Feather-handed Fascists:
Surveillance as a Signal of Bureaucratic Alignment*

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Abstract

How do bureaucratic incentives shape policy implementation in autocratic regimes? Most explanations relate higher bureaucratic output to the preferences of partisans or to the competence of non-partisan agents. This paper argues that forceful implementation of sensitive policies can be driven by non-partisan bureaucrats who need to signal their alignment with the regime. We construct a formal signalling model to establish that, if revealing alignment through effort is sufficiently rewarded, non-partisans set higher effort than partisans due to their lack of credentials. We test this on a province-year dataset of all 415 provincial prefects in Fascist Italy (1922-40) by originally digitising their biographies and appointments, and linking them to the universe of about 100,000 political surveillance dossiers. We estimate a staggered Difference-in-Differences design, with prefects that voluntarily joined the Fascist Party, particularly before it seized power, as treatment: these partisans opened about 20 per cent fewer dossiers than non-partisan colleagues. We empirically probe several competing explanations, and we corroborate a signalling mechanism with further evidence: non-partisans disproportionately focus on the usual suspects to boost their reputation, and partisans obtain more job security per surveillance record, which our model shows is consistent with revealed alignment being rewarded. These findings provide rare systematic evidence on the functioning of authoritarian bureaucracies and show how signalling alignment can be a banal yet powerful driver of coercive behaviour.

Keywords: Autocracy, bureaucracy, surveillance, consolidation, signalling, loyalty, alignment, Fascist Italy.

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All these motives, individually or combined among themselves, were operative in giving rise to this grey zone, whose components [...] were united by the will to preserve and consolidate their privilege.

— Primo Levi, *The Drowned and the Saved* (1986, pg. 27)

1 Introduction

In a pivotal scene of *Rome, Open City* by film director Roberto Rossellini, a smiling, unnamed police officer pulls out of his coat a surveillance file. It identifies the protagonist, Giorgio Manfredi, as a longstanding communist and leader of the anti-fascist resistance, prefiguring his death by torture. Who decided, years before, to surveil Mr. Manfredi, and why? This study sheds light on the inner workings of autocratic bureaucracies by focusing on leading security bureaucrats.

All modern states exercise surveillance (Foucault, 1977). Notably, autocracies use it to provide the intelligence that underwrites their repressive functions - identifying suspects to fine-tune coercion and minimise unintended spillovers (Xu, 2021; Hager and Krakowski, 2022). Due to the covert and strategic nature of the task, existing studies on the matter are primarily theoretical (e.g. Egorov and Sonin, 2011; Zakharov, 2016; Montagnes and Wolton, 2019; Tyson, 2018) and mostly frame the issue as the problem of selecting or retaining agents that are both sufficiently aligned and sufficiently competent, trading off the two when necessary.

However, most theories do not explicitly consider the political background of these agents, despite it being likely to influence the trust the leadership places in them. Political credentials should thus be expected to be major determinants of agents' behaviour. Moreover, empirical evidence on administrative personnel in autocratic systems beyond the Chinese and Russian cases remains scarce. This paper offers a simple agency model that incorporates observable credentials; it then leverages newly digitised biographies of high-level bureaucrats in Fascist Italy to assess the impact of such credentials on the level of surveillance the agents executed.

Our model defines general conditions under which signalling drives policy implementation, and best applies to the most sensitive policies (e.g. surveillance, censorship, repression). It conceptualises credentials as informative of underlying ideological alignment, which makes agents more or less willing to comply with regime objectives. For

simplicity, we assume that credentialed agents (henceforth, *partisans*) are always aligned, while agents lacking credentials (*non-partisans*) might not be. This set-up yields a core implication: if revealing alignment is sufficiently rewarded, partisans exert less effort than non-partisans, despite being more aligned, because they do not need to signal their alignment to the leadership.

We support this argument empirically in the context of the National Fascist Party (PNF)'s dictatorial rule of Italy (1922-1940). Prefects - the regime's top provincial agents - were central to the surveillance apparatus of the regime. To analyse their behaviour, we link newly digitised biographies (Cifelli, 1999) and appointments (Missori, 1989) of all 415 prefects to 99,583 individual surveillance reports (*Casellario Politico Centrale*, herein CPC). Importantly, these reports concern only potential political opponents and not ordinary criminal activity. We operationalise credentials by systematically associating prefects with their Fascist Party enrolment date. We distinguish voluntary members (partisans) from non-members or mandated members (non-partisans). We further differentiate early partisans and late ones, using the party's rise to power in October 1922 as a demarcation line: early allegiance provides a more genuine indication of ideological alignment, given the limited material advantages of joining what was then a fringe political movement. We then compare how the same province was surveilled by agents with different credentials over time, leveraging a robust Difference-in-Differences framework with staggered, non-absorbent treatment adoption (de Chaisemartin and D'Haultfœuille, 2024). We find that prefects who joined the Fascist Party early, i.e. credible partisans, directed the political police to open between 18 to 22% fewer surveillance records relative to non-partisan colleagues, on a province-year mean of just over thirty new individuals surveilled.

As multiple phenomena might explain this finding, we corroborate our signalling mechanism leveraging two empirical implications: one from our model, and one adapted from Luo and Zakharov (2025). First, our signalling equilibrium presents higher returns to effort for partisans relative to non-partisans, because non-partisans have to work much harder to reveal alignment. We operationalise returns as the likelihood to retain office given the intensity of surveillance, and observe that partisans achieve a comparable level of job security at much lower levels of surveillance output. Secondly, Luo and Zakharov highlight that security services trying to signal effectiveness will disproportionally

target segments of the population which are most likely, ex-ante, to dissent. A mediation analysis shows that this is the case in our context: non-partisans target working-class individuals much more, as they represented the most likely opponents of the regime. This evidence further corroborates our signalling interpretation.

We also leverage the richness of prefects' biographies to empirically explore alternative explanations, finding limited to no support for them. The mere rotation of prefects, regardless of their credentials, is not associated with drops in surveillance. Most interestingly, the introduction of competence proxies drawn from biographies, i.e. experience on the job and academic achievement, does not dent the core finding. Moreover, we leverage prefects' birthplaces to argue against credible fascists holding more sway on their appointments, as they are not more likely to be deployed close to their birthplaces; this also speaks against embeddedness playing a decisive role, together with the absence of an association between appointment distance from birthplaces and levels of surveillance. Instead, we find limited evidence in favour of deterrence, as fascist prefects' ruthless reputations might be leading the opposition underground, implying more limited surveillance needs. However, this might also have the opposite effect: a more careful and restrained opposition might heighten, rather than reduce, the surveillance required. We also caveat that we cannot directly speak to explanations based on the wide policy portfolio prefects managed or to the possibility that they resorted to complementary informal modes of information acquisition that were not recorded.

In sum, volumes of surveillance, reward rates, and target selection, together with the absence of strong evidence in favour of other sensible explanations, all point to signalling alignment playing a decisive role in the implementation of sensitive policies in autocratic settings.

The core contribution of this paper is to the study of bureaucratic behaviour. Brierley et al. (2023) point to the lack of evidence on high-level personnel decisions in bureaucracies outside the United States. Our case study offers precisely that: we analyse heads of provinces in a profoundly different historical context. While the case aligns with similar responses to alignment requirements by officials in democracies (Geys et al., 2025), we offer a rare empirical analysis of security officials' behaviour under autocracy: one line of research underlines the importance of partisan credentials and political alignment for successful policy implementation (e.g. Rivera, 2020; Spenkuch et al., 2023), while another

highlights career incentives and strategic signalling performed by regime outsiders, who need to establish their trustworthiness (e.g. Svolik, 2012b; Scharpf and Gläßel, 2020; Hassan et al., 2022). Our case suggests that outsiders face sharper incentives to act, and extends the detouring logic in Gläßel and Scharpf (2026) from job selection to within-role performance: non-partisan prefects signal loyalty by working harder in the same post rather than by volunteering for "dirtier" assignments.

Empirically, this supports findings mostly coming from the Chinese (Qian and Bai, 2024; Jia et al., 2015; Carter et al., 2025) or Russian (Baturo et al., 2024) contexts, underscoring the importance of the interaction between connections (which we might interpret as credentials), trustworthiness, and career prospects. We complement these insights by offering simple game-theoretical definitions of relevant concepts and with a more exhaustive empirical exploration of the mechanisms leading to such results. Furthermore, it echoes Fontana et al.'s (2025) finding that Mussolini's visits increased the number of exiled dissidents, but focusing on the cogs in the machine: state agents.

Secondly, we substantively connect with the literature on surveillance, contributing a historical exploration of physical surveillance to work largely focused on digital technologies in China (e.g. King et al., 2013; Xu, 2021; Beraja et al., 2023), with few notable exceptions. Dipoppa and Pezone (2025) leverage the same surveillance data to study how education and class determine which individuals are surveilled, while Hager and Krakowski (2022) delves into the consequences of surveillance in communist Poland. Our study, instead of focusing on the characteristics or reactions of the watched, delves into the motives of the watchers.

Finally, we assemble one of the most comprehensive micro-level historical datasets on high-level bureaucrats to date. While we focus on the period from 1922 to 1940, we have newly digitised biographical information (Cifelli, 1999) and appointment sequence (Missori, 1989) for all Italian prefects from 1861 to 1945. This will allow students of bureaucracy, policing, and Italian politics to track the highest state officials deployed on the territory.

The rest of the paper is structured as follows. The following section sketches the literature we build on, outlines our model, and highlights its key empirical implication. Section 3 contextualises the Italian case. Section 4 introduces the data on surveillance and our novel dataset on prefects. Section 5 presents the core result, followed by an in-

vestigation of the mechanisms underlying our findings in Section 6. Section 7 concludes.

2 Theory

Bureaucratic delegation in autocracies can be understood as a many-to-one accountability problem (Weingast, 1984; Holmstrom, 1982): the autocrat must ensure a large number of bureaucrats will not resist or shirk their policies.[1] The problem is especially sensitive in the area of security (Dragu and Przeworski, 2019; Tyson, 2018) due to the potential danger it poses to the citizenry and its importance for the stability of the rulers (Svolik, 2012a).

As genuine ideological alignment is hard to engender (De Juan et al., 2021), autocrats mitigate moral hazard by introducing individuals whose alignment is certain. These agents might have earned their trust through personal connections to the leadership (Lorentzen and Lu, 2018; Jia et al., 2015; Bai et al., 2023), have a long-time allegiance to the ruling faction (Francois et al., 2023), or be otherwise credentialed. The introduction of these partisans is often motivated by the need to implement policies that otherwise available personnel would resist or reject (Bauer, 2026). This suggests that it is natural to watch the introduction of partisans in the bureaucracy and the extent to which they implement these policies if we want to gauge how restrained, consolidated, or unhinged an autocracy is.

Some empirical findings echo this reasoning: in Nazi Germany, when security overtook economic performance as a priority, autocratic leadership elevated partisans to carry out "extreme measures" (Aaskoven and Nyrup, 2021, p. 51). We broaden this label and define the policies we are interested in as "sensitive", on account of their importance for the regime, divisiveness among civil servants, and disputed morality. Some examples might be mass deportations, domestic military deployment, or surveillance of political opponents.

Yet, partisans need not be the most dangerous functionaries. Despite career civil servants often being described as bulwarks against autocratisation (Guedes-Neto and Peters, 2025), these bureaucrats frequently prove quite apt at implementing sensitive measures (e.g. Mattingly, 2020, on the one-child policy). The literature largely explains

[1] For evidence of this taking place under democracy, see Wirsching (2025).

this based on their experience or ability. For example, more administrative capacity in formerly Prussian towns meant better trash collection under the Weimar Republic and more efficient deportations of German Jews under the Third Reich (Heldring, 2026).

However, ability is unlikely to be the sole explanation. Beyond being more capable, non-partisan bureaucrats might be more interested in delivering sensitive policies to signal their alignment with the autocratic leadership. As their ideological alignment is more uncertain, autocrats might be interested in screening them. When direct assessment of ideology is infeasible, alignment is inferred based on performance (Montagnes and Wolton, 2019; Wang et al., 2022).[2] Qian and Bai (2024) find evidence compatible with this argument examining the Anti-Rightist Campaign in China. They compare Red Army veterans, who had fought side-by-side with Mao, and former undercover agents, who had operated in Kuomintang-controlled areas, showing the latter had engaged in broader purges. The paper argues that this was an attempt to overcome the mistrust Mao had for agents who did not share with him their history of revolutionary action.[3]

In sum, the literature suggests that being a trusted partisan affects the willingness to implement sensitive policies in two contrasting ways. Greater ideological alignment implies ease with said policies, which we conceptualise as a lower implementation cost. However, not having to establish their alignment deprives them of the motivation to signal that they are on board with regime objectives.

To discipline our reasoning and account for both channels, we construct a simple model of effort. Two versions stylise the two competing arguments in the literature and generate the empirical implications guiding our analysis. A self-contained analytical treatment of the model is reported in Section A.

Set-up An agent (he) is observed and rewarded by a principal (she), depending on his effort and characteristics. The agent might be a partisan agent i with certain ideological alignment $\theta = H$, or a non-partisan agent j with uncertain alignment $\theta \in \{L, H\}$. His alignment is private information, and with some probability π he is misaligned: $\Pr(\theta = L \mid j) = \pi \in (0, 1)$. For brevity, we refer to partisans as i agents, misaligned non-partisans as L agents, and aligned non-partisans as H agents. Table 1 synthesizes the three agent

[2]Crabtree et al. (2020) show that personality cults also serve to infer alignment.

[3]Similarly, De Juan et al. (2021) argue that GDR bureaucrats increased their system engagement without genuine norm internalisation once indoctrination made them acquainted with elite preferences.

typologies.

TABLE 1. AGENT TYPOLOGIES

Agent Label	*Credentials*	*Alignment* (θ)
i	Partisan (i)	Aligned (H)
H	Non-partisan (j)	Aligned (H)
L		Misaligned (L)

The agent can decide how much effort $x \in \mathbb{R}_+$ to put into implementation. The policy is sensitive, hence the cost of this effort depends on the agent's true alignment $C(0, \theta) = 0$ such that implementation is easier for aligned agents: $C_x(x, L) > C_x(x, H)\ \forall x$.

