A Cursed Inheritance: Autocratic Bureaucrats and Democratic Breakdown in Weimar Germany

Moritz Emanuel Bondeli*

June 19, 2025

Abstract

New democracies often inherit state institutions and key bureaucratic personnel from past autocratic regimes. How does this legacy influence democratic development? This paper studies the effect of personnel replacement among police leadership in the state of Prussia on political life in the Weimar Republic, drawing on original career data on police officers as well as electoral records. Exploiting variation in the timing of replacement in a staggered difference-in-differences design, I show that sidelining bureaucrats associated with the autocratic regime reduced mobilization effort and electoral success of anti-democratic actors, and lowered the risk of political violence. Qualitative evidence details how autocratic police leaders leveraged their authority to facilitate violent, anti-democratic mobilization. My paper highlights the importance of bureaucratic reform in shaping trajectories of democratization.

*Postdoctoral Research Fellow, Institute of the Social Sciences, Humboldt Universität zu Berlin, moritz.bondeli@hu-berlin.de. Website: https://mbondeli.github.io. This project was made possible by grants from the Harry Frank Guggenheim Foundation, the American Political Science Association, and the Yale MacMillan Center. The statements made and views expressed are solely the responsibility of the author. I am grateful to Isabela Mares, Steven Wilkinson, Fredrik Sävje, Sarah Khan, Mashail Malik, Gemma Dipoppa, Dawn Teele, Tara Slough, Guy Grossman, Rafaela Dancygier, Carles Boix, John Huber, David Stasavage, Didac Queralt, Haley Allen DeMarco, Deepika Padmanabhan, Sofia Elverdin, Alexander Trubowitz, and audiences at APSA 2024 in Philadelphia, NEWEPS-23 at Columbia University, the Northeast Regional Workshop in European Politics at Yale, Tecnologico de Monterrey, and the Yale Comparative Politics Workshop, for excellent feedback. I am especially grateful to Viktoryia Karanikha for excellent research assistance. Remaining errors are my own.

"That the German working class movement, disoriented during the war, should have taken over the old state apparatus practically unchanged, was its grave historical error."
– Exil-SPD in 1934, cited in Gay, *Weimar Culture*, 1968, p. 22.

1 Introduction

What is the effect of inheriting autocratic bureaucrats on democratic consolidation in nascent democracies? On the one hand, new democracies that inherit state capacity should be better positioned to project authority across their territory, enforce laws, and provide vital public goods, aiding democratic consolidation. On the other hand, bureaucrats appointed and socialized under autocratic rule can use the authority and resources at their disposal to undermine democratic consolidation in an effort to benefit their former principals.

This paper examines how autocratic bureaucrats affected democratic life in a paradigmatic case of democratic breakdown: the Weimar Republic. The analysis focuses on *Landräte*, or appointed heads of law enforcement in rural counties, in the state of Prussia. As the largest and most populous state in Weimar Germany, developments in Prussia were decisive for the entirety of Weimar Germany. The 1918 transition to democracy did not prompt a dramatic personnel reshuffle among the ranks of law enforcement officials. Even years into the Interwar period, a significant share of bureaucrats were part of the autocratic elite, either appointed prior to democratic transition or members of conservative German nobility.

I theorize that the enduring presence of autocratic law enforcement officials across the Prussian territory had a detrimental effect on democratic life. Faced with violent anti-democratic mobilization from the far left and the far right, law enforcement officials played a central role in maintaining public order while safeguarding political liberties. Due to the complexity of this task, law enforcement officials enjoyed significant discretion in the exercise of their authority. Autocratic bureaucrats who were hostile to democracy used their authority to lower mobilization costs of anti-democratic political actors, giving rise to small authoritarian enclaves across the Prussian territory. Hence, I expect that anti-democratic mobilization and the associated risk of violent clashes were highest in counties administered by autocratic law enforcement officials.

I test my theory using county-level panel data from Prussia that covers most of the democratic Weimar years. The panel combines original career data on law enforcement officials and electoral records on candidate entry with secondary data on voting and incidents of lethal political violence. I exploit variation in the timing of personnel reshuffles to identify the causal effect of removing an autocratic law enforcement official on democratic life in a staggered difference-in-differences setup.

I find that the replacement of autocratic law enforcement officials indeed reduced anti-democratic mobilization. Focusing on candidate entry as a proxy for party mobilization effort,

I find that replacement on average reduces the number of Nazi candidates running for office. Lower mobilization effort translates to diminished electoral success. On average, replacement of an autocratic law enforcement official reduces the vote share of the anti-democratic NSDAP by 2 percentage points, while increasing the combined vote share of pro-democratic parties by more than 3 percentage points. Moreover, counties where autocratic law enforcement officials are replaced also see a lower incidence of lethal political violence. Additional event-study evidence shows that the democracy-stabilizing effects of removing autocratic law enforcement officials increase over time, suggesting that the full effects of personnel reshuffles take years to manifest. I complement my quantitative analysis with qualitative evidence that describes how autocratic law enforcement officials turned a blind eye on, or even actively enabled, anti-democratic mobilization efforts.

Historians and legal scholars have long argued that the continued influence of autocratic bureaucrats hampered democratic consolidation in Weimar Germany (Loewenstein 1937; Pikart 1958; Jasper 1963; Runge 1965; Muncy 1970; McElligott 2013). Due to a lack of data, the claim has not been subject to quantitative evaluation. My paper is the first to examine how autocratic bureaucrats affected democratic consolidation in Weimar Germany using modern econometric tools. In doing so, I provide a fresh direction for a large body of work examining the causes of democratic breakdown in Interwar Germany (Gerschenkron 1946; Moore 1966; Luebbert 1987; Ziblatt 2017; Weyland 2021). Recent quantitative work has emphasized external and structural factors such as century-old legacies of antisemitism (Voigtländer and Voth 2012), nationalism induced by World War I (De Juan et al. 2024) or the lack of economic and fiscal policy autonomy as a consequence of the Versailles Treaty (Galofré-Vilà et al. 2021; Doerr et al. 2022) as important causes of democratic breakdown. My work shows that not all determinants of democratic breakdown were external, structural, and beyond the control of German policymakers. More decisive and swift reforms of the administrative apparatus could have contributed to stabilizing democracy.

My findings have implications beyond the case of Weimar Germany. They add nuance to scholarship that extols the virtues of independent bureaucracies insulated against political interference as catalysts of political and economic development (Geddes 1996; Evans and Rauch 1999; Dahlström, Lapuente, and Teorell 2012; Cornell and Lapuente 2014; Dahlström and Lapuente 2017). In Weimar Germany, rules and norms that limited the influence of elected officials over administrative appointments cemented the stranglehold of affluent landed elites and the German nobility over the Weimar bureaucratic apparatus, with negative consequences for democratic stability. Purges, in particular of law enforcement officials, can set democracies on a solid path towards consolidation (Nalepa 2022; Flom 2024; Nalepa and Piotrowska 2024).

Second, the paper shows that identities and political preferences of law enforcement officials matter for explaining variation in the incidence of electoral violence (Birch, Daxecker, and Höglund 2020; Birch 2020). To understand why and where electoral violence occurs, asking whether state institutions have the *capacity* to prevent violence, and how the capacity varies across time and space, may not be sufficient (De Juan and Pierskalla 2015; Wig and Tollefsen

2016; Müller-Crepon, Hunziker, and Cederman 2021). Even when state institutions are capable, violence might still occur as a result of variation in preferences of lower-level law enforcement actors to whom policing is delegated.

Third, I contribute to a long-standing debate between *state-first* and *democracy-first* arguments regarding the optimal sequence of political development (Mazzuca and Munck 2014; Andersen and Doucette 2022). Proponents of the *state-first* perspective argue that where democracies are not built on strong administrative foundations, they are likely to perform poorly at producing public goods (D'Arcy and Nistotskaya 2017; Linz and Stepan 1996, 17). Clientelism, vote-buying, and patronage become endemic, preventing the development of programmatic politics (Shefter 1977; Mizuno and Okazawa 2025). *Democracy-first* arguments, on the other hand, posit that state-building under autocracy makes democratization less likely, given that autocrats can and often do use state capacity to weaken any opposition to their rule (Way 2005; Seeberg 2021). Extending democracy-first arguments, my paper demonstrates that even where autocratic state building cannot prevent *transition* to democracy, legacies of autocratic state building can constitute a serious impediment to later democratic *consolidation*.

In addition to these substantive contributions, the data described in this paper are likely of great interest to scholars of German political development, political violence and comparative politics. New data on bureaucratic appointments and party mobilization efforts allow for rigorous analysis of the logics of democratization and democratic erosion at subnational and local levels, generating exciting opportunities for new micro-historical research (Mares 2015, ch. 10).

The paper proceeds as follows. I first provide information on the historical context of the study. I then develop theoretical expectations regarding the effects of sidelining autocratic bureaucrats on democratic life. Subsequently, I introduce the original quantitative data used in the paper. I then present and discuss my results. The final section concludes.

2 Context: German Democracy in the Interwar Period

Two facets of German Interwar history are important to contextualize the argument made in this paper. First, the pre-democratic administrative apparatus remained largely intact during the revolutionary turmoil following World War I, both institutionally and in terms of personnel. This gave rise to tensions between newly diverse *political* elites, which included citizens of middle-class and working-class backgrounds, Jewish Germans, and women, and *administrative* elites, whose ranks were made up almost entirely of aristocratic, Protestant men. Second, democratic life in the Weimar Republic unfolded under the shadow of intense violence perpetrated by monarchist, Stalinist and Nazi forces in pursuit of their political objectives.

2.1 Political Change, Administrative Persistence

The November Revolution of 1918 that followed the German Empire's defeat in World War I marked a decisive departure from the pre-democratic status quo. Relative to other Western

Interwar democracies, the Weimar constitution launched an exceptionally ambitious democratic project that fully enfranchised women, created vigorous multiparty competition through proportional representation, and even made far-reaching provisions for popular participation in policy-making through referenda (Rux 2002). This expansion of formal democratic rights stands in contrast to the persistence of pre-democratic state institutions, including the German Army (*Reichswehr*), law enforcement, the judiciary, and educational institutions. Against opposition from parts of the left, the Weimar constitution, as well as the constitutions of the states, cemented the status quo ante of an insulated administrative apparatus staffed by career civil servants with advanced degrees, most of whom were insulated from democratic accountability (Eschenburg 1954; Jasper 1963, 211).

