.261)	(0.230)	(0.0472)	(0.0181)
Observations	617,040	617,040	617,040	617,040
Birth FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Reign x Birth FE	Y	Y	Y	Y
Mean DV	3.948	3.374	0.431	0.0946
H_0: Working Class <=			0	0

Notes: DDRD results as in Equation 1 where the dependent variable is the population share of individuals surveilled but not under intensive forms of surveillance. 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 E.4: Effect of the Education Reform on Duration of Surveillance, by class

	(1)	(2)	(3)	(4)
	Number of years 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 (RDD)	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 E.5: 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
Birth 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

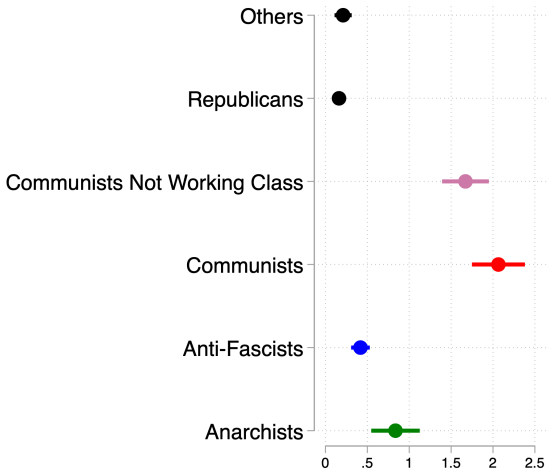
Notes: DDRD results as in Equation 1 where the dependent variable is the number of individuals surveilled with recorded biographical information in 100,000 inhabitants. Column (1) shows the results for the whole sample; columns (2)-(4) by class. We report the mean of the dependent variable for municipalities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

Table E.6: 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.

Figure E.1: Robustness: by ideology



Notes: DDRD results as in Equation 1 subsetting by ideology of the surveilled. The category *Communists* includes both Communists and Socialists. The category *Communists (Not Working Class)* includes both Communists and Socialists that were not part of the working class. The category *Others* is a residual group that includes surveilled individuals whose ideology is recorded as anti-nationalists, syndicalists, masons, and other similar affiliations.Standard errors are clustered at the municipality level.

Table E.7: 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
Birth FE			Y	Y			Y	Y
H_0: Working Class >			0.999	0.000135			0.00144	0.000135

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 E.8.

E.1 Additional Heterogeneities

Table E.8: 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 E.7 reports the results by governing party before 1922.

Table E.9: Heterogeneity by state capacity and technology

	(1)	(2)	(3)	(4)	(5)	(6)
	Share Surveilled in 100,000					Surveilled Post Wiretapping -
	Post Offices		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

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 E.10: 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.

F Results on Political Engagement

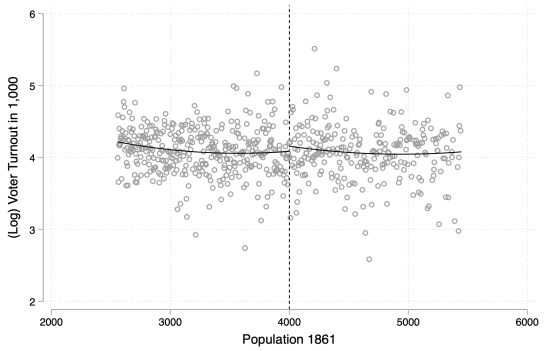
Table F.1: Effect of the education reform on votes in national elections (RDD and DDRD)

	(1)	(2)	(3)	(4)
	RDD		DDRD	
	Total	Share	Total	Share
Above4000	11.997	3.363		
	(11.094)	(4.729)		
Post × Above4000			-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	-	-
Reign FE	Y	Y	-	-
Municipality FE	-	-	Y	Y
Reign x Election Year FE	-	-	Y	Y
Mean DV	157.7	106.2	157.7	106.2