Nature draws these characteristics, and the agent puts in some effort. The principal observes it and forms a posterior $\mu \in [0, 1]$ about his alignment being high, then rewards him according to the relevant rewarding technology.[4]

In the *Baseline* case, a rewarding technology $P(x)$ determines the benefit of the agent. It is increasing in effort, convex, and intersects the origin. In the *Signalling* case, the technology depends on revealed alignment as well. For any effort level, an agent revealing more alignment is rewarded more.[5]

Equilibria We identify Perfect Bayesian Equilibria with general functions in both cases, and represent them in Figure 1. To solve the Signalling Case, we assume that non-partisans exerting less effort than the equilibrium level of H agents are interpreted as misaligned.

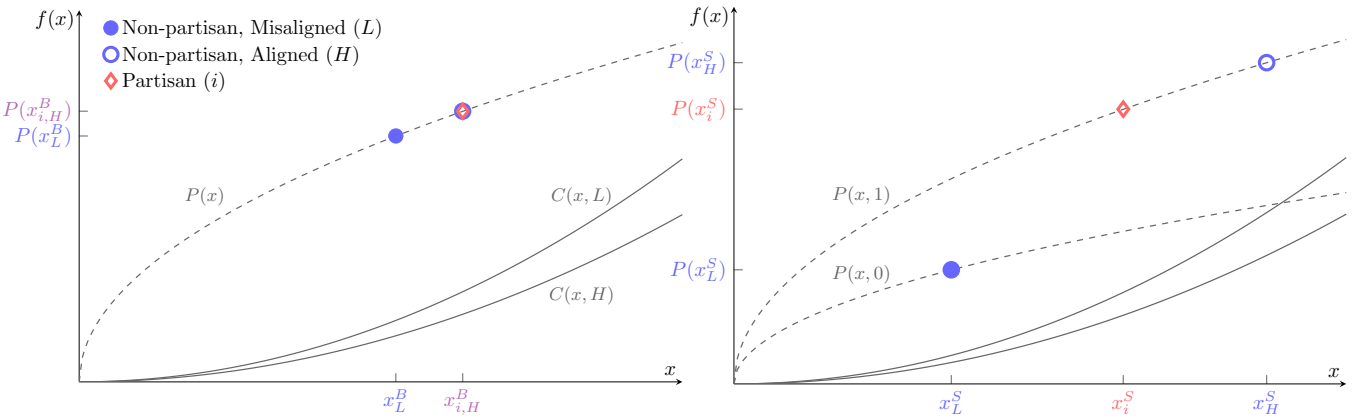
Panel 1a represents the trivial equilibrium that the Baseline Case delivers: as revealed alignment is not rewarded, all the agents simply maximise their payoff given their cost function: facing a steeper increase, misaligned agents exert less effort, while aligned agents equally exert higher effort, regardless of their credentials.

Panel 1b represent a less intuitive picture. The key conditions for this type of PBE are that (i) H agents prefer separating themselves from L agents instead of optimising their effort along the low reward schedule, and that (ii) L agents would set effort as i agents, if that were enough to reveal an aligned type. The latter distinguishes the *interesting*

[4]Note that the principal does not strategically select the rewarding technology: determining which would be most advantageous for her is beyond the scope of this model.

[5]As implied by the single-crossing condition $P_x(x, \mu) > P_x(x, \mu')\ \forall x,\ \forall \mu > \mu'$.

FIGURE 1. EQUILIBRIA VISUALIZATION



(A) BASELINE EQUILIBRIUM (B) INTERESTING SEPARATING EQUILIBRIUM

Notes. Functional forms and parameter values for the illustrations are listed in Table A2. $C(x, \theta)$ are cost functions dependent on the true agent type. $P(x)$ is the payoff function. In panel 1b, $P(x, \mu)$ also depends on the type posterior μ.

separating equilibrium, of which we demonstrate existence, from the uninteresting one.[6]

In the interesting separating equilibrium, i agents optimise on the high reward schedule, as their alignment is not in question. L agents optimise on the low reward schedule, truly revealing their type. That is because H agents exert more effort than would be optimal solely based on their cost function (i.e. higher effort than i agents) for the purpose of separating themselves from misaligned colleagues.

This classic signalling result *à la* Spence (1973) yields two empirical implications in our setting. To derive them, we need to further assume that effort noisily but unbiasedly predicts policy output (i.e. the number of surveillance dossiers), as that is the only empirical quantity we observe. This allows us to use rankings of output to infer rankings of effort.

The first implication is based on predictions about relative effort within equilibria between agent credentials. Proposition 1 shows that non-partisans always exert more effort than non-partisans in the baseline equilibrium. By contrast, Corollary 1 establishes that in the separating equilibrium, rewarding revealed alignment can reverse this ordering: when the prevalence of low types is sufficiently small, partisans exert less expected effort than non-partisans. Both intuitions are visualised in Figure A1.

Combining these two results leads to the following empirical implication about which case best describes reality:

[6] In the uninteresting separating equilibrium, H agents would exert as much effort as i agents.

Implication 1 (*Effort Levels*) Observing lower policy output by partisans provides evidence against the baseline case and in favour of the signalling case.

This intuition is visualised in Figure A1. The second empirical implication on returns to effort we derive from this model is detailed in Section 6.

In synthesis, the threat of being rewarded less per unit of effort if revealing low alignment leads aligned non-partisans to put extra effort to differentiate themselves from misaligned ones. If misalignment is not too widespread among non-partisans, they will exert, on average, more effort than partisan peers. This suggests that, in contexts where revealing alignment is meaningfully rewarded, regular bureaucrats might implement sensitive measures more intensively than day-one regime supporters, even if, on average, they are less ideologically aligned. The logic of this simple setup closely mirrors the conclusions in Luo and Zakharov (2025), who theorise that security agencies inflate repression levels to signal effectiveness.

We test this intuition empirically in Section 5. First, Sections 3 and 4 respectively detail the historical context and the data used. We introduce a second intuition from our model when discussing mechanisms in Section 6.

3 History

In Fascism's narrative, Italy became a dictatorship right after the March on Rome - 28 October 1922 - when King Victor Emmanuel III called upon Benito Mussolini to form a new government. In practice, though, Fascism's rise to power was more gradual. As Morgan (1998) noted, to consolidate its grasp on power, the new regime had to compromise with existing powers: the monarchy, the Church, and the policing, judicial and executive apparatus of the state.

In the political arena, Mussolini's rise to power was relatively rapid. Its first crucial step was the 1923 electoral law, which granted two-thirds of the MPs to the party that won the electoral majority, provided it reached at least 25% of the vote. In the following national elections of 1924, the National Fascist Party[7] secured 64.9% of the vote as part of the "National List" coalition with liberal and centrist parties. The success was in no small part due to widespread political violence and intimidation. After publicly

[7] In Italian, *Partito Nazionale Fascista*, hereafter PNF.

denouncing these irregularities in Parliament, Giacomo Matteotti, a socialist MP, was murdered by a Fascist hit squad in June 1924. In response, opposition parties began boycotting all parliamentary activities. Their absence allowed Mussolini's government to survive a motion of no confidence and pass several laws that granted the PNF *de facto* control over Parliament and the country. Between December 1925 and November 1926, the government suspended the rights to strike and assembly, outlawed non-Fascist labour unions, and banned boycotting opposition parties, removing their MPs from Parliament. Furthermore, the regime imposed censorship, established a system of internal deportation (the *confino*), and created the OVRA secret police.[8] Establishing the Special Tribunal for the Defence of the State[9] further institutionalised political repression (e.g. Panza et al., 2025).

The PNF also formalised its grasp on local administrations. In 1926, Mussolini replaced elected mayors and municipal councils with a single official - the *podestà* - directly nominated by the government via royal decree.[10] As the PNF progressively transformed into a more traditional - albeit ubiquitous and pervasive - political party, the provincial administration became its centre stage. Early local leaders - or their trusted lieutenants for those who reached positions of power in Rome - came to head the provincial sections of the PNF as federal secretaries. Even if the party organisation grew to fully identify itself with the State, Fascism also used the existing executive apparatus. Since unification, Italian provinces have been headed by a prefect, the direct representative of the government at the local level. The Italian prefects were civil servants and politicians at the same time.

As such, prefects usually followed the fortunes of the government that nominated them. New governments often made sweeping changes in the ranks of prefects to appoint their men. The first Mussolini government arguably followed this tradition in retiring and transferring many prefects in late 1922 and early 1923 (Morgan, 1998; Tosatti, 2001)[11]. Most of these movements were politically motivated. For instance, the prefects of Potenza, Reggio Emilia and Brescia, nominated by Prime Minister Nitti - the

[8]The acronym has no clear meaning. It is often interpreted as standing for *Opera Vigilanza Repressione Antifascismo*, the Organisation for Anti-fascism Surveillance and Repression.

[9]In Italian, *Tribunale speciale per la difesa dello Stato*.

[10]Law 4 February 1926, n. 237 and R.D. 3 September 1926, n. 1910.

[11]Between 8 November 1922 and the end of the year, sixty-two prefects changed position between new appointments, transfers, and early retirements. In 1923, there were seventy-one such movements (see Tosatti, 2001; Melis, 1996).

first coming from the so-called radical left[12] - in 1919-20 were sent into early retirement by the end of 1922. Others, such as the prefects of Cagliari, Udine, Campobasso, Genoa, Caserta, Pesaro, Turin and Verona, were forced to retire due to severe disagreements with the local fascists (see Saija, 2001, pp 438-40).

Prefects had almost complete control over the local administration of their province. They, for instance, convened and dissolved municipal councils, allocated the police force on the territory, dealt with emergencies and generally kept the central government informed on the local situation.

Then, if the prefect was the representative of choice of the old liberal governments, who could better represent the new Fascist government than the provincial federal secretary of the PNF, with his direct connection with either the *Duce* or his closest associates? In June 1923, Mussolini answered this question by stating that "only representative of government authority in the Province is the prefect and no one but him. (...) Provincial fascist representatives as well as other party authorities are subordinate to the prefect."[13] Mussolini later claimed to have asked the party in 1923 to give him "seventy-six prefects and seventy-six police chiefs"[14]. The PNF would then have taken over the executive and police structures in all Italian provinces. At a stage in which real political power resided with the party, leaving it for a civil servant's career probably came with a considerable loss of personal power. Nonetheless, this general fascistisation of local bodies did not materialise. Mussolini found compromising with the existing civil service easier, coopting it into the new regime (Morgan, 1998).

By the early 1930s, the latent conflict between the federal secretaries and prefects had been resolved in favour of the latter, at least formally. The prefect answered to the interior minister, while the PNF's federal secretary answered both to the PNF central secretary - the official head of the party - and the prefect itself. This implied that, barring an internecine conflict in the party-state apparatus, the PNF central secretary had to nominate a *federale* amenable to the prefect, and not vice-versa (Di Nucci, 2010, pp 419-22).

The progressive empowerment of prefects as the local arm of state repression under

[12]The Italian radical left comprised the socialist, republican and radical parties. Francesco S. Nitti (1868-1953) was the first member of the Italian Radical Party (PRI) to ever serve as Prime Minister.

[13]Circular sent by telegraph on 13 June 1923, reported in Tosatti (2001). Translation by the authors.

[14]Speech to the Chamber of Deputies on 26 may 1927, reported in Tosatti (2001). Translation by the authors.

Fascism took many steps. In 1925, a law allowed them to dismiss all public employees responsible for "activities incompatible with the general political directives of the Government" (Fried, 1967, p 179). The reforms of 1926 left the prefect even more in control of the local apparatus dedicated to political repression[15]. With the creation of the *confino* system, the prefect came to head the provincial commission that decided on the punishment to be inflicted on denounced "anti-fascists".[16]

4 Data

To examine how credentials influenced surveillance levels in fascist Italy, we build a province-year dataset that maps prefects in office to surveillance dossiers.

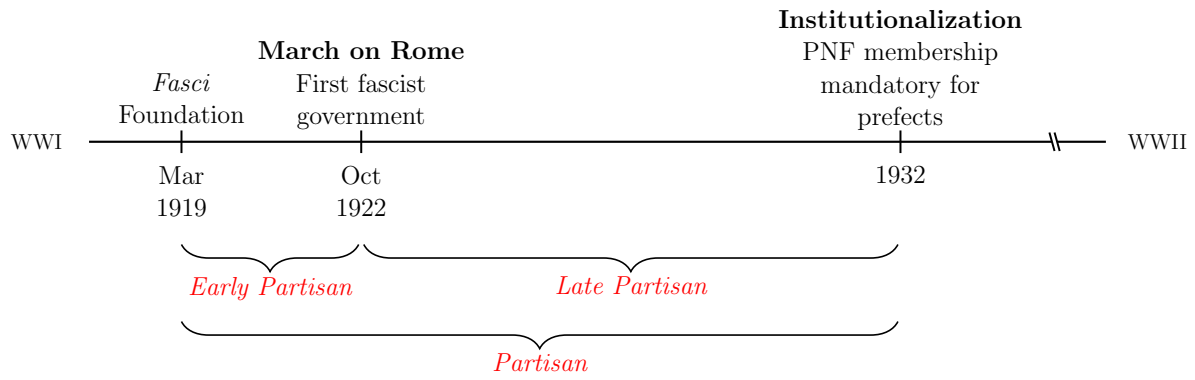
Data on the agents is systematised in two steps. First, we digitise biographical details on prefects from Cifelli (1999), extracting variables such as age, birthplace, professional background, education and, most importantly, partisan credentials. Second, we digitise the universe of prefects' appointments from Missori (1989), which allows us to locate prefects' appointments in provinces with daily precision, and merge them with the prefect's characteristics.

As anticipated, our main independent variables are partisan credentials, based the date of affiliation to the National Fascist Party, if any, reported in Cifelli (1999) for each prefect. Figure 2 summarises our approach to the definition of the three dummies in red.