Administrative continuity was reflected not just in the legal framework governing the civil service, but equally in the social composition of administrative elites. While many democratic reformers recognized the necessity of "democratizing" the state apparatus, their ability to do so was limited for two reasons. First, in its early years, Weimar democracy was threatened by communist insurgencies like the Bavarian Soviet Republic or the Spartacist uprising (Jones 2016), prompting Social Democratic leaders to cooperate with the *Reichswehr* and law enforcement in an effort to prevent a communist takeover.[1] The necessity to stave off the communist threat precluded more decisive action to democratize the military and law enforcement in the early years of the Weimar Republic (Jasper 1963, ch. 8; Runge 1965, 16–21).

Second, leading administrative positions were typically staffed by graduates of law schools who had to complete onerous civil service examinations. Access to education and credentials was highly socially stratified, with significant obstacles to entry for individuals from middle- and working-class backgrounds, women, and Jewish Germans (Wunder 1977; Runge 1965, 179–200).[2] While some reforms were made to formally open access to leading administrative posts to "outsiders", the supply of qualified candidates to fill such positions was limited, placing sharp constraints on democratic reformers' ability to quickly change the social composition of bureaucratic personnel (Runge 1965, 44–51).

The absence of meaningful reform of the administrative apparatus proved to be an impediment to democratic consolidation. In 1920, parts of the German Army led by monarchist officers marched on and briefly occupied parts of the capital city of Berlin in an effort to overthrow the democratically elected government. The event, which became known as the *Kapp Putsch*, was defeated by a general strike called by trade unions and democratic reformers, and prompted the removal of several members of the Prussian administration that supported the coup (Runge 1965, 121–134). The events of 1920 made clear that the nascent democratic order could not count on much loyalty within the armed forces and law enforcement (Erger 1967).

1. The alliance between governing Social Democrats, led by Friedrich Ebert, and the Reichswehr, led by General Wilhelm Groener, is sometimes referred to as the Ebert-Groener-Pakt. For the role of the Reichswehr more generally, see Carsten (1964).

2. For example, Walther Rathenau – whose work as foreign minister in the early years of the Republic left a lasting positive impact on German relationships with France, the U.K. and the U.S. – was initially barred from pursuing a career in the diplomatic service because of his Jewish faith (Volkov 2012).

Even after the Putsch, the legal insulation of much of the civil service and judiciary continued to place hard constraints on the democratization of the administrative apparatus. From 1921 to 1922, a wave of assassination plots against leading politicians, culminating in the murders of finance minister and leading Christian-democratic reformer Matthias Erzberger and Jewish foreign minister Walther Rathenau, and an abortive assassination attempt against the first president of the republic, Philipp Scheidemann, shook the young republic to its core (Jasper 1963; Sabrow 1994, 106–127). The judicial system – led by judges that made no secret of their affinity for anti-democratic forces – failed to effectively prosecute the assassins, and showed remarkable leniency to those who were successfully put to trial (Hannover and Hannover-Drück 1966). In response, democratic reformers attempted a strategy of institutional layering. The 1922 Law for the Protection of the Republic, designed to deter future acts of political violence, was to be enforced not by the regular German court system, but by a special Court for the Protection of the Republic (Jasper 1963, 56–92). To curb the influence of German judicial elites, the court was staffed by a majority of lay judges (Jasper 1963, 58). The fact that democratic reformers had to resort to institutional layering – rather than being able to engage in more sustainable, far reaching reform of the state apparatus – puts into sharp relief just how little success democratic reformers had in altering the politics of the bureaucratic apparatus.

2.2 The Ever-Present Threat of Violence

Weimar democracy was bookended with intense violence – the end of World War I prompted democratization, while the 1933 Enabling Act allowed the Nazi regime to embark on a campaign of intense internal repression, culminating in World War II and the genocide of European Jews. However, even throughout the democratic period, political violence was commonplace. Communist insurgencies in Bavaria and Berlin (Jones 2016), the monarchist Kapp Putsch (Erger 1967) and the 1921-1922 assassination wave (Sabrow 1994) marred democratic life in the early years of the Republic. A period of relative calm after the passage of the First Law for the Protection of the Republic was followed by an escalation in violence in the years of the Great Depression until the end of the Weimar project in 1933.

The nature of violence differed between these periods. Communist insurgencies aimed at challenging the authority of the state on a particular territory, while the assassination spree by far-right paramilitary groups targeted prominent politicians. The violence in the post-1924 period – which I can study systematically in this paper – is most adequately conceptualized as electoral violence (Birch, Daxecker, and Höglund 2020, 4). Violence occurred as a by-product of electoral competition, with armed affiliate organizations of political parties emerging as pivotal actors. In response to the early violent years of the Republic, all major political parties created strong organizational links with armed organizations sympathetic to their objectives (Schumann 2001; Siemens 2017; Elsbach 2019).[3]

3. The SPD was affiliated with the Reichsbanner Schwarz-Rot-Gold. The DNVP was affiliated with the Stahlhelm. The KPD had close connections to the Roter Frontkämpferbund (RFB), and the NSDAP was linked

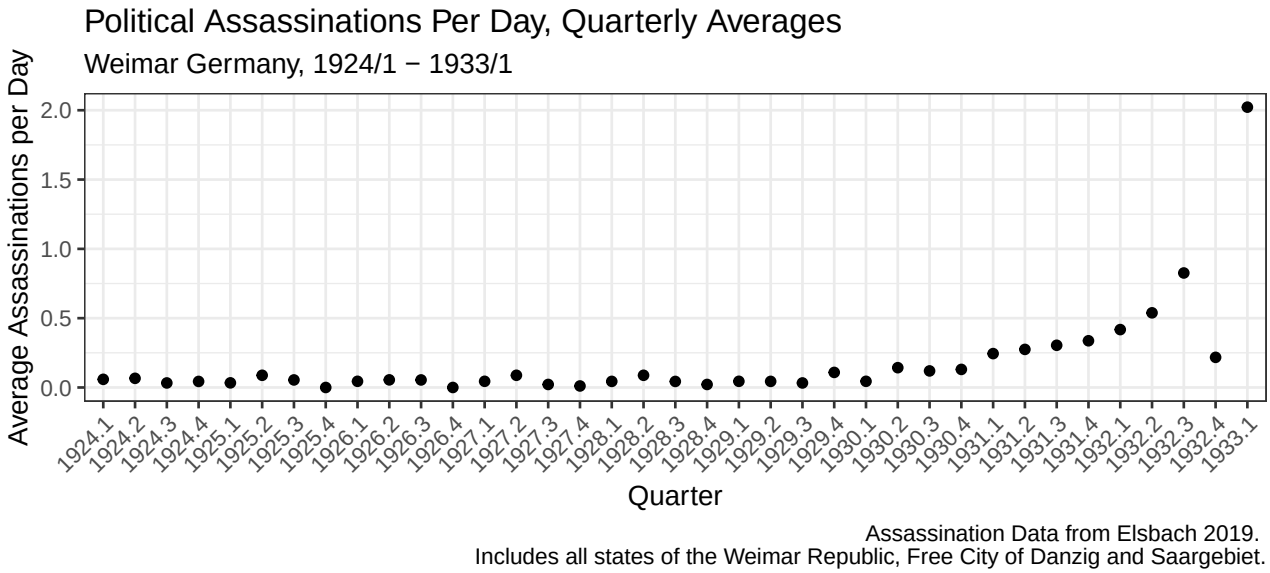

Figure 1: Political Murders per Day, Quarterly Averages, 1924/1 - 1933/1

These organizations were tasked with protecting party events in public spaces, and became critical actors in political campaigning. That was especially true for parties courting working-class voters who were typically mobilized by campaigns that were present in the streets, in beer halls and other public venues. Due to the nature of their activities, violence between members of armed party affiliates became increasingly commonplace, escalating in the years of the Great Depression. Police forces increasingly struggled to pacify elections and electoral campaigns. Tracing this evolution, Figure 1 shows quarterly averages of the number of politically motivated assassinations per day in the Weimar Republic for the period from 1924 to 1933. Violence increases steadily starting in 1930, and reaches a peak of two assassinations per day on average in the first quarter of 1933.

2.3 Prussia: The Bulwark of Weimar Democracy

The empirical focus of this paper is the largest and most populous subnational state of the Weimar Republic: the Free State of Prussia. In addition to providing the data required for my inquiry, Prussia is critical for at least two reasons. First, more than 60% of the population of the Weimar Republic lived in Prussia, and the Free State made up more than 60% of the territory of the Weimar Republic, making it the largest and most populous state of the Republic by some distance. Second, for almost the entire period between the end of World War I and the 1933 Enabling Act, Prussia was governed by a stable pro-democratic coalition of the social democratic SPD, the Catholic Zentrum, and other smaller coalition partners DDP and DVP, sometimes

to both the SA and SS.

dubbed the "Weimar coalition". With a few brief interruptions, the Prime Ministership of Prussia was held by the social democrat Otto Braun. In contrast to turbulent politics at the federal level and in other states marked by frequent turnover in government, Prussian politics were remarkably predictable. Unsurprisingly, the influential liberal foreign minister Gustav Stresemann referred to Prussia as the "bulwark of democracy" (Winkler 1988, 400). Control of Prussia was considered to be of paramount importance for national politics. In the words of close Hitler confidant Joseph Goebbels: "The key to power in Germany lies in Prussia. They who control Prussia control all of Germany" (Ribhegge 2007, 488).

The defects of Weimar democracy identified above – the rift between administrative and political elites, and the ever-present threat of political violence – filtered through from the federal level to Prussian state-level politics. High-level offices in the Prussian administration were firmly controlled by traditional elites, who had no intention of giving up their control over the state apparatus. In response, social-democratic ministers of the interior Carl Severing and Albert Grzesinski[4] embarked on a program of administrative reorganization, with the democratization of law enforcement as a key objective (Lessmann 1989; Alexander 1992, 129–132). Reforms aimed both at diversifying the ranks of administrative elites and changing how policing operated on Prussian territory. The police were meant to become democratically accountable, and police officers were to be well-trained and equipped servants of a democratic society. Severing and Grzesinski opened police academies, procured state-of-the-art equipment, and tried to modernize policing through the collection and dissemination of new statistical data on crime and police activity (Lessmann 1989).

Despite these efforts, Prussia saw significant political violence throughout the democratic period. In line with its general aim to democratize policing, the Prussian government attempted to tackle violence, which typically erupted between the armed affiliates of political parties, in a manner that would impose as few limits as possible on political expression and campaigning. In 1927, Minister of the Interior Grzesinski ordered the creation of roundtable discussions with the heads of the parties' armed factions to adhere to codified rules of engagement, including a ban on cudgels and particularly provocative activities, such as the performance of political song when in close proximity to other events.[5] As violence steadily escalated, the Ministry of the Interior responded by collecting extensive statistical data on incidents of violence, confiscation of weaponry and police activity.[6] The data were meant to make violence legible to authorities and enable police to respond with targeted interventions. Ultimately, however, efforts to tackle violence fell short. In July 1932, a riot in the town of Altona, featuring clashes between members

4. Severing served as Prussian Minister of the Interior from March 1920 to March 1921, November 1921 to October 1926, and again from October 1930 to July 1932. Grzesinski served from October 1926 to February 1930.