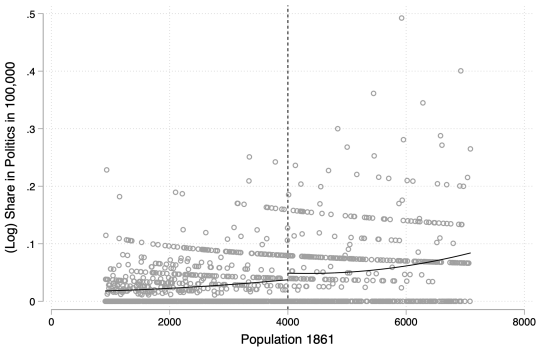
Notes: RD (columns 1-2) and DDRD (columns 3-4) results. Columns (1)-(3) show the effect for the absolute number of votes; columns (2)-(4) for the share of votes per 1,000 inhabitants. In columns (1)-(2), we subset the analyses to election years from 1880 onward, when the first treated cohort gained voting rights (Figure F.3 shows there is no effect in the pre-period). The regressions include year and region fixed effects. In columns (3)-(4), we use the same DDRD specification as in 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 after the reform. Standard errors are clustered at the municipal level.

Figure F.1: Effect of the education reform on political engagement (RD)

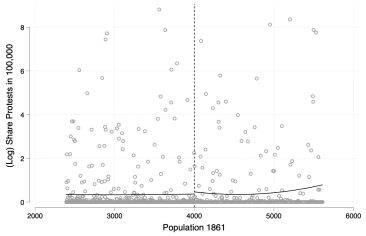
(a) Voting



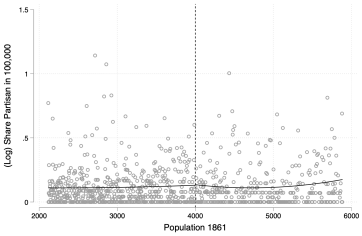
(b) Individuals in Politics



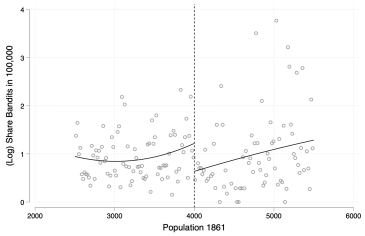
(c) Protests



(d) Partisans

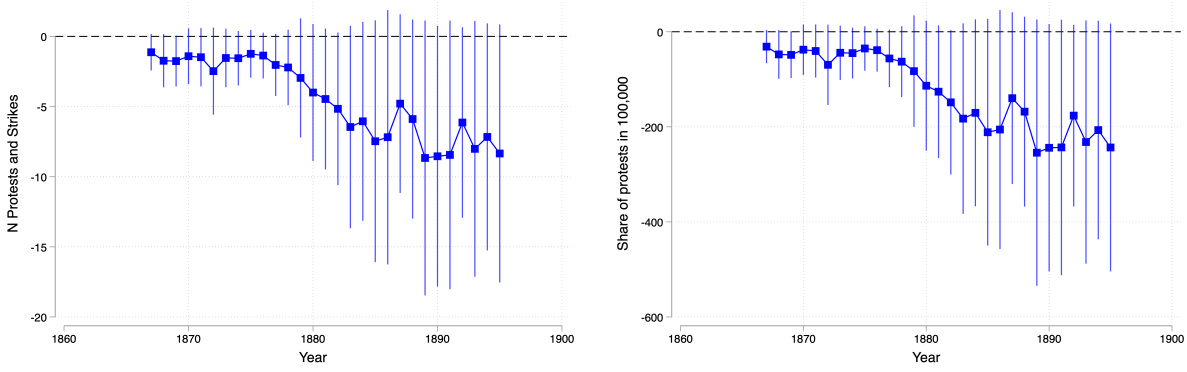


(e) Banditism



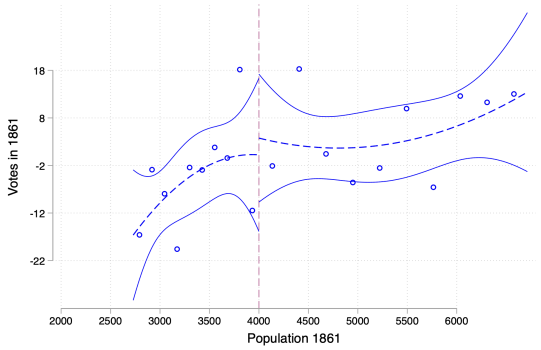
Notes: RD (Calonico, Cattaneo and Titiunik, 2015) results showing the effect of being above the 4,000 cutoff for the cohorts affected by the reform (i.e. born after 1854) on: (a) the (log) share of votes in 1,000; (b) the (log) share of notable individuals in politics; (c) the (log) share of protests in 100,000; (d) the (log) share of partisans in 100,000 inhabitants; (3) the (log) share of bandits in 100,000 inhabitants. Standard errors are clustered at the municipal level.

Figure F.2: Effect of the 4,000 cutoff on Protests (RDD), 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 F.3: 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 the 1861 national election. 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 F.2: Effect of the Education Reform on notable individuals from Wikidata

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		Non Politics		Politics	
	Share	Total	Share	Total	Share	Total
Post × Above4000	-0.294*	-0.0869**	-0.110	-0.0866**	-0.184**	-0.000292
	(0.159)	(0.0361)	(0.131)	(0.0341)	(0.0805)	(0.00310)
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	0.800	0.0192	0.517	0.0122	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 (see Section A 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 F.3: Effect of the 4,000 cutoff on Protests (RDD)

	(1)	(2)	(3)	(4)	(5)	(6)
	Protests					