Partisan broadly distinguishes between partisan and non-partisan prefects based on whether party affiliation reflects discretionary political credentials or a purely formal requirement. From 1932, PNF membership formally became mandatory to be promoted to the highest echelons of the Italian public administration, including prefectural positions.[17] We residually classify prefects who received party membership after that date as non-partisan, as their affiliation reflects an institutional prerequisite rather than a discretionary political credential.[18] Since their enrolment was compulsory, such membership is not evidence of political alignment.

[15]Law 3 April 1926, n. 660.
[16]Law 6 November 1926, n. 1848.
[17]See the Prime Minister Decree, 17 December 1932, on requisites for public employment
[18]This distinction is already present in Cifelli (1999), which reports the PNF membership date only for those prefects that enrolled before it became mandatory.

FIGURE 2. PARTISAN CREDENTIALS BASED ON PNF MEMBERSHIP DATE, 1918-1940



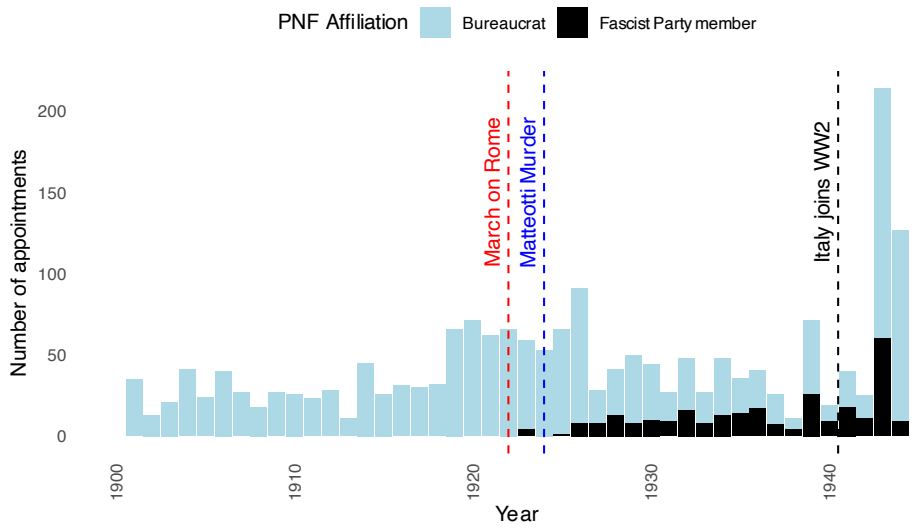
Notes: We define three dummies based on the date in which the prefect acquired his membership in the PNF (or in its direct antecedent, the *Fasci di Combattimento*). *Partisan* identifies any agent that voluntarily became a member of the party. *Early* and *Late Partisan* identify agents that obtained their membership before and after the party rose to power, respectively. All other prefects, i.e. those who did not become members voluntarily, are residually classified as *non-partisan*.

Before 1932, however, party enrolment was voluntary and implies a degree of political alignment with the regime's methods and objectives. Nonetheless, this broad credential is clouded by political convenience, as a PNF membership favoured public careers long before it became a formal requirement (Dunnage, 2012). To refine this distinction, we further differentiate among partisan prefects according to the timing of their membership acquisition.

Early Partisan identifies prefects who joined the PNF or its predecessor, the *Fasci Italiani di Combattimento*, before October 1922, i.e. the March on Rome and the first Mussolini government. These individuals acquired their partisan credentials before the party's political success, often played an active role in its violent formative years, and in some cases faced substantial risks from the state and opposing forces. We interpret this credential as a stronger indication of alignment. Conversely, *Late Partisans* marks prefects who joined the PNF between the March on Rome and 1932. While still discretionary, these credentials may reflect a more opportunistic investment in political capital rather than deep ideological alignment. Given the extensive system of honorifics and *ad hoc* decorations awarded by the PNF to early members,[19] this distinction appears salient to party leadership.

[19]For instance, participants in the March on Rome received a state-sanctioned commemorative medal in December 1923 (F.O. M.V.S.N. 31 December 1923). A ministerial circular of 20 January 1930 included, among the merits recognised for public officials, participation in the March on Rome, attested by a certificate issued by the PNF.

FIGURE 3. PREFECT APPOINTMENTS BY PARTISAN CREDENTIALS



(A) YEARLY APPOINTMENTS, 1900-1944

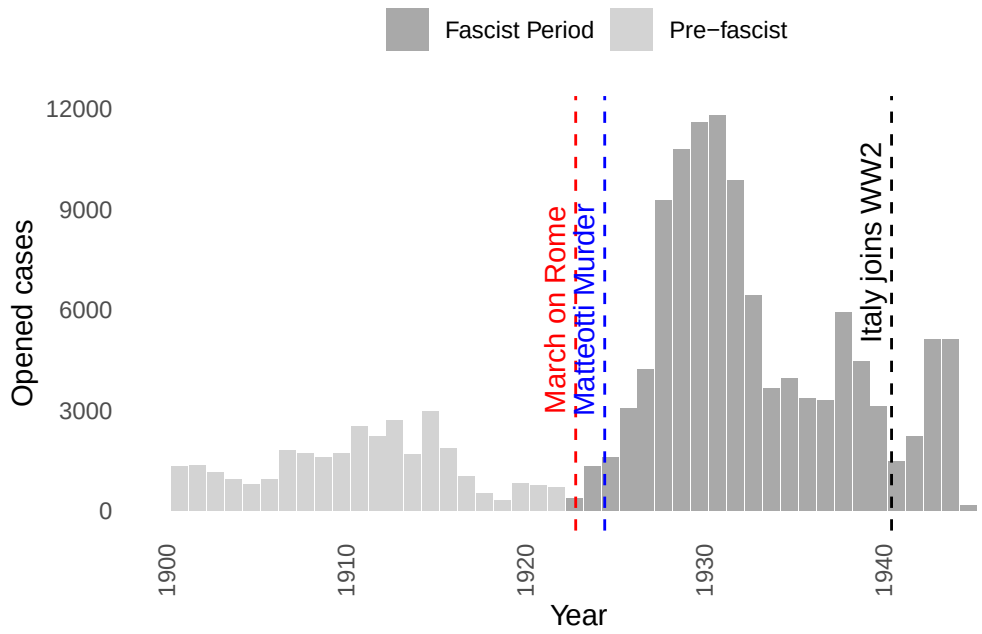
Source: Authors' elaboration on data from Missori (1989) and Cifelli (1999).

We then leverage the appointment records in Missori (1989) to locate prefects across years and provinces. To accurately reproduce the geographies at the time, we produce a previously unavailable digital map of Italy in the interwar period, which includes territories later ceded to Slovenia and Croatia.

Our main dependent variable is the number of surveillance records opened at the provincial level each year. We calculate this quantity aggregating individual-level data on surveillance targets from the digitised Central Political Records Office archive (CPC), housed at the Archivio Centrale dello Stato.[20] Established in 1894 to monitor political opponents, the registry expanded significantly during the Fascist era to include communists, socialists, anarchists, and other groups targeted by the regime for political reasons. The archive contains a total of 152,589 personal files, primarily covering the period from 1894 to 1945. These files document individuals through biographical cards, police reports, interrogation records, and other materials. This dataset provides detailed information on the affiliations, professions, municipalities, and ages of individuals. Figure 4 illustrates the types of information derived from this source, while Section C offers examples of the data structure through the file of Caterina Picolato in Table A6. Each record also includes information on the start and end dates of surveillance, as well

[20] Accessible online at http://dati.acs.beniculturali.it/CPC/.

FIGURE 4. YEARLY NEW SURVEILLANCE RECORDS, 1900-1944



Source: Authors' elaboration on data from Archivio Centrale dello Stato.

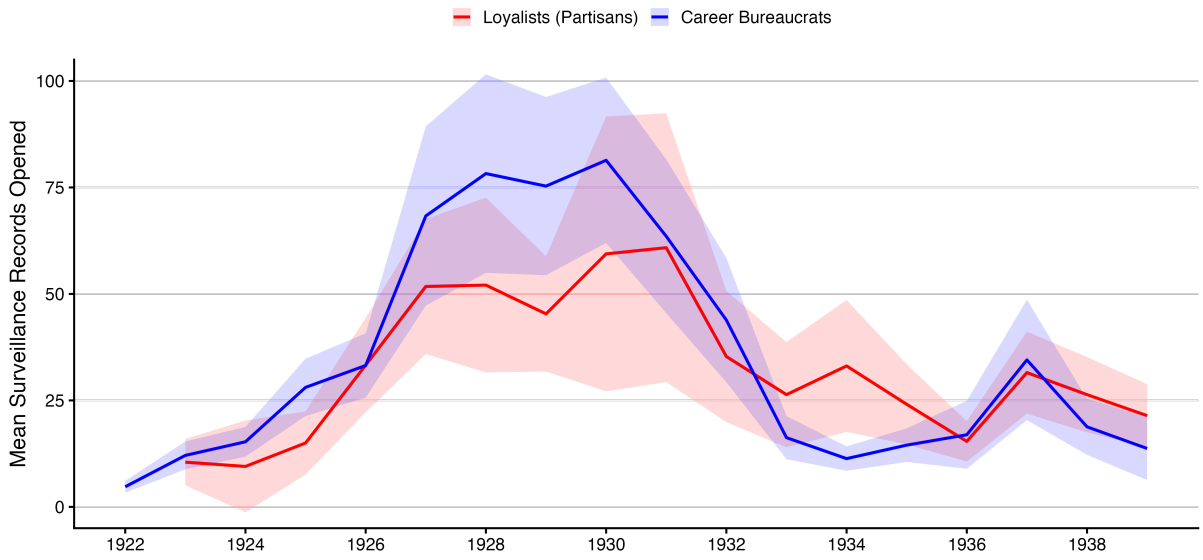
as notes on measures taken against individuals (see Figure A4). Different subsets of the same source have underpinned other quantitative studies: beyond the aforementioned Dipoppa and Pezone (2025), Panza et al. (2025) isolate Special Tribunal referrals and investigate the long-term effects of anti-fascist repression.

Combining these two rich sources, we construct a dataset with one observation for each province-year, recording the characteristics of the currently appointed prefect and the amount of surveillance they directed. Because surveillance data are available only at the yearly level while prefects can change within a year, we ultimately set $Partisan_{p,t}$ to the share of prefect-days in year t held by a partisan prefect in province p. The variable ranges from 0 to 1, where a value of 0.5 would indicate that partisans held the prefecture for six months.[21]

Prefects were granted considerable discretion. For instance, Figure A5 presents a letter from the Prefect of Turin to the Interior Minister, requesting the termination of surveillance on Caterina Picolato. The Prefect justifies this by noting that she "behaves well and, despite not being a PNF member, is compliant with the institutions of the regime".

[21] Most observations are exactly 0 or 1; results in Section 5 will be robust to excluding fractional values (see Table A7).

FIGURE 5. YEARLY MEAN OF NEW POLICE RECORDS, 1922-1940



Notes: Raw annual means of the amount of surveillance records opened in provinces with (red) and without (blue) an early partisan in office; shaded bands are 95% confidence intervals.

The resulting dataset connects a very granular account of the universe of recorded surveillance in Fascist Italy with an original mapping of the political credentials of the bureaucrats in charge of it. The following section details the patterns connection we can draw between these two elements.

5 Main Result

This section details the core result of the study: partisan bureaucrats surveil political opponents less than non-partisan colleagues. Recalling *Implication 1*, in the framework of our model this is evidence in favour of signalling dynamics being core determinants of bureaucratic activity.

Ahead of outlining the estimation strategies deployed to isolate a plausibly causal effect, Figure 5 plots the raw, calendar-year means of opened surveillance records, separating provinces with an early partisan in office from those without one. These raw means already show provinces headed by credibly partisan prefects opening relatively fewer surveillance records, particularly between 1926 and 1934, where the main increase in levels took place.

This, however, might be caused by long-standing differences among provinces,

e.g. population and political character, which we should expect to be correlated with assignment decisions. It is, moreover, unclear if this tendency is a significant one across the whole period.

To address these doubts, we turn to two complementary estimation strategies—TWFE and DiD regressions—to establish the presence of a robust causal connection between prefects' credentials and surveillance levels. Formally, our target estimand is the average treatment effect on the treated (ATT), i.e., the average difference in surveillance activity in provinces after the of a partisan prefect relative to what would have been observed had a non-partisan prefect been appointed.

5.1 Two-Way Fixed Effects

The first empirical strategy we deploy is a series of panel OLS regressions with provinces as units of observation and years as periods. The analysis is restricted to the period between 1922, when fascism took power, and 1940, when Italy entered the Second World War. Our baseline regression is a simple two-way fixed effects (TWFE) model, specified as follows:

$$\text{Surveillance}_{p,t} = \beta_1 \text{Partisan}_{p,t} + \alpha_p + \gamma_t + \varepsilon_{p,t} \tag{1}$$

where β_1 is the estimand of interest. Our key explanatory variable, $Partisan_{p,t}$, measures the presence of a prefect with partisan credentials: we consider all voluntary members (columns 1 and 3 in Table 2), then we separate early and late partisans (columns 2 and 4), according to the definitions in Figure 2. The dependent variable, $Surveillance_{p,t}$, represents the number of new surveillance dossiers opened in province p during year t. α_p and γ_t account for province-level and yearly fixed-effects, respectively. Finally, $\varepsilon_{p,t}$ represents the error term.

Our identification strategy relies on parallel trends: absent the appointment of a partisan prefect, surveillance activity in treated provinces would have evolved as in control provinces. A violation would arise if partisan prefects were deployed to provinces expected to require less surveillance in the immediate future. This cannot be directly observed; however, Figure 6 shows no differential pre-trends, and the empirical exploration of preferential deployment in Section 6.2 finds no evidence consistent with

it.