5. See GStA PK, I. HA Rep. 77, Tit. 4043 Nr. 119.

6. Data collection began during the tenure of social-democratic Minister of the Interior Albert Grzesinski. The original decree creating the relevant statistics is lost, but its contents can be reconstructed from GStA PK, I. HA Rep. 77, Tit. 4043 Nr. 119 Bl. 326. The records collected by the Ministry of the Interior are located primarily in GStA PK, I. HA Rep. 77, Tit. 4043 Nr. 119-124.

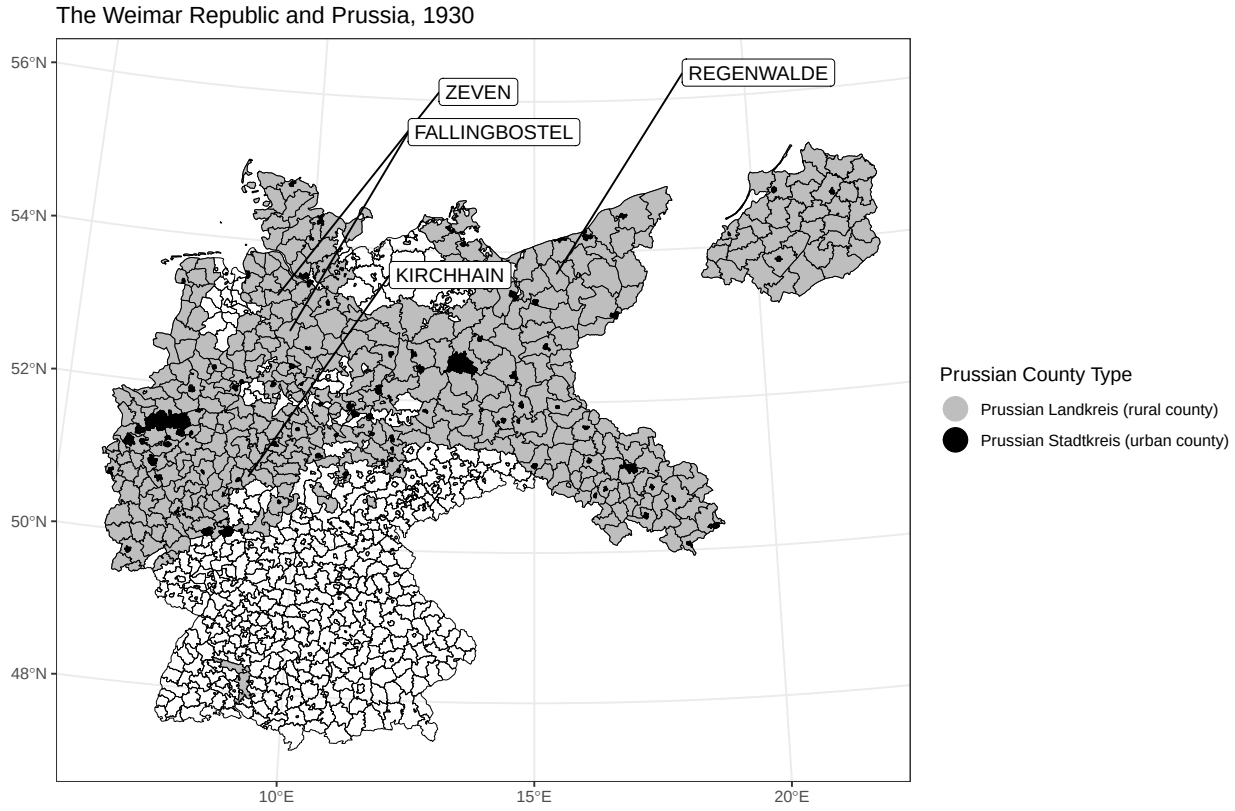

Figure 2: Map of Weimar Germany and Prussia, 1930

of the SA and communists, ended with 18 people killed. The event served as a pretext for the so-called *Preussenschlag*, a pseudo-legal coup initiated by the Nazi party, in the course of which the democratically elected Prussian government was illegally removed from office (Biewer 1983).

2.4 Policing Rural Prussia

Within the state of Prussia, my paper focuses on rural counties or *Landkreise*. Figure 2 presents a map of the entirety of the Weimar territory, the Prussian territory and its Landkreise and Stadtkreise (urban counties). I specifically label four Landkreise that I will focus on later in the paper.

Landkreise shared a common model of police governance, with authority over policing decisions vested in the *Landräte*. The Landräte were in charge of matters such as deciding where and when to deploy police officers in response to unrest, or whether or not to grant a permit to a political party organizing a public protest. While originally created as a locally elected office, since the Imperial period, the Landrat was directly appointed by the central government (Hue

de Grais 1926; Bitter 1928).[7] Already during the Imperial period, the Landrat had emerged as a critical broker of conservative interests in rural areas, leveraging the influence of his office to tilt the electoral playing field to preserve Imperial rule, particularly against social-democratic opposition (Mares 2015, ch. 3). Accordingly, Landräte were often staunch monarchists, drawn from the families of *Junkers* or landed elites (Muncy 1970, 58–62).

The democratic Prussian constitution left the office of the Landrat intact. As in the Imperial period, the Landrat had the status of a political appointee[8], subject to dismissal at the sole discretion of the Prussian cabinet (Bitter 1928; Runge 1965, 33–34). Importantly, this did not mean that Landräte who were dismissed from office were simply *fired*. Instead, dismissed Landräte were *idled*.[9] Idled officials continued to be paid a substantial share of their wage before being posted to a different office within the Prussian administration (Runge 1965, 21–23, 57–59). However, democratic reformers made one important change to the appointment process. They opened up the possibility for outsiders – individuals without law degrees and completed civil service examinations – to serve as Landräte (Bitter 1928). Previously, access to the office had been limited to trained lawyers, making it all but impossible for individuals from middle- or working class backgrounds, women or Jewish Germans to be appointed to these positions (Runge 1965, 57).

This key policy change demonstrates that democratic reformers were aware of the important role Landräte played in policing their counties. Nonetheless, replacement of officials proceeded only gradually, with many Landräte who served under the Emperor remaining in their posts. Figure 3 shows the share of traditional elite Landräte and female Landräte as a proportion of all Landräte for the years 1921 to 1932. I contrast these proportions with the proportions of women and members of the nobility among political elites, defined as candidates for and members of the Prussian state parliament, the *Landtag*.[10] Two conclusions can be drawn from the figure. First, while its influence waned over time, the nobility made up a far greater share of administrative elites compared to political elites. Second, democratization efforts were successful insofar as the share of traditional elite Landräte declined, but women remained entirely excluded from critical positions in the state apparatus, despite making inroads in electoral politics.

7. Local parliaments or *Kreistage* were allowed to propose suitable candidates, but the Prussian state government retained complete authority in staffing matters.

8. In German: *politischer Beamter*.

9. In German: *zur Disposition stellen*.

10. See the Data section in the paper for further information on sources.

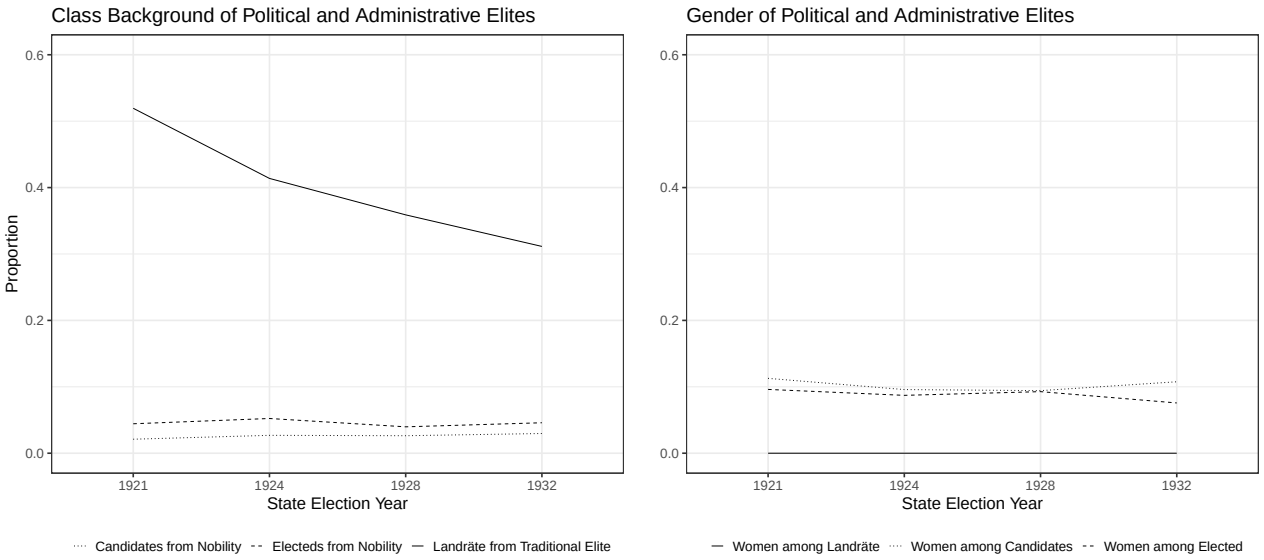

Figure 3: Administrative and Political Elites in Prussia, 1921-1932

3 Theory

This section theorizes about the effects of replacing an autocratic, traditional elite Landrat with a Landrat vetted and appointed by democratic reformers. What are the effects of sidelining autocratic bureaucrats on democratic life? My theory focuses on three sets of actors: The pro-democratic central government, the Landräte, and anti-democratic mobilizers or political parties, notably the KPD and NSDAP.

In policing, the democratic central government pursues two objectives: On the one hand, it seeks to maintain public order and limit violence. On the other hand, it seeks to enable free political expression and campaigning. These two objectives can come in conflict. Allowing unfettered political expression may increase the risk of violence in polarized contexts. For example, allowing competing political parties to hold simultaneous campaign rallies in the same town can lead to violent clashes. This in turn justifies limitations on expression, giving rise to tradeoffs between the two competing objectives of maintaining order and permitting expression. The central government delegates the resolution of these tradeoffs to directly appointed agents, or the Landräte.