	Share		Winsorized		Ln	
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 F.4: Effect of the Education Reform on partisan 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 F.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**	-0.112	-0.0904**	-0.189**	-0.000252
	(0.160)	(0.0386)	(0.132)	(0.0365)	(0.0807)	(0.00326)
Post × Above4000 × HighSurveillance	0.0470	0.0254	0.0169	0.0257	0.0300	-0.000271
	(0.0917)	(0.0173)	(0.0658)	(0.0161)	(0.0512)	(0.00333)
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	0.486	0.0117	0.253	0.00590

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 A 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 F.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*	-0.0171	-0.103*	-0.127	0.00763
	(0.172)	(0.0558)	(0.142)	(0.0530)	(0.0853)	(0.00500)
Post × Above4000 × NoSurveillance	-0.202***	0.0108	-0.125**	0.0215	-0.0771**	-0.0106***
	(0.0722)	(0.0274)	(0.0538)	(0.0258)	(0.0359)	(0.00389)
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	0.710	0.0190	0.394	0.00960

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 A 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.

G Alternative Interpretations

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Share Surveilled in 100,000								
	Stayers			Movers			Movers To Control Cities		
Post × Above4000	2.100***	2.036***	2.075***	1.479***	1.429***	1.433***	1.550***	1.488***	1.416***
	(0.218)	(0.212)	(0.194)	(0.134)	(0.131)	(0.120)	(0.121)	(0.117)	(0.108)
Observations	617,040	617,040	617,040	617,040	617,040	617,040	617,040	617,040	617,040
Municipality FE	N	Y	Y	N	Y	Y	N	Y	Y
Reign x Birth FE	N	N	Y	N	N	Y	N	N	Y
Mean DV	2.694	2.694	2.694	1.980	1.980	1.980	1.764	1.764	1.764

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 (columns 4-9), for which the registered birth and residence locations differ. Columns 4-6 present the effect for migrants to any city and columns 7-9 to cities with population below 4,000 in 1861. 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 G.2: Placebo Reform - More Bureaucrats but Not More Schools

	(1)	(2)	(3)
	Share Surveilled in 100,000		
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 G.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 G.4: Effect of the Education Reform, by age

	(1)	(2)	(3)	(4)	(5)
	Share Surveilled in 100,000				
	0-17	18-25	26-35	36-45	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.