Table 2. Partisan Prefects Surveil Less Relative To Non-partisans

Dependent Variable:	Number of Records Opened			
Model:	(1)	(2)	(3)	(4)
Variables				
Partisan	-5.622**		-5.468**	
	(2.480)		(2.687)	
Early Partisan		-6.845**		-6.284**
		(2.641)		(2.860)
Late Partisan		2.194		-0.087
		(6.387)		(6.560)
Restricted Sample			Yes	Yes
Fixed-effects				
Province	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Dependent variable mean	31.057	31.057	31.766	31.766
Observations	1,794	1,794	1,412	1,412
Adjusted R^2	0.613	0.614	0.619	0.619
F-test	6.033	5.825	6.348	6.120

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The result of fitting the model in Equation 1 with the different partisan credentials is reported in Table 2. In columns 1 and 2, the reference category is non-partisan prefects, i.e. bureaucrats who were not voluntary members of the Fascist National Party. Hence, non-partisan agents are defined residually. In columns 3 and 4, we instead restrict the comparison to prefects who were not voluntary party members and were explicitly recorded by Cifelli (1999) as having entered the administrative career through a public examination process.

The headline result, consistent across samples, is that provinces receiving a partisan prefect display fewer police records being opened per year (columns 1 and 3). Partisans are associated with more than five fewer political surveillance records, amounting to approximately 18% of the yearly provincial mean.

This correlation, however, seems to be influenced by the credibility of the credential. In fact, differentiating between early and late partisans, we see that the effect is driven by

prefects who acquired partisan credentials before the party seized power in October 1922 (columns 2 and 4), who open about 22% fewer records than non-partisan appointees. Nonetheless, estimation of the behaviour of late partisans might suffer from a lack of statistical power due to their limited numerosity.

We note that these results are not tied to the years with partial credential values, in which a change of prefects implies only a fraction of the year was presided over by a partisan. We address this concern in Section D.1 in two ways. First, we exclude province-years with partial treatment exposure. Second, we fill in fractional values and introduce a dummy variable to account for partially treated years. Both exercises yield the same results.

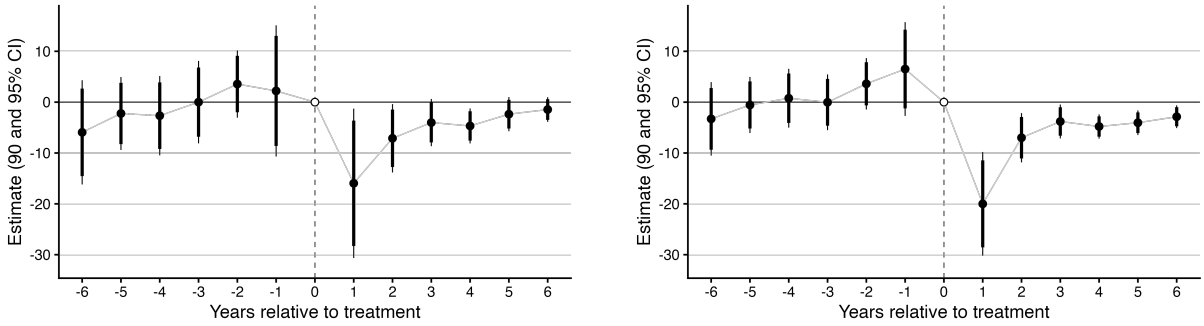
Secondly, prefect turnover alone might be causing readjustments due to the lack of location-specific knowledge of the new prefects. This would be consistent with the larger drop in surveillance records observed in the first year of treatment in Figure 6. However, turnover cannot account for the result if we disregard credential status. In Section D.2, we introduce a dummy that identifies any prefect change, which does not show any systematic effect on the intensity of surveillance.

5.2 Robust Difference-in-Differences

In our setting, TWFE estimates are likely to be biased: treatment is staggered, is defined continuously, it might have heterogeneous effects in time, and units experience treatment exit. To test the robustness of the finding, we rely on the routines proposed by de Chaisemartin and D'Haultfœuille (2024), which provide us with a DiD estimator that accounts for all four sources of bias listed.

Intuitively, the estimator compares the change in surveillance activity for provinces that receive a partisan prefect at a given time with the corresponding change in provinces that have not yet received such a prefect at the same point in relative time. These comparisons are then aggregated across appointment cohorts to estimate dynamic treatment effects. In our main specification, we normalise treatment effects by exposure duration. This means that the estimated coefficients represent the effect of having a partisan prefect per year of treatment, rather than the cumulative effect since appointment. Normalisation ensures that treatment effects are comparable across provinces, regardless of whether they received a partisan early or late in the period.

FIGURE 6. EFFECTS OF APPOINTING A PARTISAN PREFECT ON SURVEILLANCE LEVELS.



(A) *PARTISAN* PREFECTS. (B) *EARLY PARTISAN* PREFECTS.

Notes: Estimates obtained using the de Chaisemartin and D'Haultfœuille (2024) staggered DiD estimator, with not-yet-treated provinces as controls. Coefficients are normalised by treatment exposure and represent effects per year. Blue points represent point estimates; red whiskers indicate 95% confidence intervals. Period $\ell = 0$ in relative time is the omitted baseline. Panel 6b controls for late partisans to keep the reference category constant.

Figure 6 presents the event-study estimates. As anticipated, pre-treatment coefficients are statistically indistinguishable from zero, supporting the identifying assumption of parallel trends before treatment.

Following the appointment of a partisan prefect, the number of surveillance dossiers opened in a province drops sharply by roughly 15 dossiers per year and remains lower over the subsequent four years (panel 6a). The effects are even more pronounced when singling out early partisans only (20 fewer dossiers, panel 6b).

Taken together, prefects with partisan credentials oversaw significantly fewer surveillance dossiers after appointment. Specifically, the aggregate difference estimated ranges between 18 and 22% depending on specifications. Interpreted through the lens of our model, this suggests partisans did not need high numbers of surveillance records to demonstrate ideological alignment with the regime. Yet, several alternative explanations would be consistent with this empirical finding. The following section delves into the potential mechanisms.

6 Mechanism

Is what we observe really due to signalling dynamics? The concentration of the main effect in early partisans, displayed in Table 2, is consistent with this interpretation. Late partisans do not escape the leadership's suspicion and are not able to put in less effort,

as joining a party after it gains power could be a merely instrumental move, hard to distinguish from sycophantic cheap talk (Baturo et al., 2024).

In what follows, we outline and find support for two empirical implications of signalling beyond this preliminary observation: (i) in our model, partisans are rewarded more per unit of effort; (ii) in the signalling model in Luo and Zakharov (2025), non-partisans tend to round up the usual suspects to display alignment. We also evaluate evidence on other plausible explanations for the main result, and we discuss some we cannot speak to due to data limitations.

6.1 Signalling

Returns to Effort The first argument in favour of a mechanism based on signalling dynamics is based on the difference in the reward per unit of equilibrium effort our model predicts agents will be granted based on their credentials. The assumption of convexity of the reward functions is the cornerstone of this intuition.

Proposition 2 establishes that, in the baseline equilibrium, partisans agents i will be, on average, less rewarded per unit of equilibrium effort. This is because misaligned non-partisan agents L select the steeper section of the single reward function, increasing average returns for non-partisans. Conversely, Corollary 2 shows that, in the interesting separating equilibrium, i agents are rewarded more, provided L agents are not too many. That is due to aligned non-partisans H agents setting higher effort than i agents do, thus obtaining lower returns per unit. This reasoning is illustrated in Figure A2. As a consequence, another testable implication the model delivers is:

Implication 2 (*Returns to Effort*) Observing larger returns to effort for partisans provides evidence against the baseline case and is consistent with the signalling case.

Intuitively, taking the returns to effort of i agents as reference, the returns of L agents could be lower because they reveal misalignment, while the returns of H agents is certainly lower as they need to work far more to reveal their alignment. As a result, equilibrium effort by partisans is rewarded more.

We operationalise rewards with the likelihood of retaining prefectural office, in line with contributions identifying career concerns as core determinants of bureaucratic behaviour under autocracy in the Chinese context (Jia et al., 2015; Carter et al., 2025).

We reshape the data to set aggregate observations at the prefect-mandate level, and we run a simple survival analysis:

$$\begin{aligned}\text{Retain Office}_{i,p,m} = \beta_1 \text{Records}_{i,p,m} + \beta_2 \text{Early Partisan}_{i,p,m} \\ + \beta_3 \text{Records}_{i,p,m} \times \text{Early Partisan}_{i,p,m} + \phi_p + \gamma_m + \varepsilon_{i,p,m}\end{aligned} \tag{2}$$

where $\textit{Retain Office}_{i,p,m}$ is a dummy that marks that prefect i was reappointed after mandate m in province p, and $\textit{Records}_{i,p,m}$ is the number of political police records opened per month. We are especially interested in β_3, which will capture whether record opening is rewarded differently based on agents' credentials. The results are reported in Table 3, where we progressively introduce province and mandate number fixed effects (ϕ_p and γ_m). In columns 3 and 6, we fit logit models.

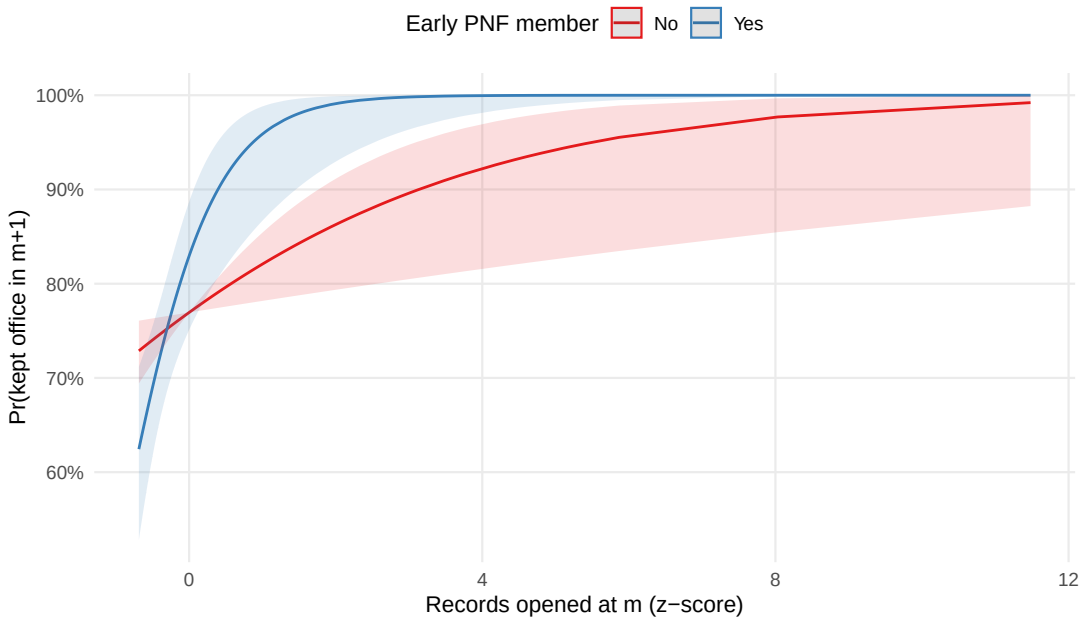
Table 3. Early Partisans Gain More Job Security from Surveillance

Dependent Variable:	Retain Office					
Model:	(1) OLS	(2) OLS	(3) Logit	(4) OLS	(5) OLS	(6) Logit
Variables						
Records Opened	0.001*	0.001**	0.009***	0.001*	0.001**	0.007***
	(0.001)	(0.001)	(0.003)	(0.001)	(0.000)	(0.003)
Early Partisan				-0.088**	-0.092*	-0.508**
				(0.039)	(0.043)	(0.220)
Early Partisan × Records Opened				0.005***	0.005***	0.029***
				(0.001)	(0.001)	(0.008)
Fixed-effects						
Province	Yes	Yes	Yes	Yes	Yes	Yes
Mandate number		Yes	Yes		Yes	Yes
Fit statistics						
Dependent variable mean	0.599	0.599	0.609	0.599	0.599	0.609
Observations	780	779	767	780	779	767
Adjusted R^2	0.025	0.027		0.031	0.033	
F-test	1.751	1.790		0.626	0.632	

Clustered (Province & Mandate number) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Opening more records seems to generally improve prefects' job security (columns 1 to 3). However, it does so differently for early partisans relative to other prefects, as they gain more job security for the same amount of records opened (columns 4 to 6). This is graphically represented in Figure 8, which shows that for almost any level of record opening, partisan bureaucrats have a higher probability of retaining office

FIGURE 7. MARGINAL EFFECT OF EARLY PARTISAN CREDENTIALS ON THE PROBABILITY OF OFFICE RETENTION.



Notes: Predicted probability of being kept in office in $m+1$ from a logistic regression with province and mandate-number fixed effects (model 5 in Table 3). Lines plot predictions over the standardised number of surveillance records opened at m, separately for early partisans and non-partisans; shaded areas are 95% confidence intervals. Uncertainty is computed with two-way clustered standard errors at the province and mandate-number levels.

than non-partisan bureaucrats. The figure also helps us to interpret the substantive importance of the effect: in order to reach the job security of a partisan agent at mean output, a non-partisan agent must perform at about two standard deviations above the mean.

In line with *Implication 2*, we interpret this as evidence of signalling dynamics. This result also mirrors the central observation in Jia et al. (2015) about the complementarity between credentials (in their work, personal connections) and performance for career prospects in China.

Usual Suspects We depart from our analytical framework to leverage the signalling model by Luo and Zakharov (2025). Based on their Proposition 6, security agents who increase repression to signal their effectiveness will disproportionately target social groups that are ex-ante more likely to harbour dissent. That is because the dictator only observes the identity of targeted individuals, not whether they actually represent a menace. Thus, agents "over-repress" more menacing ethnic groups to boost their

Table 4. Causal-mediation decomposition of the *Early Partisan* effect. Mediator: working-class targets

Effect	Estimate	95% CI
ACME (indirect)	–7.24	[–11.26 ; –3.73]
ADE (direct)	0.27	[–1.14 ; 1.76]
Total effect	–6.97	[–10.85 ; –3.56]

Notes: Two-equation OLS with province and year fixed effects; standard errors and confidence intervals from a 1,000-draw cluster bootstrap (province level). ACME = average causal mediation effect; ADE = average direct effect. Point estimates in new records per province-year.

appearance of effectiveness.