The Landräte are in charge of making day-to-day policing decisions, trading off public order and political expression concerns. However, the Landräte also hold their own preferences over political regimes. Traditional elite officials are hostile to democracy and sympathetic to anti-democratic forces. Outsiders to the traditional bureaucratic elite, vetted by the pro-democratic central government, are hostile to anti-democratic forces. Regime preferences affect how Landräte police their counties. Traditional elites will allow for greater mobilization and expression by anti-democratic forces than non-traditional elites. Because policing decisions are

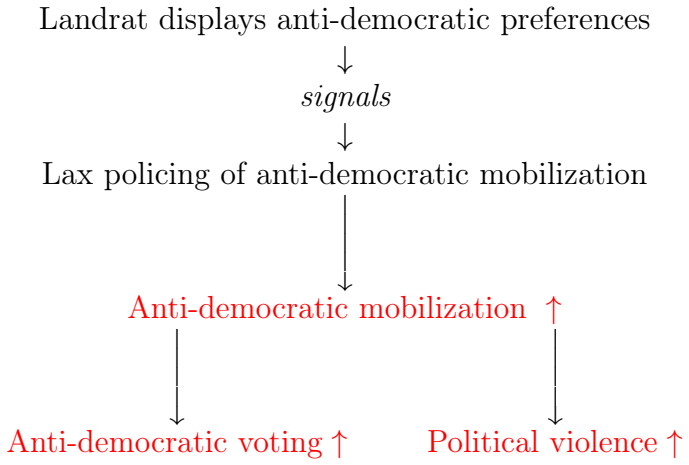

Figure 4: Expected Effect of a Traditional Elite Landrat

complex and depend on local knowledge, it is difficult for the central government to monitor individual policing decisions, giving Landräte wide discretion in decision-making.

Finally, anti-democratic mobilizers – like any political actor seeking power and influence – must decide where to focus their limited resources for maximum impact. A central consideration is the perceived cost of mobilizing in a particular location imposed by law enforcement. Unsympathetic law enforcement can impose costs by imposing restrictions on campaigning. Sympathetic law enforcement, on the other hand, facilitates campaigning by removing restrictions and turning a blind eye to the use of violent and intimidating campaign tactics. Anti-regime mobilizers will thus be very sensitive to signals conveying information about the political preferences of Landräte, and allocate campaigning resources accordingly.

Figure 4 summarizes how I expect a traditional elite Landrat to affect political life in his county. When a Landrat signals anti-democratic preferences through policing action, inaction, or the use or non-use of politically charged symbols, anti-democratic mobilizers infer that their mobilizing activities will be met with a lax response from law enforcement. This lowers the cost of anti-democratic mobilization, and increases the likelihood of violent incidents, given the embrace of violent tactics by anti-democratic forces. Greater resources devoted to mobilization should also pay electoral dividends, increasing the vote share of anti-democratic parties in elections.

I expect that the replacement of a traditional elite Landrat changes the cost-benefit calculus for anti-democratic mobilizers. I summarize the expected effects of replacement in Figure 5. Replacement signals a change of the preferences of the Landrat, and, consequently, a change in policing priorities. Anti-democratic mobilizers can no longer count on lax policing of their activities. As mobilization is reduced due to increased costs, the risk of violence declines, as does the electoral success of anti-democratic parties.

In sum, this theory generates three predictions regarding the causal effect of Landrat re-

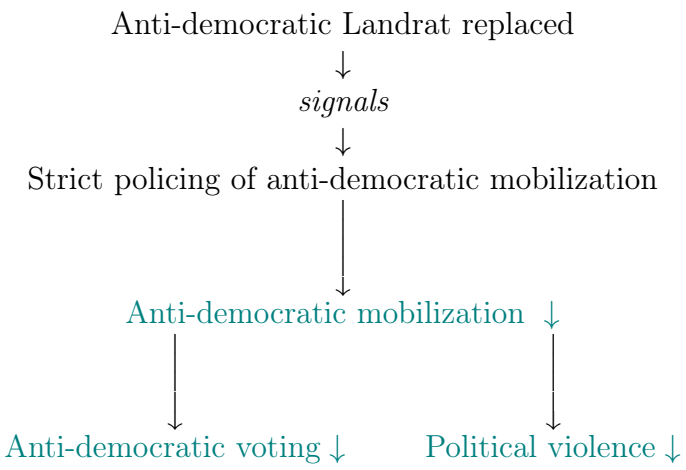

Figure 5: Expected Effect of Replacing Traditional Elite Landrat

placement. First, I expect a mobilization effect:

H_1: Replacing a traditional elite Landrat with an outsider Landrat *decreases* mobilization efforts by anti-democratic parties.

Second, I expect a pacification effect:

H_2: Replacing a traditional elite Landrat with an outsider Landrat *decreases* the incidence of political violence.

Third, I expect electoral effects:

H_3: Replacing a traditional elite Landrat with an outsider Landrat *decreases* the vote share of anti-democratic parties, and *increases* the vote share of pro-democratic parties in elections.

4 Data

I test my theoretical predictions about the effects of law enforcement staffing on political violence, party mobilization efforts and electoral outcomes using original panel data from the state of Prussia, covering the period between 1920 and 1932.

4.1 Unit of Analysis

The cross-sectional unit of analysis in this paper is the rural county (*Landkreis*) in the state of Prussia. Due to cession of various territories as required by the Treaty of Versailles as well as a number of internal territorial reforms, the number of counties declines slightly, from 430 in 1920 to 416 in 1932.

4.2 Law Enforcement Personnel

In each county, ultimate authority in local policing decisions was vested in a *Landrat*. I collected original biographical data on the universe of Landräte in the state of Prussia for the years 1917 and 1920 to 1932, using official government records – the *Handbooks of the Prussian State* – as a primary source.[11] In a first step, I recorded the name of the Landrat for each county-year. Using string pattern recognition, I then recorded additional information for each county-year. First, I recorded whether a Landrat in a particular county-year after democratization had already been in office prior to democratization (in 1917). Second, based on the name of the official, I recorded whether a Landrat in a particular county-year was a member of the German nobility. Nobility status is identifiable based on the appearance of nobility titles such as "Graf" or "Freiherr" as well as the prefix "von" in an official's last name. Although nobility as a legal status was abolished in the Weimar constitution, and nobility titles no longer held legal significance, they survived as components of family names (Runge 1965, 180).

Based on this information, I then create an indicator variable denoting a Landrat's status as an outsider to traditional administrative elites. To be classified as an outsider (and coded as 1), an official must not have been occupying the office of police chief in a particular county since before democratization, and must not belong to nobility. Officers who remained in their posts since before democratization or were members of the nobility are classified as traditional elites and coded as 0.

Focusing on the social class of appointees – rather than simply their tenure in a particular county – is important given that members of the nobility were among the staunchest opponents of German democracy, while also claiming the lion's share of elite positions in the pre-democratic German administrative apparatus (Muncy 1970; Runge 1965, 179–204). The problematic role of the landed nobility – the infamous *Junker* class – has been emphasized in many accounts of the erosion of Weimar democracy (Gerschenkron 1946; Muncy 1970), and reforms intended to open elite administrative positions were intended precisely to break the stranglehold of the *Junker* class on administrative appointments. Wherever possible, the Prussian executive attempted to appoint outsiders to Landrat positions, progressively marginalizing members of the nobility.

Figure 6 displays Landrat identity – my treatment variable – for the years 1921, 1924, 1928 and 1932 in Prussian counties. The maps show that traditional elite Landräte were progressively replaced, but remained in charge of various counties right until the end of the democratic period.

4.3 Mobilization – Candidate Entry

Hypothesis 1 posits that variation in Landrat identity causally affects mobilization efforts by various political forces. Ideally, I would examine effects on a holistic measure of campaign effort, including information on campaign rallies and voter outreach. Unfortunately, existing

11. I cross-check information with records of Landrat appointments on German Wikipedia, and rely on the Wikipedia pages for Landkreise wherever possible when encountering missing information.

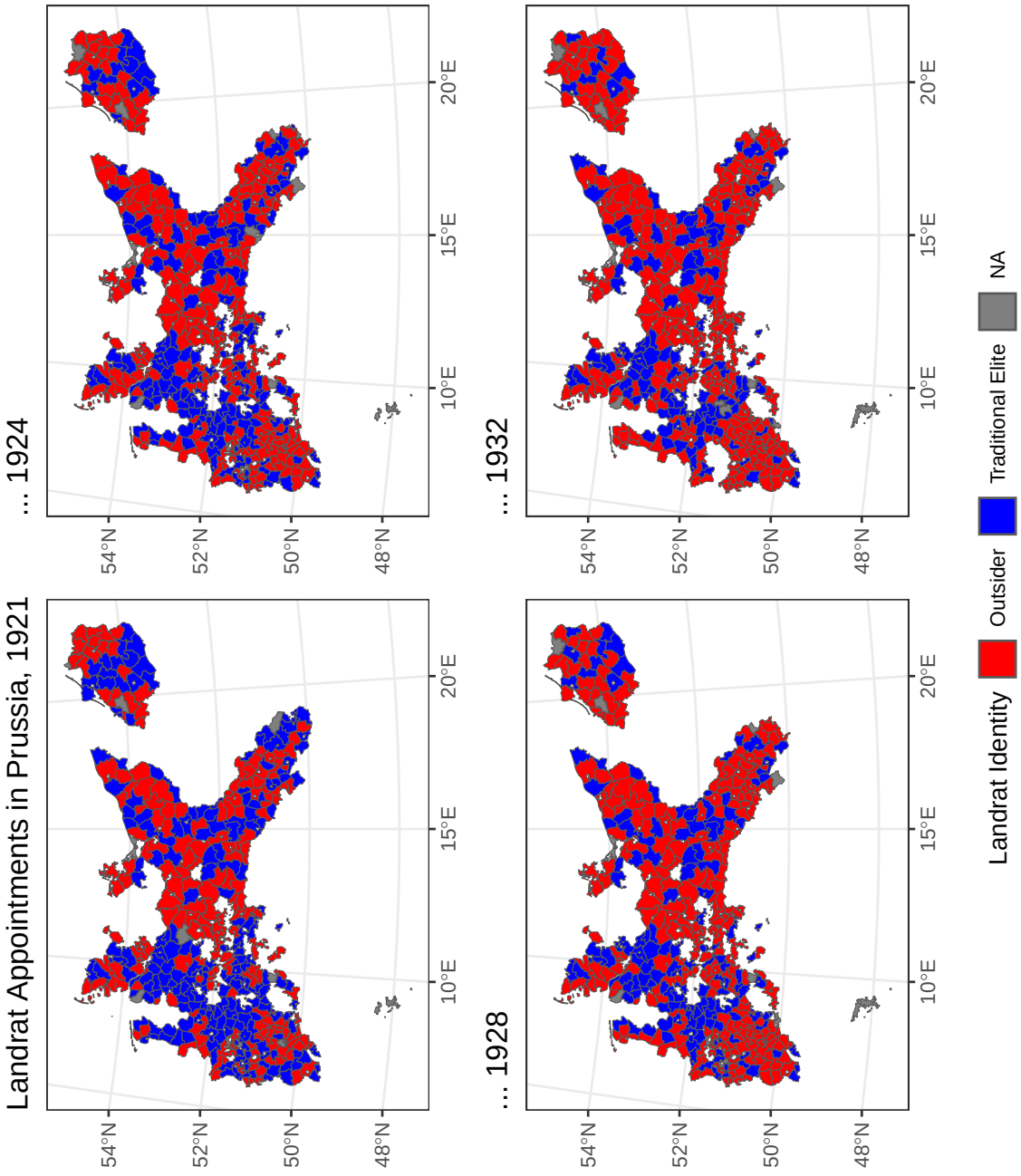

Figure 6: Landrat Appointments in Prussia, 1921-1932

data on mobilization efforts suffer from important limitations. Caprettini, Caesmann, Voth, and Yanagizawa-Drott (2022) study the effects of NSDAP propaganda marches on electoral outcomes. While important, their paper studies marches in a single city (Hamburg) in a single year (1932). Selb and Munzert (2018) collect data on location and time of campaign speeches by Adolf Hitler to estimate electoral effects. Unfortunately, Hitler typically spoke in or near urban areas, and there is little variation to exploit for rural areas, which are the focus of this paper. Moreover, existing work focuses on campaign efforts by the NSDAP and ignores outcomes for other parties, including pro-democratic forces as well as the Stalinist, anti-democratic KPD.

In this paper, I therefore use the entry of candidates for office in state parliament elections, disaggregated by political party family, as a proxy for mobilization effort. While not a perfect measure, I argue that candidate entry is a good proxy for party mobilization effort for at least two reasons. On the one hand, seeing that a given party nominates a large number of candidates residing in a particular locality on its party lists can indicate that the party has historically enjoyed success in that locality, and is *retrospectively* rewarding its activists with list positions.[12] On the other hand, given that electoral lists were public, seeing that a given party nominates a large number of candidates residing in a particular locality on its party lists can indicate that the party *prospectively* targets said locality for mobilization, and is attempting to sway voters with the promise of local representation. Either way, variation across time and space in candidate entry gives us a good sense of variation across time and space in mobilization efforts.

To quantify candidate entry, I build an original dataset relying on lists (*Wahlvorschläge*) containing the universe of candidates for the Prussian State Parliament, collected from the *Handbooks of the Prussian Landtag*. I analyse the four free and fair elections for Prussian parliament held in 1921, 1924, 1928 and 1932. In the closed-list proportional representation system used in elections to the federal and state parliament, parties submitted ordered lists of candidates for office. Lists could be submitted for each of Prussia's 23 electoral districts as well as for the entire state. Candidates were eligible to appear on multiple lists irrespective of their residence. Each list entry identifies the candidate's name, their party affiliation, their position on the ordered list, a self-reported descriptor of their occupation, their place of residence, as well as whether they won a seat in parliament.

After digitizing the candidate lists, I converted the lists to tabular form with the help of a research assistant. The research assistant also coded the gender of the candidate as female or male based on the name of each candidate entry. In a second step, I used string matching to identify candidates who appeared on multiple lists based on their name and other characteristics, to create a dataset of all *unique* candidates running for office for each election year. Using the string pattern recognition approach described earlier, I then coded whether candidates were members of the nobility and whether they held a doctoral degree based on their names. I also record whether candidates were novices (i.e. they had never run for office before) and whether they were incumbents (i.e. they had won a seat in parliament in the previous election).

12. Note that legislative elections in Prussia were conducted according to closed-list proportion-representation rules. For a more detailed description of the electoral system, consult the *Handbooks of the Prussian Landtag*.

In a third step, I geocode candidate residences using the Google Maps API, and a handcoded crosswalk where automated geocoding failed or gave incorrect results.[13] Using historical shapefiles created by Project Mosaic (MPIDR and CGG 2011), I then record the county of residence of each candidate, and aggregate the candidate data to create an election year-county panel.[14] For each election year-county, I record the number of candidates vying for office, overall as well as separately by party family. I analyze these measures both in absolute terms and adjusted for county population. For population adjustment, I use data on the number of voting-age adults by county-year computed by Falter and Hänisch (1990).[15]

4.4 Political Violence

Hypothesis 2 posits that variation in Landrat identity causally affects the incidence of political violence. To measure incidence of political violence, I rely on a dataset of assassinations committed with a demonstrably political motive compiled by the historian Sebastian Elsbach (2019, 618–667). Based on evidence triangulated from multiple sources, including government records, the press, and records of civil society organizations, Elsbach's data cover all of Weimar Germany including the Free City of Danzig and the occupied Saargebiet for the period from 1924 to 1933. For each assassination event, the data record date and place of the assassination, the name of the victim, and the organizational affiliations of the perpetrator and victim. Each entry in the list is accompanied by detailed information on the sources used to characterize the event. During a visit to the Prussian Privy State Archives, which houses some of the law enforcement records Elsbach relies on, I was able to verify the reliability of the information provided in the list.

The Elsbach dataset has a number of desirable properties for the purposes of my analysis. First, it covers the entire period from 1924 to 1933, and the entirety of the state of Prussia. Second, though it does not contain information on other important forms of violence, including vandalism and assault, the focus on assassinations has the advantage that the occurrence of assassinations is often easier to verify by triangulating information from multiple sources. In high-capacity states, such as Weimar Germany, assassinations leave a significant administrative "paper trail", in sharp contrast to less intense forms of violence that do not entail the death of one or more individuals. This paper trail allows the historian to verify the occurrence of an assassination with greater reliability, reducing measurement error.

These advantages come into particularly sharp relief when comparing the Elsbach data to another potential source of information on incidents of political violence, the *Statistic on Political Riots* compiled by the Prussian Ministry of the Interior, starting in 1927. The statistic

13. Automated geocoding failed for around 500 unique places of residence. I corrected missing or erroneous geocoding by hand, relying primarily on *Meyers Gazetteer*, URL: https://www.meyersgaz.org/help/help.html.

14. The use of Project Mosaic shapefiles and Google Maps API for geocoding is standard in quantitative research on Weimar Germany (Selb and Munzert 2018; De Juan et al. 2024).

15. The denominator is not available for the year 1921, so I am using information on the voting-age population from 1920 instead.

was designed to collect yearly county-level counts of political riots necessitating the intervention of police forces, with information on the organizations involved in a particular riot.

These data are inferior to the Elsbach data in a variety of ways. First, although data were collected from 1927 to 1933, archival records are incomplete, and are missing almost entirely for the years 1927 to 1929, rendering longitudinal data analysis difficult. Second, and critically, the data were collected under the supervision of the *Landrat* in each county – precisely the authority whose professional reliability and willingness to cooperate with central Prussian authorities is doubtful! Indeed, during archival research, I came across correspondence between the Prussian Ministry of the Interior and county-level officials revealing concerns about incorrect, incomplete or untimely reporting, raising broader concerns about the reliability of these data.[16] As in many other contexts, we cannot take law enforcement data from Weimar Germany at face value without further verification (Cook and Fortunato 2023).

To go from the raw Elsbach data to county-year measures of the incidence of political violence, I first geocoded assassination events using the Google Maps API. Using historical shapefiles created by Project Mosaic (MPIDR and CGG 2011), I then record which county a particular assassination event took place in, and generate a county-year panel aggregating information from the raw dataset. The key outcome variable I will focus on is an indicator variable taking the value 1 for each county-year with a recorded assassination, and 0 otherwise.

4.5 Electoral Outcomes

Hypothesis 3 posits that variation in Landrat identity causally affects electoral outcomes. To measure electoral outcomes at the county level, I use data compiled by Falter and Hänisch (1990) on county-level electoral returns in federal parliament (*Reichstag*) elections for the years 1920, 1924, 1928, 1930 and 1932. Unfortunately, I am not aware of county-level electoral data for state parliament elections. Where multiple elections were held in a single year, I average over the electoral returns to get a single election year-county estimate. The data contain information on the number of voting-age adults in a particular county, the vote shares of all major parties, and voter turnout.

For the purposes of this analysis, I am particularly interested in the results of the pro-democratic *Weimar Coalition* composed of SPD, Zentrum, DVP and DDP on the one hand, and the anti-democratic KPD and NSDAP on the other hand. The vote shares of these two sets of parties arguably matter most to explain democratic erosion. The 1932 coup against the Prussian government followed an election which gave the KPD and NSDAP a negative majority,

16. In one notable case, the Landrat of Rastenburg county in the province of East Prussia, Dodo zu Innhausen und Knyphausen (in office since 1912) was accused of producing misleading data, claiming that a riot was started by the Reichsbanner Schwarz-Rot-Gold (a pro-democratic paramilitary group), conflicting with reports of his superiors that the riot had been started by the NSDAP. The Landrat offices were asked to collect data on perpetrators and victims of violence in addition to counts, a classification task that is far from obvious. However, disagreements also emerged over which types of incidents to report and over untimely reporting. See GStA PK, I. HA Rep. 77, Tit. 4043 Nr. 119 Bl. 327-336.

undermining the legitimacy of the working pro-democratic government (Biewer 1983). Hence, the key dependent variables will be the combined vote share of KPD and NSDAP and the combined vote share of the parties of the Weimar coalition, respectively.

Table 1 shows descriptive statistics. Note that because outcome variables are available for different years, the panels differ in the number of time periods available for analysis. The table shows descriptive statistics separately for each panel.

5 Estimation

My theory makes predictions about the causal effects of replacing a traditional elite Landrat with a democratically vetted Landrat on my outcomes of interest. I estimate this causal quantity from panel data. Due to recent advances in panel data econometrics, best practice for estimating such effects has evolved rapidly, as the limitations of the workhorse two-way fixed effects (TWFE) estimator have been clarified. This research has shown that even when basic identifying assumptions are met, the TWFE estimator correctly quantifies causal effects only under further restrictive assumptions on the nature of treatment effects. Alternative estimators are now available for causal panel analysis, many of which either impose significant restrictions on the nature of treatment trajectories of different units (such as staggered adoption) or the units admissible for treatment estimation, thus limiting statistical power (Liu, Wang, and Xu 2024, 160, 175).