This logic travels well to our context, where "over-surveilling" more ex-ante dissenting groups would boost prefects' alignment posteriors. We try to proxy this ex-ante dissent by focusing on working-class individuals, as this was the most fertile segment for anti-fascist opposition (Delzell, 1961).[22] We could also use political labels (e.g. communist, socialist), but these ideological categories might represent "cheap talk" by the prefect classifying individuals. Employment was easier to observe and verify, as ethnicity needs to be in Luo and Zakharov's framework.

If the reasoning applies, partisans should generate fewer surveillance dossiers by neglecting likely opponents, relative to non-partisans who disproportionately focus on them, trying to signal their alignment.

We test this implication in Table 4 using a two-stage causal mediation design and find that it is indeed the case: the effect of credible credentials on surveillance levels is entirely mediated by fewer working-class subjects. This is consistent with non-partisan bureaucrats targeting the ex-ante more suspicious segments of society to display alignment with the leadership.

In sum, the patterns of office retention and the targets selected by prefects suggest signalling alignment[23] has been a major driver of agents' effort in the fascist surveillance apparatus.

[22] Ethnicity was not recorded, had little variation in the population, and was not central to the socio-political landscape at the time.

[23] An alternative way to interpret the findings in Section 6 in conjunction would be that non-partisans over-surveil working-class subjects, thus surveil less effectively and are rewarded less per dossier. This is entirely plausible, and it remains consistent with a signalling interpretation: aligned non-partisans signal by putting more effort and focusing on plausible suspects, proving their alignment at the expense of their effectiveness, which in turn reduces their returns relative to partisans'.

6.2 Alternative Explanations

Several reasons besides signalling might lead to partisan agents putting less effort in political surveillance. We find little to no support for the ones we are able to tackle empirically. At the end of this section, we devote a paragraph to the ones we cannot rule out due to historical data limitations.

Competence When rulers privilege trusted agents, they may do so at the expense of expertise (Colonnelli et al., 2020; Lewis, 2011; Bellodi et al., 2025): evidence from democracies shows that politicised appointments lead to worse program implementation (Gallo and Lewis, 2012) and to a net decrease in agency responsiveness (Lowande, 2019). It would be reasonable to deduce that appointing agents for their credentials disregarded their competence in directing the political police. In addition, newly appointed partisans might have shorter professional experiences, reducing learning on the job (Emeriau, 2023). In sum, they might not be able to surveil the population.

We leverage Cifelli's biographies to extract educational attainment: although we cannot directly assess the ability of these administrators, we follow Besley et al. (2011) in considering a university degree an acceptable proxy for skills when entering the job. Similarly, we compute years of service as a prefect and the number of mandates held from (Missori, 1989) as proxies of the experience gathered. Descriptives in Table A4 show that partisan prefects tend to be younger, enter the career at an earlier age, and are less likely to be university-educated. Lack of competence is thus plausible.

However, the results displayed in Table A11 suggest that competence is not a significant factor in determining the difference in surveillance levels between partisans and other prefects. It does not seem to consistently impact records when considered in isolation (columns 1 to 4), and all three of the proxies we employ do not meaningfully alter the estimate on the impact of early partisans (columns 5 to 8). The same is true when considering all partisans simultaneously, as shown in Table A12.

However, we cannot rule out an argument based on innate ability. Theory establishes that agents are more driven to express loyalty when they are less competent (Egorov and Sonin, 2011; Zakharov, 2016) - in our setting, they will be more likely to become party members. Scharpf and Gläßel (2020) confirm that intuition, showing that ineptitude leads agents towards duties where alignment matters more to implementation than

ability. Nonetheless, this reasoning would not completely apply to individuals taking up membership before the fascist party was in power, unless they predicted this would have turned out to be useful in the future.

Preferential deployment Empirical evidence shows that bureaucrats' personal (Park and Somanathan, 2004), factional (Francois et al., 2023), and ethnic (Hassan, 2017) connections tend to land them in more desirable positions. Accordingly, fascist prefects might receive favourable appointments in already pacified provinces. As anticipated when discussing the parallel trend assumption underlying our interpretation of the results, province fixed-effects would not account for these time-varying levels of opposition. Consequently, another explanation compatible with our findings is that partisans exercise less surveillance because they receive "easy" appointments thanks to their upward embeddedness (Toral, 2024), rather than due to their lack of signalling needs. As we only observe realised surveillance, we cannot directly test this reconstruction. Nonetheless, the evidence we produce speaks against it.

If partisan prefects held more sway over appointments, we would expect them to generally receive more desirable positions. We operationalise this desirability in two ways. First, we leverage the information we have on birthplaces to compute the distance of each deployment location from the prefect's home province. Resting on the assumption that, all else equal, it is generally appreciated to be deployed closer to home, we consider short distance to be another desirable appointment feature. Secondly, we calculate a prestige index for each province based on the number of prefects who held office there and went on to become Members of Parliament, as indicated in their biographies. Since this measure would be endogenous to who is appointed to the province in the first place, we compute the index based on the period from 1861 to 1921 in our dataset, which precedes the appointments we study. It is reasonable to assume that ambitious individuals will seek appointments that have proven useful in advancing their institutional ranks.

We probe the correlation between credentials and the desirability of postings and represent the results in Table A13. We are not able to introduce province or prefect fixed effects when evaluating prestige scores because they would completely capture the dependent and the independent variables, respectively.

Partisan prefects do not appear to consistently receive better appointments. The point estimates are directionally consistent with the hypotheses, but they are largely insignificant. Late partisans seem to be the exception, as some specifications associate them with favourable postings (columns 2 and 8). However, this is possibly additional evidence against preferential deployment explaining lower levels of surveillance, as the effect is driven by early partisans and absent in late ones (see Table 2). In summary, loyalists do not appear to have preferential access to desirable postings, which likely includes locations with waning underlying opposition.

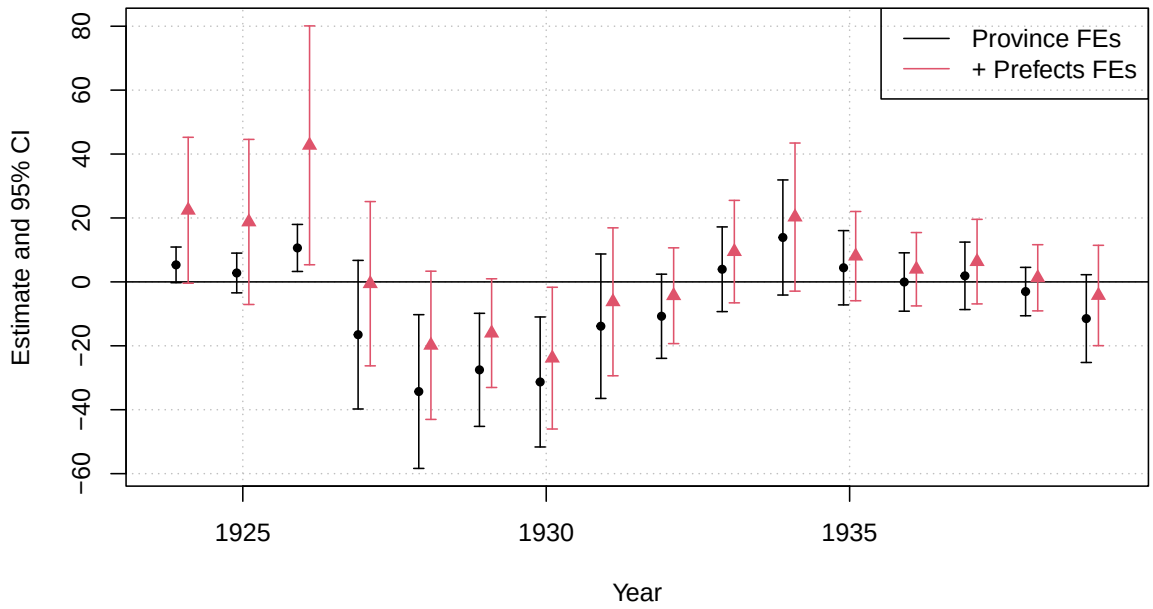
Embeddedness Bureaucrats' (downward) embeddedness might be leading to fewer records opened as well in two ways. In line with Bhavnani and Lee (2018)'s argument, prefects more local to the communities they surveil might be more knowledgeable about them - they might have more *métis* (Scott, 1998). This could allow them to surveil more efficiently, i.e. opening fewer, more targeted records. For example, Mattingly (2019) shows how embedded officials in China draw on social networks to gather information and exercise control. Alternatively, they might display lower levels of surveillance because they are more hesitant to surveil - or more easily corrupted by - a population they are closer to (Xu, 2021).

However, the lack of correlation between credentials and birthplace closeness shown in A13 seems to disprove that partisans are consistently allocated closer to where they were born in the first place, so embeddedness would not explain the pattern we observe. Plus, regressing surveillance records on birthplace distance (Table A14) displays no association between how close a prefect was born and the amount of surveillance he exercises on the community. In sum, this suggests embeddedness is not a key determinant of policy output in this context.

Deterrence Finally, particularly harsh crackdowns during the earlier years of fascist rule by partisans might have created a deterrent effect: the opposition might prefer to go underground rather than challenging the regime (Przeworski, 2015; Bramstedt, 2013; Choulis et al., 2024). This shift would also explain why we observe lower levels of realised surveillance under partisan prefects.

If the premise holds, early partisans might have initially opened more records and gained a reputation for tough enforcement, which then deterred opposition forces,

FIGURE 8. THE EFFECT OF *EARLY PARTISAN* IN TIME



Notes: The figure represents the year-by-year correlation between *Partisan* and *Records* with province fixed-effects (i.e. the model in Table 2, column (2) by year). Red coefficients also account for prefect fixed-effects.

leading to fewer records later on (as historically documented by Licht and Allen, 2018). While Figure 5 seems to offer evidence against this, a more precise estimation of the effect of having an early partisan in office is in order. Thus, we estimate equation 1 by year and plot the β_1 coefficients, adding prefect fixed effects in the most stringent specification.

The estimation partly supports deterrence building, albeit not conclusively. In the years from 1924 to 1926, early joiners seem to open more records relative to their counterparts. In subsequent years, they produce markedly less and by the mid-thirties, the difference fades. The patterns are broadly replicated when considering within-prefect variation due to prefect fixed-effects. Even if year-bound coefficients are sparsely significant, this dynamic calls for further investigation.

To do so, we single out the surveillance prefects have carried out during their first mandate in *First Records*$_{i,m}$, as a proxy for the establishment of a particularly harsh reputation. We then fit the model below:

$$\text{Records}_{i,m} = \beta_1 \text{Partisan}_{i,m} + \beta_2 \text{First Records}_{i,m} + \beta_3 \text{Partisan}_{i,m} \times \text{First Records}_{i,m} + \alpha_p + \gamma_m + \varepsilon_{i,m}$$

which interacts early surveillance with partisan. Should the interaction term β_3 be significant, it would indicate that prefects exercised lower surveillance also because they had built deterrence in the past. Moreover, β_1 losing its significance when interacted with early surveillance would constitute strong evidence that partisans surveil less only when they have built deterrence. However, results shown in Table A15 seem to suggest deterrence did not play a key role in lowering surveillance levels by loyalists. Higher early surveillance is correlated with higher surveillance overall - possibly due to the idiosyncratic characteristics of the prefect. Most importantly, having a partisan prefect retains its negative and significant impact, while the interaction term with early surveillance does not. In sum, while 8 suggests deterrence played a role in lowering equilibrium surveillance, a more detailed analysis seems to suggest it is a second-order determinant.

Other explanations Concluding, we address two further arguments we are not able to explore empirically due to the limited availability of historical data.

First, partisan prefects might be keeping the opposition in check by employing less orthodox, informal or extrajudicial methods. This is plausible, and it would square with their returns being higher than those of non-partisans, as they are being rewarded also for the activities they undertake that do not make it to the books. However, we would expect this to be somewhat captured by the embeddedness analysis, as it is reasonable to expect that prefects leveraging informal surveillance or repression would do so to a larger extent in locations they are better connected to. Unfortunately, due to the hidden nature of these activities, we are not able to inquire into them directly.

Secondly, as prefects managed a large portfolio of policies beyond political surveillance, they could influence society through other channels. It is then entirely possible that partisans fostered better socio-economic outcomes in the provinces they were entrusted with, and that this led to less dissent among the population, ultimately reducing surveillance requirements. While sufficiently granular and frequent economic data for

the period is, to the best of our knowledge, not available, we believe this is unlikely to be the case. Looking at Figure 6, the effect is concentrated in the first years of partisans' mandates, and thins out progressively. Instead, if what we observe is the result of good government, the effect would likely take some time to set in and then appear.

We believe the evidence we present points to competence, preferential deployment, and embeddedness not being relevant to the main result we observe in this context. However, it is possible that deterrence played a secondary role by lowering surveillance needs. With the necessary caveats about informal enforcement and good governance, we believe this section presents strong evidence in favour of signalling dynamics being a major driver of bureaucratic output.

7 Conclusion

What drives bureaucrats' willingness to implement the sensitive policies autocrats require? This paper builds on the literature spotlighting career concerns as a key factor, and underlines that they decisively depend on signalling alignment to regime leadership. Divisive and coercive policies then offer an ideal opportunity for agents lacking credentials to establish their trustworthiness. As a result, our model establishes that when alignment is adequately rewarded, day-one partisans might prove lighter-handed.

Utilising an original, individual-level dataset that links detailed biographical records of Italian prefects (1922-40) to province-level surveillance reports, we demonstrate that this intuition applied to political surveillance in Fascist Italy. Agents that joined the fascist party before it took power initiated approximately 18 to 25 per cent fewer surveillance files than their non-partisan colleagues. We highlight evidence consistent with a signalling-based mechanism: as our model predicts, partisans are rewarded more per surveillance record open, as they effortlessly reveal alignment in equilibrium. Moreover, non-partisans tend to round up the usual suspects to send a clearer signal of alignment (Luo and Zakharov, 2025).

This *prima facie* counterintuitive phenomenon speaks to two empirically elusive subjects. First, micro-level empirical evidence on top personnel decisions outside of the United States, Russia, or China is rare. Second, while the literature on surveillance focuses on digital surveillance and mostly on the characteristics of the surveilled, we

contribute an analysis of offline surveillance and shift the focus from the watched to the watchers.