Given the particularities of my estimation task, in this paper, I rely on the fixed effects counterfactual (FEct) estimator developed by Liu, Wang, and Xu (2024). This estimator ensures that average causal effects are convex combinations of individual treatment effects. I use time and unit fixed effects for effect estimation, and standard errors are computed using the bootstrap. In addition to the FEct estimator, I also implement the traditional TWFE estimator, using time and unit fixed effects. The estimator is computed using OLS with CR2 standard errors clustered at the unit level.

One central difference between the FEct and the TWFE estimators is the definition of appropriate control units. The TWFE estimator uses data from always-treated units – that is, units that are in treatment throughout the panel – as controls for later-treated units. The FEct discards these units as inappropriate, in order to establish robustness to heterogeneous treatment effects (Chiu et al. 2024, 9). Table 2 shows the proportion of counties that are in control (never-treated), always-treated, and later-treated.

Table 2 shows that approximately half of all units are always-treated. These units will be discarded by the FEct estimator – hence, as we will see in the results section, this estimator uses a lower number of units for estimation.[17] However, discarding units implies that the FEct estimator is robust to heterogeneous treatment effects. When the two estimators give divergent

17. I also exclude the small number of units that exhibit missing data or treatment reversal to keep the analysis tractable.

Panel 1: Landrat Appointments and Violence

County-year, 1924-1932

Variable	*Min.*	*1st Quartile*	*Median*	*Mean*	*3rd Quartile*	*Max.*	*SD*	*Num. obs.*	*T*
Replace Traditional Elite	0	0	1	0.66	1	1	0.47	3429	9
Any Assassination	0	0	0	0.03	0	1	0.16	3331	9
N Victims	0	0	0	0.03	0	4	0.21	3331	9

Panel 2: Landrat Appointments and Candidate Entry

County-election year (Prussia): 1921, 1924, 1928, 1932

Variable	*Min.*	*1st Quartile*	*Median*	*Mean*	*3rd Quartile*	*Max.*	*SD*	*Num. obs.*	*T*
Replace Traditional Elite	0	0	1	0.60	1	1	0.49	1541	4
Candidates/10k voters	0	0.34	0.69	0.82	1.15	4.89	0.70	1444	4
Weimar Coalition candidates/10k voters	0	0	0.10	0.25	0.40	2.27	0.34	1444	4
Communist candidates/10k voters	0	0	0	0.12	0.20	2.85	0.24	1444	4
Nazi candidates/10k voters	0	0	0	0.11	0	2.20	0.25	1444	4

Panel 3: Landrat Appointments and Voting

County-election year (Reich): 1920, 1924, 1928, 1930, 1932

Variable	*Min.*	*1st Quartile*	*Median*	*Mean*	*3rd Quartile*	*Max.*	*SD*	*Num. obs.*	*T*
Replace Traditional Elite	0	0	1	0.63	1	1	0.48	1917	5
N Voters	4446	21957	29627	34180.00	41415	371736	20599.71	1829	5
Vote Share, Weimar Coalition, in %	10.45	34.26	48.79	50.89	65.99	99.43	20.85	1829	5
Vote Share, Nazi and Communist, in %	0	3.18	10.59	18.47	28.96	75.06	19.23	1829	5
Vote Share, Nazi, in %	0	0.37	3.14	12.04	19.71	69.16	16.36	1829	5
Vote Share, Communist, in %	0	1.67	4.60	6.43	9.01	42.53	6.61	1829	5
Voter Turnout, in %	51.45	75.36	79.81	78.97	83.28	93.41	6.33	1829	5

Table 1: Summary Statistics

County Treatment Status	Proportion of Counties
Always Treated	0.461
Later Treated	0.242
Control	0.297
Total	1

Table 2: Counties by Treatment Status

results, the FEct estimator is more reliable. The estimated effects should be interpreted as average treatment effects that are local to units that come into treatment during the panel.[18]

6 Results

How did the sidelining of traditional elite Landräte affect political life in Prussian counties? In this section, I present the results of my causal panel estimation. In line with best practice, I present results from TWFE and FEct in tabular form. I also show dynamic treatment effect plots developed Liu, Wang, and Xu (2024, 166–168). I plot estimated period-wise average causal effects and corresponding 95% confidence intervals against time period relative to treatment onset, with period 1 being the first treated period. The bar plot at the bottom of the plot shows the number of treated units for each time period.[19] Standard errors are clustered at the county level.

6.1 Mobilization Effects

H_1 posits that replacing a traditional elite Landrat with an outsider Landrat should decrease mobilization efforts by anti-democratic parties. Table 3 and Figure 7 display overall and period-wise effects separately for the pro-democratic parties of the Weimar coalition, the Nazi party, and the KPD.

Table 3 shows a negative and statistically significant effect of Landrat replacement on entry of candidates affiliated with the Nazi party. Consistent with Hypothesis 1, the replacement of a traditional elite Landrat with a democratically appointed outsider Landrat on average reduces the number of candidates affiliated with the Nazi party running in a particular locality.

Contrary to my expectation, we do not see a similar effect for the Stalinist KPD. In fact, the point estimate for entry effects for KPD candidates is positive, although the coefficients are small and statistically indistinguishable from zero. Entry effects for candidates affiliated with pro-democratic parties of the Weimar coalition are also positive but statistically indistinguishable

18. Just like the treatment effects in a regression discontinuity design are local to the units at the discontinuity.
19. The number above the zero bar gives the number of treated units in period zero.

	DV: Weimar Coalition candidates/10k voters			*DV: Nazi candidates/10k voters*			*DV: Communist candidates/10k voters*		
	TWFE	FEct	FEct	TWFE	FEct	FEct	TWFE	FEct	FEct
Replace Traditional Elite	0.050	0.052	0.037	-0.058^{**}	-0.066^{**}	-0.090^{***}	0.015	0.026	0.029
	(0.043)	(0.050)	(0.043)	(0.026)	(0.032)	(0.035)	(0.026)	(0.033)	(0.032)
obs equally weighted		✓			✓			✓	
units equally weighted			✓			✓			✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
RMSE	0.281	0.249	0.249	0.221	0.152	0.152	0.199	0.138	0.138
N Clusters	393	202	202	393	202	202	393	202	202

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 3: Replacement of Traditional Elites and Entry, by Partisan Group

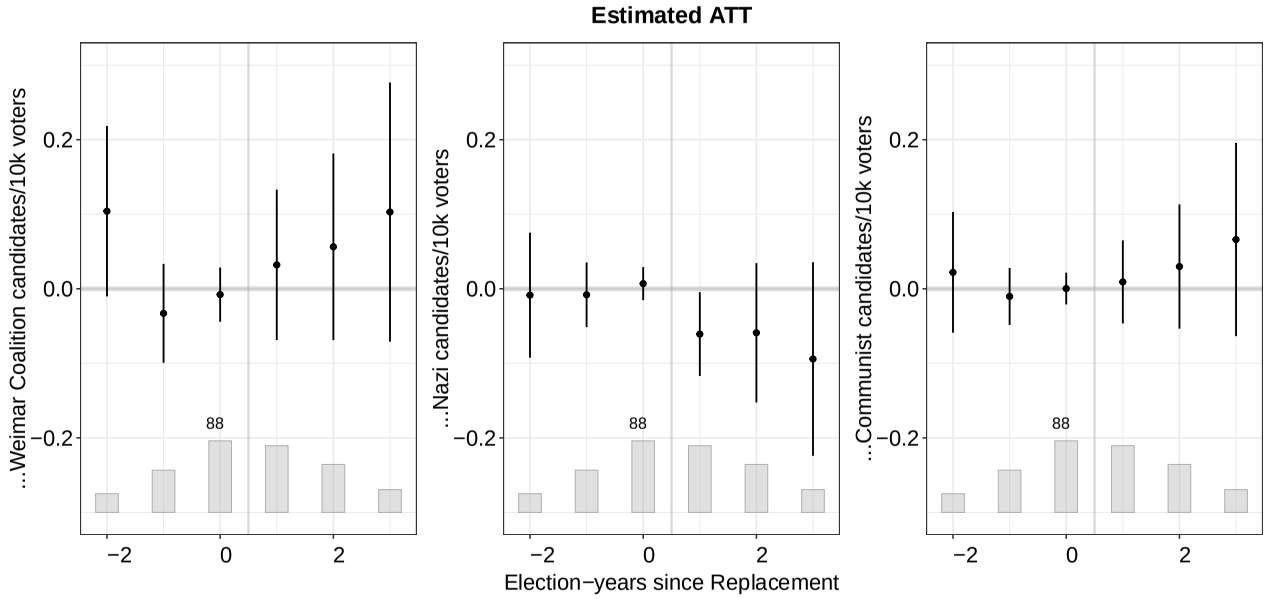

Figure 7: Period-wise ATT Estimates on Candidate Entry, by Party

	DV: Any Recorded Assassination		
	TWFE	FEct	FEct
		obs equally weighted	units equally weighted
Replace Traditional Elite	-0.037^{***}	-0.036^{**}	-0.039^{**}
	(0.012)	(0.015)	(0.016)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
RMSE	0.153	0.082	0.082
N Clusters	388	163	163

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 4: Replacement of Traditional Elites and Violence

from zero.[20]

In sum, replacement of traditional elite Landräte with democratically appointed outsiders caused a decline in Nazi mobilization efforts. However, we do not see the same effect for the KPD, suggesting the mobilization responses were asymmetric. One plausible explanation for this asymmetry is that while democratically appointed Landräte deterred anti-democratic mobilization by both Stalinist and Nazi forces, the presence of traditional elite Landräte weakened deterrence specifically against anti-democratic forces on the far right.

6.2 Pacification Effect

In H_2, I conjectured that replacing a traditional elite Landrat with a democratically vetted Landrat would reduce the incidence of political violence. Table 4 and Figure 8 show the estimated causal effect of replacement on the incidence of political assassinations.

Both TWFE and FEct estimators[21] agree that on average, replacing a traditional elite Landrat with an outsider Landrat *reduces* the incidence of political violence. Compared to a county with a traditional elite Landrat remaining in office, a county in which such a Landrat is replaced reduces its risk of seeing a political assassination by on average 3 to 4 percentage points. Figure 8 plots dynamic treatment effects. Replacement of a traditional elite Landrat by an outsider official reduces the risk of deadly political violence.[22]

20. Appendix B.1 shows that results do not change when we do not normalize the dependent variable for county population.

21. The number of clusters for FEct is smaller than in the estimation of mobilization and electoral effects. This is because violence data are only available for 1924 and the years after, while the other two panels start earlier. Given progressive replacement of Landräte, for a panel starting in 1924, there are more always-treated units, which FEct discards.