Concluding, we point out some of the avenues for future research that our theoretical contribution and the dataset we constructed offer.

Dissecting the strategic choice of reward system for the ruler is beyond the scope of our model, but it is interesting to notice that, in the parameter space selected, the autocrat obtains more effort from agents (in exchange for fewer rewards) when rewarding alignment and effort in conjunction, not when focusing squarely on effort. However, this rewarding technology hints at an understudied trade-off between alignment and leverage in personnel decisions: partisan agents are certainly aligned, but they will exert less effort due to their credentials. Developing these intuitions could contribute to a more nuanced understanding of reputation and incentives within organisations.

Empirically, understanding whether alignment-signalling shapes effort in contemporary authoritarian settings remains an important avenue for further investigation.[24] Moreover, the core insight applies to democratic institutions with ideological turnover as well: new political appointees must decide whether to fill key roles with known co-partisans or take advantage of the increased effort of holdover staff trying to retain their positions.[25] This trade-off is especially salient where once anti-establishment parties now govern and must manage administrative bodies that may not share their programme. By mapping the incentives bureaucrats face and how these shape observable surveillance, this research agenda can help policymakers design staffing and oversight rules that strengthen democratic resilience.

[24]For a classic account emphasising ordinary bureaucratic compliance rather than ideological fervour, see (Arendt, 1963).

[25]For example, our dataset is suited to investigate prefects' performance under different democratic governments in the Italian Liberal Era.

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Appendix

A Model

A.1 Set-Up

A.1.1 Players and information

A principal interacts with a representative agent. The agent belongs to one of two *observable* categories:

1. **Partisan** i: type is known to be aligned, $\theta = H$.
2. **Non-partisan** j: type $\theta \in \{L, H\}$ is private information, with

$$\Pr(\theta = L \mid j) = \pi \in (0, 1), \qquad \Pr(\theta = H \mid j) = 1 - \pi.$$

Let $\mu \in [0, 1]$ denote the principal's posterior belief that the agent is aligned $(\theta = H)$.

A.1.2 Actions and timing

The agent chooses observable effort $x \in \mathbb{R}_+$. The timing is:

1. Nature draws the agent's observable category (i or j) and, if j, privately draws $\theta \in \{L, H\}$.
2. The agent chooses effort x.
3. The principal observes the category and x, forms posterior beliefs $\mu(x)$, and rewards the agent according to the reward technology.
4. Payoffs are realised, game ends.

A.1.3 Payoffs

The principal's reward technology is $P(x, \mu)$. The agent's cost is $C(x, \theta)$. The agent's utility is

$$U(x, \theta, \mu) = P(x, \mu) - C(x, \theta).$$

Reward. In the baseline case, rewards do not depend on inferred alignment:

$$P(x, \mu) = P(x) \quad \forall \mu,$$

where P is increasing, concave, and intersects the origin absent effort:

$$P_x(x) > 0, \quad P_{xx}(x) < 0, \quad P(0) = 0.$$

In the signalling case, rewards depend on inferred alignment and satisfy a single-crossing property in μ:

$$P_x(x, \mu) > P_x(x, \mu') \quad \forall x, \ \forall \mu > \mu'. \tag{SC-P}$$

P is increasing and concave with $P(0, \mu) = 0$ in this case as well, for all μ.

Cost. Costs are convex in effort and satisfy $C(0, \theta) = 0$ for both types. Marginal costs satisfy a single crossing condition in θ:

$$C_x(x, L) > C_x(x, H) \quad \forall x. \tag{SC-C}$$

Define the cost wedge

$$\Delta C(x) \equiv C(x, L) - C(x, H).$$

Then (SC-C) implies $\Delta C_x(x) > 0$ for all x, i.e. the wedge is increasing in effort.

A.1.4 Solution Assumptions

When indifferent, agents choose the lower-effort action.

In the signalling equilibrium constructed below, the principal's off-path beliefs for *non-partisans* are specified by a cutoff rule:

$$x < x_H^S \Rightarrow \mu = 0, \qquad x \geq x_H^S \Rightarrow \mu = 1,$$

where x_H^S is the equilibrium effort of aligned non-partisans.

A.2 Baseline case

In the baseline case, all agent types solve

$$\max_{x \geq 0} \; P(x) - C(x, \theta).$$

Let x_θ^B denote the (interior) solution characterised by the first-order condition

$$P_x(x_\theta^B) = C_x(x_\theta^B, \theta). \tag{B-FOC}$$

A.2.1 Equilibrium Structure

Proposition 1 (Baseline effort ordering)**.** *Under* (SC-C)*, equilibrium effort satisfies*

$$x_L^B < x_H^B, \qquad \textit{and since partisans are known to be } H, \quad x_i^B = x_H^B.$$

Implication: a partisan's and a non-partisan's expected effort are, respectively,

$$\mathbb{E}[x_j^B] = \pi x_L^B + (1-\pi) x_H^B,$$
$$\mathbb{E}[x_i^B] = x_i^B,$$

hence $\mathbb{E}[x_i^B] > \mathbb{E}[x_j^B]$ for all $\pi \in (0, 1]$.

Proposition 2 (Baseline returns to effort)**.** *Define the* return to effort *as reward per unit of equilibrium effort. For partisans and non-partisans respectively,*

$$R_i^B = \frac{P(x_i^B)}{x_i^B}, \qquad R_j^B = \pi \frac{P(x_L^B)}{x_L^B} + (1-\pi)\frac{P(x_H^B)}{x_H^B}.$$

Then $R_i^B < R_j^B$.

Proof. Since $x_i^B = x_H^B$, it suffices to show $P(x_L^B)/x_L^B > P(x_H^B)/x_H^B$. Concavity and $P(0) = 0$ imply that $x \mapsto P(x)/x$ is (weakly) decreasing on $\mathbb{R}_+$; with $x_L^B < x_H^B$ the inequality follows. □

A.3 Signalling case

In the signalling case, the reward depends on posterior beliefs. We construct a fully separating PBE in which agents' equilibrium effort truthfully reveals their private type. Partisans and aligned non-partisans are assigned $\mu = 1$, misaligned non-partisans are assigned $\mu = 0$, and the principal's off-path beliefs satisfy the cutoff rule stated above. We focus on the existence of the subcase where aligned non-partisans exert more effort than partisans (*interesting* separating equilibrium).

A.3.1 Candidate separating profile

Let x_i^S denote the partisan's effort on the high reward schedule, defined by

$$P_x(x_i^S, 1) = C_x(x_i^S, H). \tag{S-i}$$

Let x_L^S denote the misaligned non-partisan's effort on the low reward schedule, defined by

$$P_x(x_L^S, 0) = C_x(x_L^S, L). \tag{S-L}$$

Let x_H^S denote the aligned non-partisan's separating effort. In the "interesting" separating case, $x_H^S > x_i^S$ and the low type's incentive constraint binds:

$$P(x_H^S, 1) - C(x_H^S, L) = P(x_L^S, 0) - C(x_L^S, L), \tag{IC-L}$$

and x_H^S is taken to be the minimal effort satisfying (IC-L).

Finally, define x'_H as the aligned non-partisan's best response on the low schedule:

$$P_x(x'_H, 0) = C_x(x'_H, H). \tag{S-H'}$$

A.3.2 Incentive constraints

Misaligned non-partisans (no mimicking). Given the cutoff beliefs, any $x < x_H^S$ yields $\mu = 0$. The misaligned type optimally chooses x_L^S on the low schedule. To prevent profitable mimicking at x_H^S, it suffices that

$$P(x_H^S, 1) - C(x_H^S, L) \leq P(x_L^S, 0) - C(x_L^S, L),$$

which holds with equality in the interesting separating equilibrium (IC-L).

Aligned non-partisans (no downward deviation). Under cutoff beliefs, any $x < x_H^S$ yields $\mu = 0$. The most attractive such deviation is x'_H defined in (S-H′). Thus we impose

$$P(x_H^S, 1) - C(x_H^S, H) > P(x'_H, 0) - C(x'_H, H). \tag{IC-H}$$

Aligned non-partisans (no upward deviation). Since x_i^S solves (S-i), for $x > x_i^S$ one has $C_x(x, H) > P_x(x, 1)$. Hence, once $x_H^S > x_i^S$, further increases in effort strictly reduce the aligned type's payoff on the $\mu = 1$ schedule.

A.3.3 Separating Equilibrium Existence

Proposition 3 (Existence of an interesting separating equilibrium)**.** *Suppose the interesting case holds, i.e.*

$$P(x_i^S, 1) - C(x_i^S, L) > P(x_L^S, 0) - C(x_L^S, L),$$

so that separation requires $x_H^S > x_i^S$. Consider the candidate profile (x_i^S, x_L^S, x_H^S) with beliefs given by the cutoff rule. Then IC-L and IC-H are simultaneously satisfied.

Proof. Under the candidate profile, effort levels satisfy

$$x_L^S < x'_H < x_i^S < x_H^S,$$

where $x_L^S < x'_H$ follows from (SC-C) on the low schedule, and $x'_H < x_i^S$ follows from (SC-P) as players share the same cost type H.

Substitute (IC-L) into (IC-H). IC-H is implied by

$$P(x_L^S, 0) - C(x_L^S, L) + [C(x_H^S, L) - C(x_H^S, H)] > P(x'_H, 0) - C(x'_H, H).$$

Add and subtract $C(x'_H, L)$ on the right-hand side and rewrite as

$$\underbrace{P(x_L^S, 0) - C(x_L^S, L)}_{a} + \underbrace{\Delta C(x_H^S)}_{c} > \underbrace{P(x'_H, 0) - C(x'_H, L)}_{b} + \underbrace{\Delta C(x'_H)}_{d}.$$

By optimality of x_L^S for type L on the low schedule, $a > b$. Since $\Delta C(\cdot)$ is increasing and $x_H^S > x'_H$, we have $c > d$. Thus $a + c > b + d$, establishing IC-H. The remaining deviations

are ruled out by construction. □

A.3.4 Equilibrium Structure

Corollary 1 (Expected effort reversal)**.** *In the separating equilibrium,*

$$\mathbb{E}[x_i^S] = x_i^S, \qquad \mathbb{E}[x_j^S] = \pi x_L^S + (1-\pi)x_H^S.$$

Hence $\mathbb{E}[x_i^S] < \mathbb{E}[x_j^S]$ *whenever*

$$\pi < \bar{\pi} \equiv \frac{x_H^S - x_i^S}{x_H^S - x_L^S}.$$

Corollary 2 (Returns to effort in the signalling case)**.** *Define returns to effort as*

$$R_i^S = \frac{P(x_i^S, 1)}{x_i^S}, \qquad R_j^S = \pi \frac{P(x_L^S, 0)}{x_L^S} + (1-\pi)\frac{P(x_H^S, 1)}{x_H^S}.$$

Hence there exists $\hat{\pi} \in (0, 1]$ *s. t.* $R_i^S > R_j^S$ *whenever*

$$\pi < \hat{\pi} \equiv \frac{\frac{P(x_i^S,1)}{x_i^S} - \frac{P(x_H^S,1)}{x_H^S}}{\frac{P(x_L^S,0)}{x_L^S} - \frac{P(x_H^S,1)}{x_H^S}}.$$

Proof. Since $x_H^S > x_i^S$ and both lie on the $\mu = 1$ schedule, concavity (with the origin normalisation on that schedule) implies

$$\frac{P(x_H^S, 1)}{x_H^S} < \frac{P(x_i^S, 1)}{x_i^S}.$$

$\frac{P(x_L^S,0)}{x_L^S}$ cannot be ranked relative to the high-schedule returns without further structure, because it trades off a lower schedule (lower returns) against a lower effort level (higher returns).

- With
$$\frac{P(x_L^S, 0)}{x_L^S} \leq \frac{P(x_i^S, 1)}{x_i^S}.$$
then the inequality holds $\forall\ \pi$.

- With
$$\frac{P(x_L^S, 0)}{x_L^S} > \frac{P(x_i^S, 1)}{x_i^S}$$
the inequality holds if $\pi < \hat{\pi}$ as defined in Corollary 2.

□

A.4 Observable Empirical Implications

We now summarise the implications derived from the solution of the model, contrasting the baseline equilibrium and the interesting separating equilibrium in Table A1.

To reach these implications, we assume that observed policy output y is a noisy but unbiased signal of the agent's underlying effort choice x. In particular, policy output is generated according to

$$y = g(x) + \varepsilon,$$

where $g(\cdot)$ is a strictly increasing function and ε is a mean-zero disturbance term conditional on x, which captures idiosyncratic factors. Under this assumption, expected policy output is strictly increasing in effort, which allows us to use rankings of average output to rank underlying effort.

Table A1. Equilibrium Implications

	Baseline Equilibrium	*Interesting Separating Equilibrium*
Expected Effort	$\mathbb{E}[x_i^B] > \mathbb{E}[x_j^B]$	$\mathbb{E}[x_i^S] < \mathbb{E}[x_j^S]$ if $\pi < \bar{\pi}$
Returns to Effort	$R_i^B < R_j^B$	$R_i^S > R_j^S$ if $\pi < \hat{\pi}$

Therefore, an empirical pattern of *lower effort among partisans* together with a *higher returns to effort for partisans relative to non-partisans* is incompatible with the baseline environment and is consistent with a signalling interpretation in a setting where few misaligned non-partisans are present.

A.5 Model Specification for Figures

To produce the figures that visualise the equilibria in the main text, we selected the following specific functional forms and parameter values, in compliance with the generic requirements outlined in the setup.

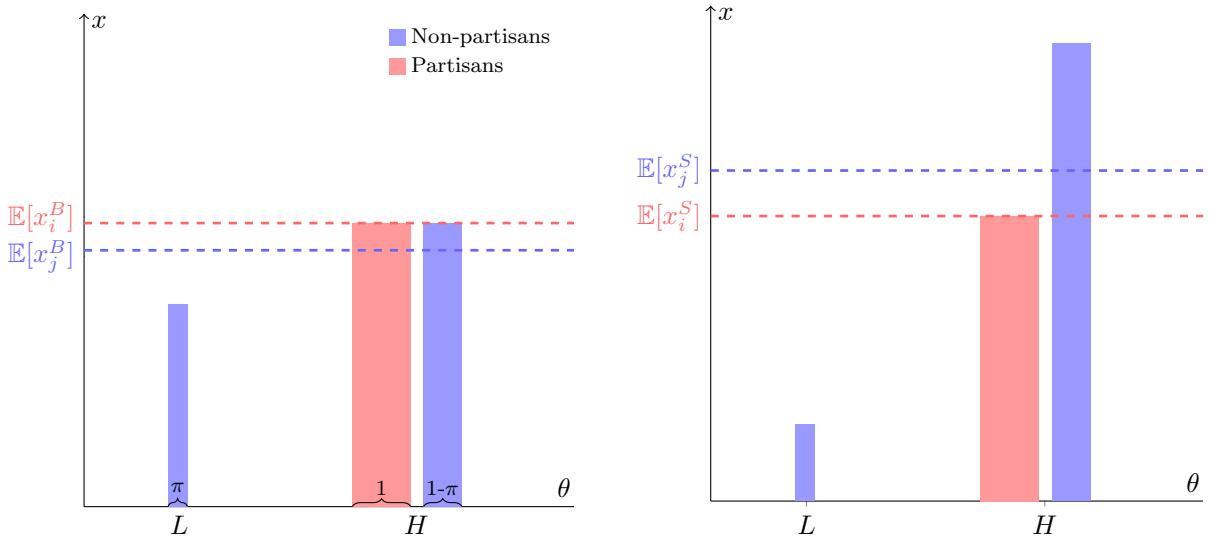
TABLE A2. FUNCTIONAL FORMS AND PARAMETER VALUES IN VISUALISATIONS

Panel A: Functions			
Description	*Generic form*	*Specific form*	
Cost function	$C(x, \theta)$	$C(x, \theta) = \frac{\kappa}{2}x^2$	
Payoff function	$P(x, \mu)$	$P(x, \mu) = \frac{\sqrt{x}}{\lambda}$	
Panel B: Parameters			
Description	*Parameter*	*Value*	
Misaligned types prevalence	$\pi \in (0, 1)$	$\pi = \frac{1}{3}$	
Cost curvature	$\kappa \in (0, 1]$	$\kappa = \begin{cases} \frac{3}{4} & \text{if } \theta = H \\ 1 & \text{if } \theta = L \end{cases}$	
		Baseline case	*Signalling case*
Alignment premium	$\lambda \in [1, \infty)$	$\lambda = 1$	$\lambda = \begin{cases} 1 & \text{if } \mu = 1 \\ \frac{9}{5} & \text{if } \mu = 0 \end{cases}$

Notes. Both functions are increasing in effort and pass through the origin. $P(x, \mu)$ is concave in effort while $C(x, \theta)$ is convex.

Figure 1 represents the two equilibria. Figures A1 and A2 instead depict the intuitions underpinning Implications 1 and 2, respectively.

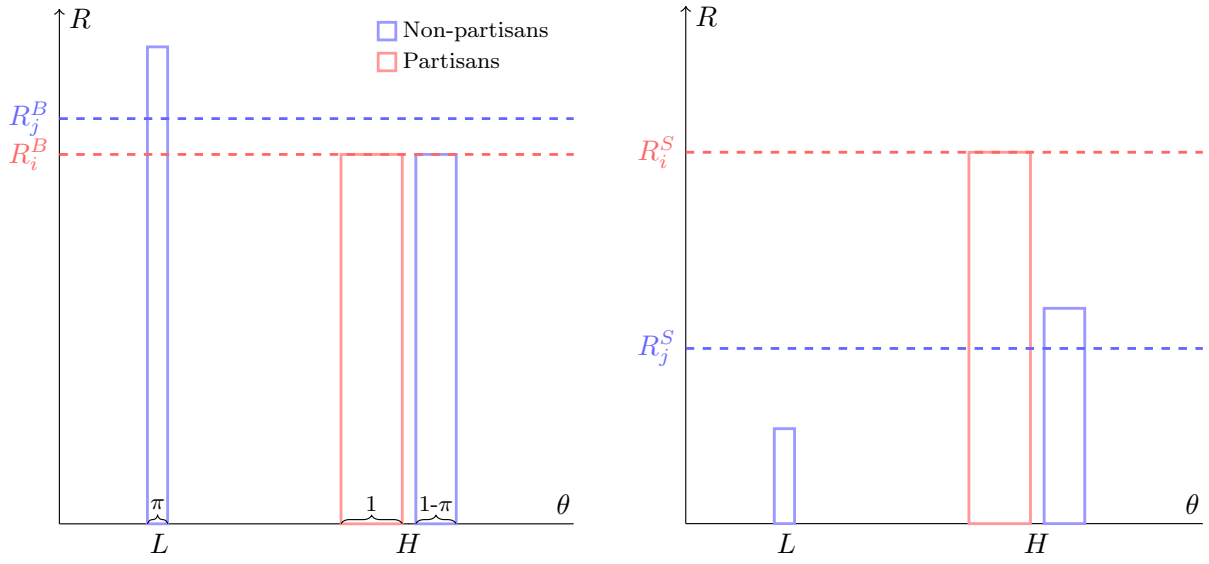
FIGURE A1. IMPLICATION OF SIGNALLING ON EXPECTED EQUILIBRIUM EFFORT



(A) BASELINE EQUILIBRIUM (B) INTERESTING SEPARATING EQUILIBRIUM

Notes. Functional forms and parameter values for the illustrations are listed in Table A2. The height of the bars represents equilibrium effort by the relevant agent. The width of the bars reflects the prevalence of the type within the observable category. The dashed lines mark the weighted average of the values based on the ex-ante probability π that a non-partisan agent is a high type. Observing less effort by partisans is not compatible with the baseline equilibrium.

FIGURE A2. IMPLICATION OF SIGNALLING ON EQUILIBRIUM RETURNS TO EFFORT



(A) BASELINE EQUILIBRIUM (B) INTERESTING SEPARATING EQUILIBRIUM

Notes. Functional forms and parameter values for the illustrations are listed in Table A2. The height of the bars represents equilibrium returns to effort (payoff per unit of effort) for the relevant agent. The width of the bars reflects the prevalence of the type within the observable category. The dashed lines mark the weighted average of the values. Observing higher equilibrium returns to effort for non-partisans is not compatible with the baseline equilibrium.

B Descriptives

Table A3. Descriptive Statistics for Key Variables. Prefect dataset.

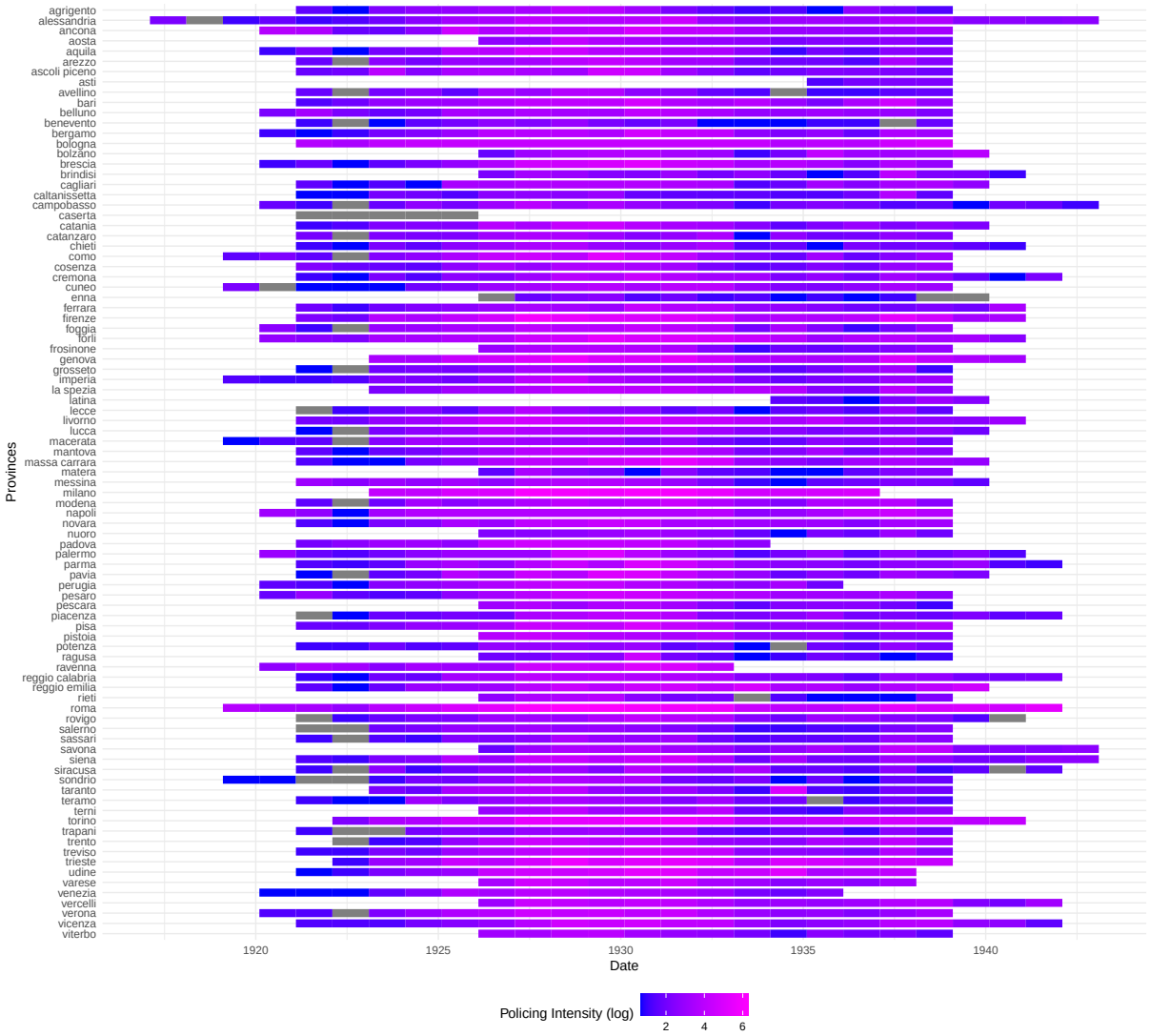
Variable	Unique	Missing Pct.	Mean	SD	Min	Median	Max
Partisan (Voluntary PNF Member)	19	1	0.2	0.4	0.0	0.0	1.0
Early Partisan (Pre-March on Rome)	17	0	0.2	0.4	0.0	0.0	1.0
Late Partisan	13	1	0.0	0.1	0.0	0.0	1.0
Career Bureaucrat	24	0	0.7	0.4	0.0	1.0	1.0
Mandate number	12	0	3.0	2.0	1.0	3.0	12.0
Experience as Prefect (years)	552	0	4.5	3.4	0.0	3.8	20.0
log(Opened Records)	186	3	2.9	1.1	0.7	2.9	6.3
Opened records	177	3	33.2	52.1	1.0	17.0	548.0

Table A4. Balance Table for Key Variables. Prefect dataset.

	Non-partisan (N=320)		Partisan (N=90)			
Variable	Mean	SD	Mean	SD	Diff.	p-value
Age at start	52.8	5.8	45.2	7.0	−7.6	< 0.001
Birth year	1876.6	9.0	1892.3	7.4	15.7	< 0.001
University	1.0	0.2	0.8	0.4	−0.2	< 0.001
Mandate length (days)	650.4	536.5	653.9	582.3	3.5	0.960
Anormal length (de jure)	0.4	0.5	0.4	0.5	0.1	0.253
Anormal length (de facto)	0.0	0.2	0.0	0.1	0.0	0.241
Education	N	Pct.	N	Pct.		
Diploma	5	1.6	9	10.0		
License	2	0.6	4	4.4		
Other	4	1.2	9	10.0		
University Degree	309	96.6	68	75.6		

Note: Differences are based on means or proportions. p-values from two-sample t-tests.

FIGURE A3. REPRESSION OVER TIME BY PROVINCE.



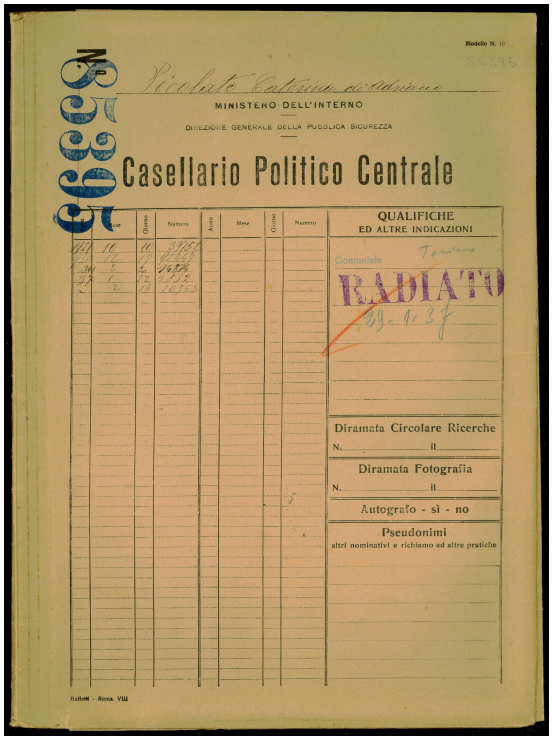
C The Casellario Politico Centrale

The dataset has a row for each individual with a folder in the CPC. For example, Table A6 presents an example of the variables of interest for Caterina Picolato.[26]. As it can be seen in Table A6, in the dataset we do have information on when surveillance started and ended, as well as some notes on the measures taken against her (see Figure A4).

Variable	**Value**
DENOMINAZIONE	Picolato Caterina
RESIDENZA	Torino, Piemonte, Italia
UNITDATE	1923-1937
DATAINIZIO	1923
DATAFINE	1937
BUSTA	3951
FASCICOLO	B47025
MESTIERE	impiegata
ANNOTAZIONI	radiato
MATRICOLA	NULL
NOTEDOCARC	NULL
NOTA ARCHIVISTICA	NULL

Table A6. Example of variables in the CPC dataset.