22. Appendix B.2 shows that these results are robust to a different operationalization of the dependent variable,

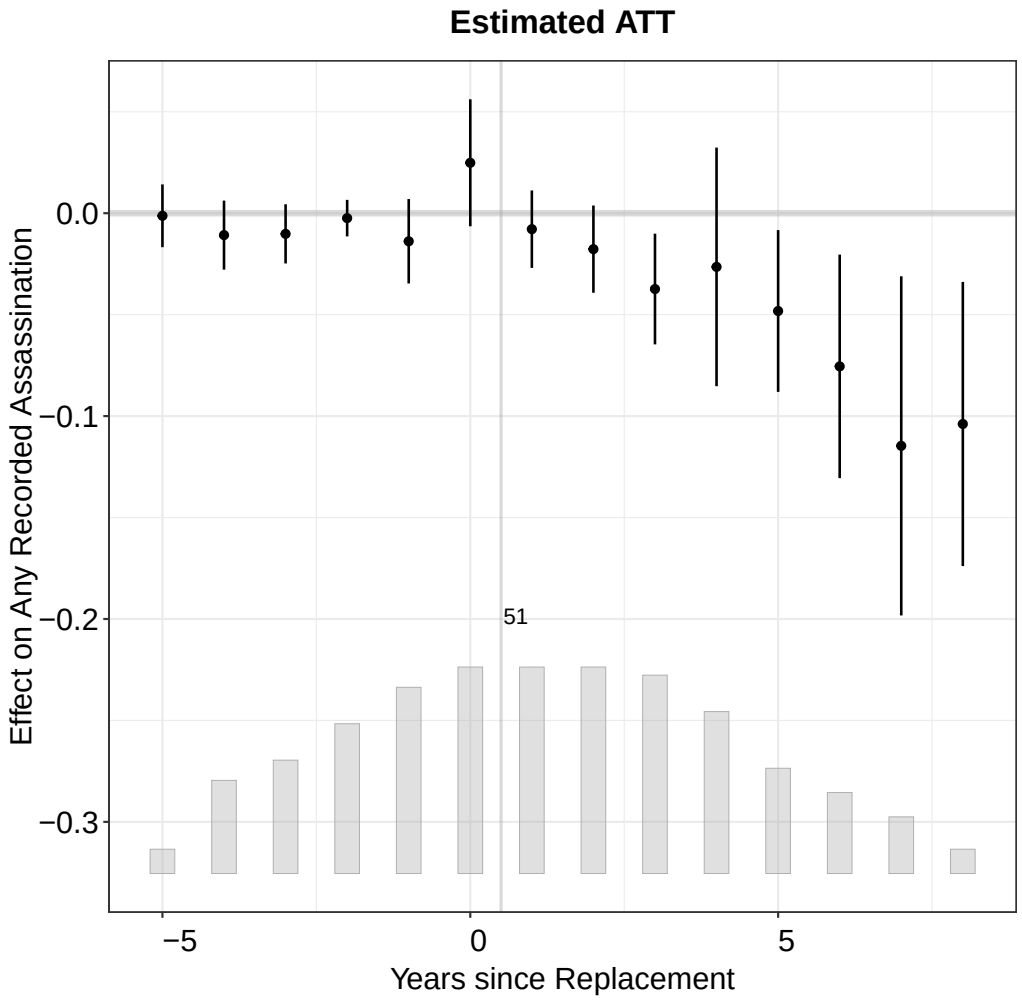

Figure 8: Period-wise ATT Estimates on Any Recorded Assassination

	DV: Vote Share, Weimar Coalition			*DV: Vote Share, Nazi and Communist*		
	TWFE	FEct	FEct	TWFE	FEct	FEct
Replace Traditional Elite	0.190	3.311*	3.444**	−1.670*	−1.913*	−2.091
	(1.291)	(1.742)	(1.733)	(1.007)	(1.076)	(1.279)
obs equally weighted		✓			✓	
units equally weighted			✓			✓
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
RMSE	7.938	7.449	7.449	6.837	4.015	4.015
N Clusters	395	206	206	395	206	206

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 5: Replacement of Traditional Elites and Party Vote Shares

Moreover, the figure shows that the pacifying effect of the democratization of law enforcement increases over time, with the largest effects estimated more than five years after treatment onset. This suggests that democratically appointed outsider Landräte became more effective at reducing violence the longer they were in office. Hence, early replacement would have paid particularly large dividends in terms of reducing violence.

6.3 Electoral Effects

What are the effects of change in law enforcement personnel on electoral outcomes? In H_3, I conjectured that replacing a traditional elite Landrat with an outsider Landrat should depress voting for anti-democratic parties. Conversely, replacement should increase vote shares for parties of the pro-democratic Weimar coalition. Table 5 and Figure 9 summarize the findings.

First, replacing a traditional elite Landrat has a positive effect on the vote share of the pro-democratic Weimar coalition parties, as seen in the first three columns of Table 5 and the upper panel of Figure 9. The TWFE and FEct estimators differ substantially in the estimated effect size. Where the two estimators disagree – for example, in the presence of effect heterogeneity – FEct is a more reliable estimator than TWFE. The results suggest that compared to control counties, counties which replace a traditional elite Landrat with an outsider Landrat on average see vote share for Weimar coalition parties increase by 3.3 to 3.4 percentage points. This effect is substantial, and in line with comparable effects found in the literature on Weimar voting. For comparison, De Juan and coauthors (2024, 153) estimate that compared to counties with below-median WW1 casualty fatality rates, counties with above-median WW1 casualty fatality rates see an increase in voting for nationalist parties by around 5 percentage points.

Second, we see the opposite average effect on voting for anti-democratic parties, an effect

dividing the number of recorded victims of assassinations by county population.

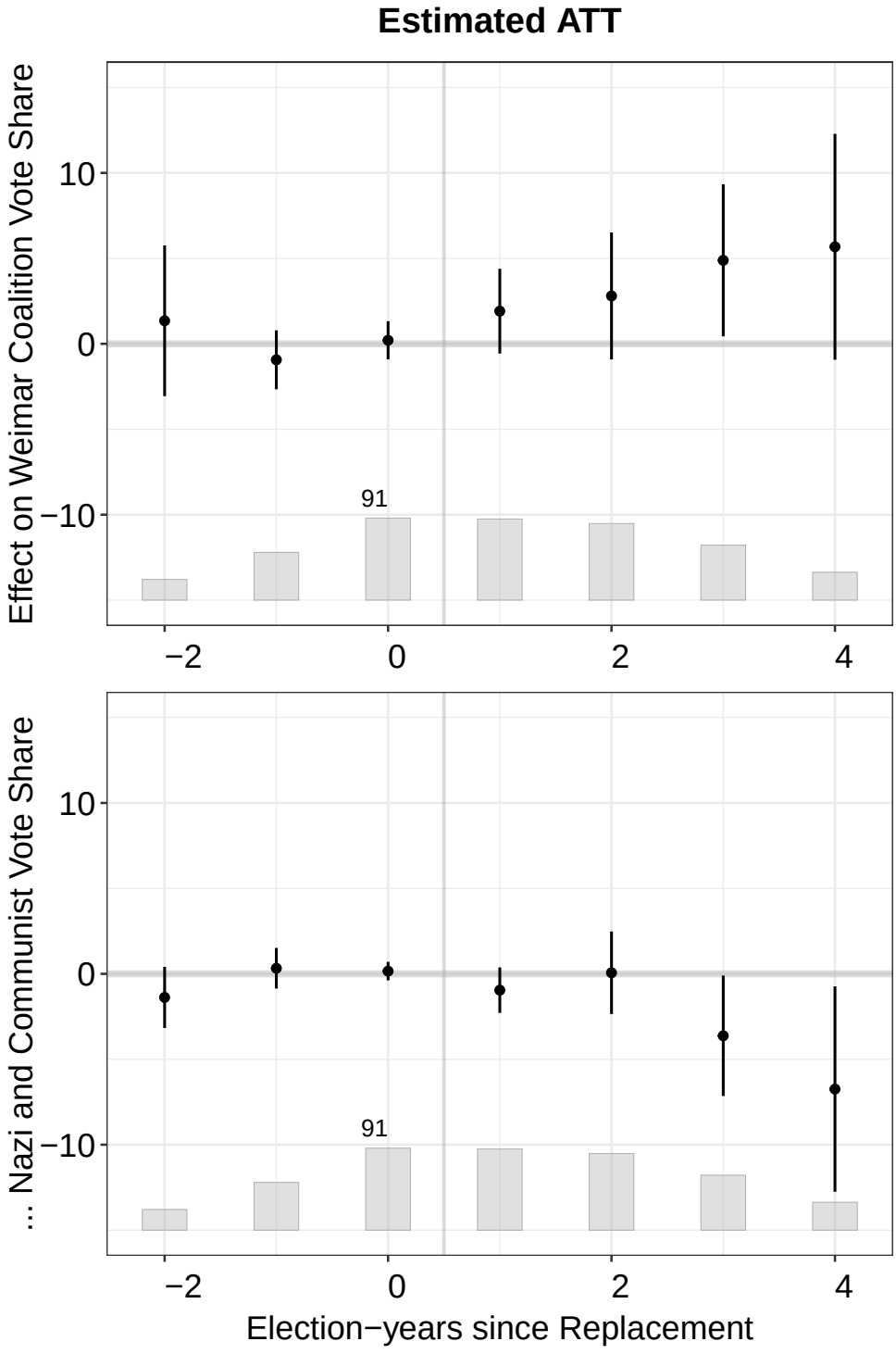

Figure 9: Period-wise ATT Estimates on Voting

that manifests irrespective of the estimator used. The estimates are summarized in the last three columns of Table 5 and the lower panel of Figure 9. Compared to control counties, counties which replace a traditional elite Landrat with an outsider Landrat on average see the combined vote share for the NSDAP and stalinist KPD decline by 1.7 to 2 percentage points. The democratization of law enforcement thus increases vote shares for pro-democratic parties and limits vote shares for anti-democratic parties, lending support to H_3.

The combined vote share of NSDAP and KPD is the central quantity of interest because it is directly linked to democratic erosion in Prussia. The fact that the 1932 state parliament elections gave NSDAP and KPD a negative majority was a prerequisite for the pseudo-legal coup that forced the democratically elected Prussian government out of office (Biewer 1983). Nonetheless, given that we found that treatment negatively affected NSDAP mobilization but not KPD mobilization, it is also worth disaggregating electoral results to see if mobilization and electoral outcomes show a similar effect pattern. Table 6 shows average treatment effects separately for NSDAP and KPD.