[26]Available at https://tecadigitaleacs.cultura.gov.it/item/835585d4-a5e5-4c7a-ac28-05c2d9721160.


MINISTERO DELL'INTERNO

DIREZIONE GENERALE DELLA PUBBLICA SICUREZZA

Casellario Politico Centrale

QUALIFICHE ED ALTRE INDICAZIONI

RADIATO

Diramata Circolare Ricerche

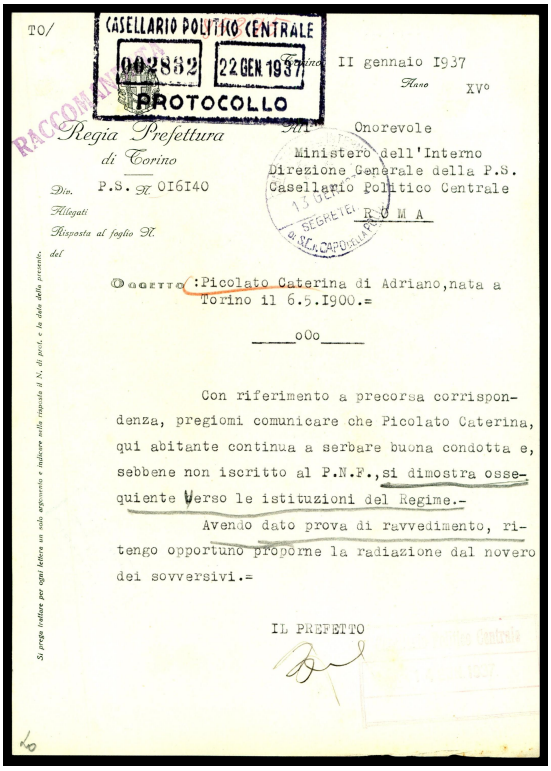
Diramata Fotografia

Autografo - sì - no

Pseudonimi

altri nominativi e richiamo ad altre pratiche

FIGURE A4. COVER OF CATERINA PICOLATO'S REGISTRY AT THE CPC.


CASELLARIO POLITICO CENTRALE

002852 22 GEN 1937

PROTOCOLLO

Regia Prefettura di Torino

Div. P.S. N. 0I6I40

Allegati

Risposta al foglio N. del

Torino II gennaio I937 Anno XV°

Onorevole Ministero dell'Interno Direzione Generale della P.S. Casellario Politico Centrale ROMA

Oggetto: Picolato Caterina di Adriano, nata a Torino il 6.5.I900.=

_____oOo_____

Con riferimento a precorsa corrispondenza, pregiomi comunicare che Picolato Caterina, qui abitante continua a serbare buona condotta e, sebbene non iscritto al P.N.F., si dimostra ossequiente verso le istituzioni del Regime.-

Avendo dato prova di ravvedimento, ritengo opportuno proporne la radiazione dal novero dei sovversivi.=

IL PREFETTO

FIGURE A5. LETTER FROM THE PREFECT OF TORINO, ASKING FOR CATERINA PICOLATO'S EXCLUSION ("RADIAZIONE") OF SURVEILLANCE AT THE CPC.

D Robustness Checks

D.1 Avoiding Fractional Credential Values

TABLE A7. FULL VALUES ONLY

Dependent Variable:	Number of Records Opened			
Model:	(1)	(2)	(3)	(4)
Variables				
Partisan	-4.741*		-5.356**	
	(2.535)		(2.678)	
Early Partisan		-6.063**		-6.180**
		(2.716)		(2.849)
Late Partisan		4.849		1.401
		(6.359)		(6.585)
Restricted Sample			✓	✓
Fixed-effects				
Province	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Dependent variable mean	30.877	30.944	31.367	31.450
Observations	1,611	1,597	1,382	1,368
Adjusted R^2	0.610	0.611	0.613	0.613
F-test	6.034	5.843	6.230	6.021

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A8. FILLED VALUES WITH CREDENTIAL CHANGE DUMMY

Dependent Variable:	Number of Records Opened			
Model:	(1)	(2)	(3)	(4)
Variables				
Credential Change	-1.536	-1.329	-4.862	-6.246
	(2.833)	(2.797)	(6.487)	(6.370)
Partisan	-4.861*		-5.236*	
	(2.504)		(2.680)	
Early Partisan		-6.356**		-6.286**
		(2.619)		(2.796)
Late Partisan		2.012		-0.270
		(5.955)		(5.521)
Restricted Sample			✓	✓
Fixed-effects				
Province	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Dependent variable mean	30.964	30.964	31.766	31.766
Observations	1,828	1,828	1,412	1,412
Adjusted R^2	0.599	0.599	0.619	0.619
F-test	5.474	5.299	6.122	5.916

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

D.2 Turnover versus Credentials

We have shown that voluntary party membership - and especially early joining - predicts a substantial drop in police-registry openings. A natural question is whether this effect simply reflects the arrival of any new prefect, rather than the difference in credentials. To test this, in Table A9 we replace our partisan indicator with a dummy for "any prefect change" from year $t-1$ to t.

We re-estimate equation (1) but with $\Delta_{p,t} = \mathbf{1}\{\text{prefect at } (p,t) \neq \text{prefect at } (p,t-1)\}$ as an additional control.

Table A9 reruns the core specification shown in 2. Table A10 avoids fractional treatment values and marks as treated any year where a partisan was present, regardless of the number of months.

In both tables, columns 1 and 2 use the full sample; columns 3 and 4 restrict to provinces where prefects have explicit career-admin and PNF information. All models include province and year fixed-effects, with standard errors clustered by province.

Across every specification, the coefficient on "any prefect change" is essentially zero and never statistically significant. Whether we look at the full sample or the restricted subsample, mere turnover without a change in credentials bears no systematic relationship with policing intensity.

These null findings confirm that it is not the replacement of a prefect per se that drives our main results. Rather, the reductions in police-registry openings documented in Table 2 arise specifically when a *fascist* (PNF) prefect - particularly an early joiner - assumes office.

TABLE A9. MERE TURNOVER DOES NOT DRIVE THE RESULT

Dependent Variable:	Number of Records Opened			
Model:	(1)	(2)	(3)	(4)
Variables				
Any Change	-1.632	-1.618	-0.759	-0.809
	(1.553)	(1.546)	(2.202)	(2.206)
Partisan	-5.712**		-5.532**	
	(2.493)		(2.683)	
Early Partisan		-6.939**		-6.364**
		(2.657)		(2.856)
Late Partisan		2.009		-0.160
		(6.394)		(6.559)
Restricted Sample			✓	✓
Fixed-effects				
Province	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Dependent variable mean	31.057	31.057	31.766	31.766
Observations	1,794	1,794	1,412	1,412
Adjusted R^2	0.613	0.614	0.618	0.618
F-test	5.815	5.622	6.114	5.903

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A10. MERE TURNOVER DOES NOT DRIVE THE RESULT (FILLED VALUES)

Dependent Variable:	Number of Records Opened			
Model:	(1)	(2)	(3)	(4)
Variables				
Any Change	-0.519	-0.408	-0.542	-0.523
	(1.484)	(1.489)	(2.198)	(2.216)
Partisan	-5.332**		-5.743**	
	(2.248)		(2.640)	
Early Partisan		-6.740***		-6.705**
		(2.438)		(2.784)
Late Partisan		1.688		-1.532
		(5.894)		(5.483)
Restricted Sample			✓	✓
Fixed-effects				
Province	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Dependent variable mean	30.964	30.964	31.766	31.766
Observations	1,828	1,828	1,412	1,412
Adjusted R^2	0.599	0.599	0.619	0.619
F-test	5.473	5.298	6.119	5.910

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

E Mechanisms

E.1 Competence

TABLE A11. EARLY FASCISTS POLICE LESS CONTROLLING FOR COMPETENCE PROXIES

Dependent Variable:	Records Opened							
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Has a Degree	4.985			4.346	-2.120			-1.963
	(4.275)			(4.128)	(3.452)			(3.466)
Years of Experience		-0.381		-1.332		-0.538		-1.288
		(0.627)		(1.277)		(0.605)		(1.341)
Mandate Number			1.005	2.456			0.583	2.020
			(0.931)	(2.089)			(0.769)	(2.100)
Early Partisan					-7.291**	-6.355**	-6.865**	-6.154**
					(2.791)	(2.665)	(2.638)	(2.930)
Late Partisan					2.006	1.783	2.515	2.152
					(6.457)	(6.391)	(6.475)	(6.455)
Fixed-effects								
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Dependent variable mean	30.964	30.964	30.964	30.964	31.057	31.057	31.057	31.057
Observations	1,828	1,828	1,828	1,828	1,794	1,794	1,794	1,794
Adjusted R^2	0.598	0.598	0.598	0.602	0.613	0.614	0.614	0.616
F-test	5.660	5.654	5.668	5.347	5.619	5.637	5.625	5.317

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A12. PARTISANS POLICE LESS CONTROLLING FOR COMPETENCE PROXIES

	Number of Police Records Opened			
	(1)	(2)	(3)	(4)
Partisan	-5.987**	-5.236**	-5.597**	-4.969*
	(2.657)	(2.471)	(2.470)	(2.722)
Has a Degree	-1.809			-1.663
	(3.424)			(3.453)
Years of Experience		-0.575		-1.328
		(0.596)		(1.333)
Current Mandate Number			0.539	2.023
			(0.755)	(2.095)
Dependent variable mean	31.057	31.057	31.057	31.057
Observations	1,794	1,794	1,794	1,794
Adjusted R^2	0.613	0.614	0.613	0.616
F-test	5.811	5.833	5.816	5.488
Province fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

, **, and * correspond to 10, 5, and 1% levels of significance, respectively.*
Standard errors are clustered at the province level.

E.2 Preferential Deployment

We fit the simple following model:

$$\text{Desirability}_{i,m} = \beta_1 \text{Partisan}_{i,m} + \alpha_p + \gamma_m + \varepsilon_{i,m} \tag{3}$$

where $Desirability_{i,m}$ is alternatively measured in the negative distance of the province of the current mandate to the birthplace of the prefect, or in province prestige score. α_p and γ_m are province and mandate fixed-effects. Finally, $\varepsilon_{i,m}$ represents the error. We always include γ_m to compare prefects at the same stage of the career progression (results are unaffected if the term is omitted).

Table A13. Partisans Are Not Consistently Deployed to Better Places

Dependent Variables:	Birthplace Closeness (km)						Prestige Score	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Partisan	30.415		17.950	14.006			0.005	
	(23.659)		(22.068)	(23.416)			(0.006)	
Early Partisan		24.200			16.620	12.933		0.001
		(25.924)			(24.546)	(25.834)		(0.006)
Late Partisan		72.539*			26.324	20.802		0.032***
		(38.355)			(38.702)	(37.602)		(0.007)
Fixed-effects								
Province			Yes	Yes	Yes	Yes		
Mandate number				Yes		Yes	Yes	Yes
Fit statistics								
Dependent variable mean	-433.264	-433.264	-433.264	-433.264	-433.264	-433.264	0.158	0.158
Observations	788	788	788	788	788	788	800	800
Adjusted R^2	0.001	0.000	0.049	0.047	0.047	0.046	0.111	0.111
F-test	1.566	1.052	18.037	1.634	9.020	1.509	1.565	0.791

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

E.3 Embeddedness

Table A14 suggests embeddedness, to the extent it is proxied having been born closer to the province of appointment, is not a consequential factor in determining surveillance levels. Columns 3 to 6 feature interaction terms also allowing for closeness to be differently leveraged by fascist prefects.

TABLE A14. MORE LOCAL PREFECTS DO NOT SURVEIL LESS

Dependent Variable:	Records Opened					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Birthplace Distance	-0.003	-0.004	-0.004	-0.005	-0.004	-0.005
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Birthplace Distance × Partisan			0.012	0.012		
			(0.009)	(0.009)		
Birthplace Distance × Early Partisan					0.013	0.012
					(0.009)	(0.009)
Fixed-effects						
Province	Yes	Yes	Yes	Yes	Yes	Yes
Mandate Number		Yes		Yes		Yes
Fit statistics						
Dependent variable mean	30.201	30.201	30.201	30.201	30.201	30.201
Observations	751	751	751	751	751	751
Adjusted R^2	0.485	0.486	0.484	0.486	0.483	0.485
F-test	105.705	9.144	35.344	7.861	26.522	7.340

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

E.4 Deterrence

TABLE A15. EARLY SURVEILLANCE DOES NOT SEEM TO BUILD DETERRENCE

Dependent Variable:	Number of Records Opened							
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Early Surveillance	0.369***	0.371***	0.375***	0.384***	0.362***	0.384***	0.362***	0.391***
	(0.107)	(0.103)	(0.111)	(0.100)	(0.117)	(0.105)	(0.119)	(0.099)
Partisan	-9.329***	-8.899**			-8.978***	-8.064**		
	(3.179)	(3.504)			(3.103)	(3.509)		
Early Surveillance × Partisan	0.091	0.163			0.050	0.116		
	(0.213)	(0.198)			(0.201)	(0.186)		
Early Partisan			-9.443***	-8.966**			-9.209***	-8.229**
			(3.456)	(3.873)			(3.341)	(3.857)
Late Partisan			-7.276	-5.345			-7.949	-5.666
			(6.025)	(5.619)			(5.907)	(5.252)
Early Surveillance × Early Partisan			0.074	0.104			0.078	0.095
			(0.245)	(0.249)			(0.246)	(0.254)
Restricted Sample					✓	✓	✓	✓
Fixed-effects								
Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mandate Number		Yes		Yes		Yes		Yes
Fit statistics								
Dependent variable mean	32.599	32.599	32.599	32.599	32.401	32.401	32.401	32.401
Observations	567	567	567	567	506	506	506	506
Adjusted R^2	0.570	0.579	0.569	0.578	0.569	0.583	0.568	0.582
F-test	52.757	13.000	39.551	12.048	54.588	13.729	40.951	12.738

Clustered (Province) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*