	DV: Vote Share, NSDAP			*DV: Vote Share, KPD*		
	TWFE	FEct	FEct	TWFE	FEct	FEct
Replace Traditional Elite	−1.319	−1.935*	−2.334*	−0.351	0.022	0.244
	(0.990)	(1.041)	(1.270)	(0.413)	(0.430)	(0.419)
obs equally weighted		✓			✓	
units equally weighted			✓			✓
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
RMSE	5.828	2.941	2.941	2.693	3.102	3.102
N Clusters	395	206	206	395	206	206

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 6: Replacement of Traditional Elites and Anti-Democratic Voting, by Party

Indeed, Table 6 shows that electoral and mobilization patterns respond similarly to treatment. On average, compared to control counties, treated counties saw lower vote shares of the NSDAP, with an estimated reduction in vote share of between 1.3 to 2.3 percentage points. By contrast, the effect on KPD vote share is indistinguishable from zero on average. This reinforces the idea that presence of traditional elite Landräte did not deter the far right from mobilizing, and that the appointment of outsider Landräte reestablished deterrence against far right mobilization.

6.4 Robustness

The central assumption underpinning a causal interpretation of the estimated effects of Landrat replacement is the parallel trends assumption. I contend that the parallel trends assumption is plausible for two reasons. First, in Appendix A, I implement the new generation of panel placebo tests developed by Liu, Wang, and Xu (2024, 166–171) to test for pre-trends. For the parallel trends assumption to be plausible, the average difference between the outcomes of later-treated units and the estimated control potential outcomes of later-treated units should be zero. The diagnostic tests presented in Appendix A provide reassurance in this regard. There is no evidence of a divergence in trends between treated and control units prior to treatment onset.

Second, I present evidence that the timings of Landrat replacements were primarily determined by non-strategic and plausibly exogenous factors. In other words, I argue that treatment timing can likely be considered "as good as random", which would imply parallel trends (Fadlon and Nielsen 2021; Roth and Sant'Anna 2023, 676). My argument is based on in-depth analysis of qualitative evidence on the mechanics of bureaucratic staffing. A strong qualitative understanding of how and when units are assigned to treatment is crucial to making causal claims with historical observational data (Kocher and Monteiro 2016).

The legal framework governing civil service appointments imposed significant constraints on the firing of unwanted bureaucrats. An unwanted Landrat could not simply be fired by the central government. Instead, officials who were relieved of their duties were made "idle", which meant that they remained on government payroll, receiving a significant proportion of their previous wage. Rules around idling disincentivized the replacement of bureaucrats, as more idled officials put pressure on the public purse (Runge 1965, 21–23, 57–59). A strong norm against the *proactive* idling of bureaucrats appears to have guided the central government in key personnel decisions.[23] Instead, the central government followed a *reactive* approach, with the appointment of new officials resulting from vacancies due to retirement or incapacitation.

To probe whether Landrat appointment was indeed reactive, I collected information on the birth years of Landräte who were in office prior to democratization in 1917.[24] Two pieces of

23. This norm appears to have influenced critical decision makers in the Prussian central government. Discussing the staffing of the bureaucracy in his memoir, long-time Prussian Prime Minister Otto Braun (SPD) notes with regret that democratic reformers "[...] too often paid attention to decorum and the financial situation and social standing of specific civil servants", preventing them from taking more decisive personnel decisions (Braun 1940, 42). In his biography of long-time Prussian Minister of the Interior Carl Severing (SPD), the historian Thomas Alexander (1992, 129) notes that Severing "revered" the Prussian bureaucracy, and that this sense of reverence prevented him from reforming the administrative apparatus more decisively. On top of financial considerations regarding idling, socialization into norms about appropriate interventions with bureaucratic careers thus seem to have influenced key decision-makers.

24. I collected this information from two sources: First, limited biographical information on Prussian bureaucrats is available in the *Acta Borussica*, a collection of primary sources on Prussian history published by the Berlin-Brandenburg Academy of Sciences and Humanities (2002). Additional information was taken from German Wikipedia. In total, I have information on the birth years of 85% of Landräte in office in 1917.

evidence would be suggestive that appointment followed a reactive approach, and that timing of Landrat replacement is governed by plausibly exogenous factors. First, I would expect that most Landräte were replaced at or around the retirement threshold of age 65 (Triepel 1921), and that replacement typically occurred at old age, when incapacitation is more likely. Second, I would expect that on average, later-treated counties were administered by older Landräte in 1917 than control counties.

Figure 10 shows how old Landräte in treated counties were when they were replaced by successor officials. I add two vertical lines: The first line denotes the typical retirement threshold at age 65. The second line gives average age of Landräte in 1917, which was $\mu_{1917} = 47.55$. Two features of this distribution are notable. First, there is a concentration of mass around the retirement age threshold of 65, suggesting that a large proportion of Landräte only departed once they reached the retirement threshold.[25] Second, almost all of the mass of the distribution lies above average age of Landräte in 1917. Average age at replacement $\mu_{replacement} = 57.23$ is much higher than average age of Landräte in 1917 $\mu_{1917} = 47.55$.[26] Replacements typically occurred when Landräte were at an advanced age.

Figure 10: Landräte Ages at Replacement, Treated Units

Figure 11 shows how old Landräte were in 1917, prior to democratization. I contrast the age

25. 65 was an advanced age for members of the social elite born in 19th-century Germany. Stelter, Croix, and Myrskylä (2021, 124) estimate that German university professors – that is, members of academic elites with a comparable social standing to administrative elites – born in the mid-19th century could expect to live for approximately another 35 years at age 30, with an expected age at death of 65.

26. A Welch Two-Sample t-test rejects the null hypothesis of $\mu_{1917} = \mu_{replacement}$ with $t = 10.16, p < 0.001$.

distribution in control counties where traditional elite Landräte remained in office throughout the Weimar period (in blue) with later-treated counties where traditional elite Landräte were replaced (in red). The two distributions clearly differ. On average, control counties were administered by younger Landräte ($\mu_{control} = 45.7$) than later-treated counties ($\mu_{treated} = 48.2$) before democratization. The difference in means is statistically significant ($\mu_{treated} - \mu_{control} = 2.5(1.1), p < 0.05$).[27]

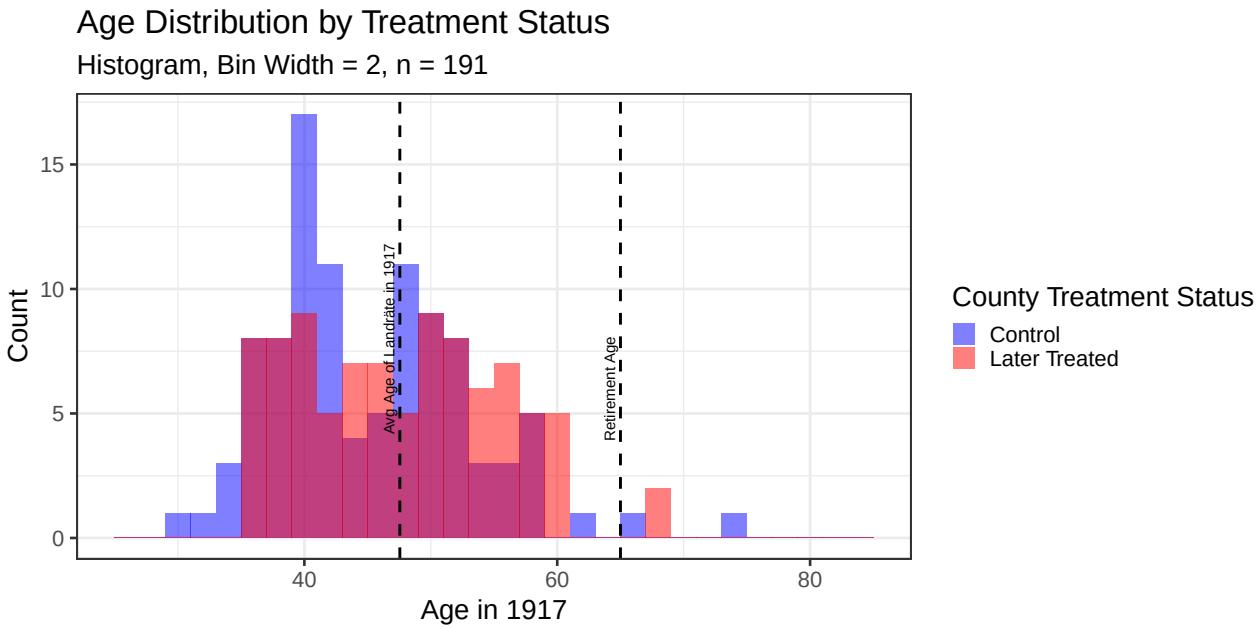

Figure 11: Landräte Ages Before Democratization, by Treatment Status

Taken together, this evidence corroborates my interpretation that the Prussian central government was reactive and primarily appointed new Landräte in response to naturally occurring vacancies, rather than following a *proactive* strategy of strategic appointment. The timing of these naturally occurring vacancies is plausibly as good as random, strengthening the case for a causal interpretation of my results.

6.5 Causal Mechanism

Quantitative evidence demonstrates that sidelining traditional elite Landräte improved democratic life in Weimar Germany. This section presents additional qualitative evidence documenting that traditional elite Landräte were hostile to pro-democratic actors, and used their authority and resources – often covertly, and sometimes overtly – to undermine democratic governance. This evidence draws on personnel files compiled by the Prussian Ministry of the Interior as well as secondary literature.

27. Difference in means estimated in a bivariate regression with robust standard error in parentheses.

The first case study comes from the rural counties of Zeven and Fallingbostel[28], located in the Western province of Hannover. The Landräte of both counties – Karl Freiherr von Hammerstein-Gesmold in Zeven and Hermann Rotberg in Fallingbostel – were appointed to their offices long before the transition to democracy in 1918. Their socio-economic background was typical for Imperial German administrative elites: von Hammerstein-Gesmold was born into Hanoverian nobility, and joined the ranks of the highly elitist, monarchist and often antisemitic fraternities[29] when studying for his law degree (Herlemann and Schatz 2004, 137–138). Rotberg similarly joined a fraternity while studying law in Munich and Marburg, and was appointed Landrat in Fallingbostel in 1909 (Herlemann and Schatz 2004, 307–308).

Both Landräte remained in office years after transition to democracy. In 1930, they were thrust into serious controversy when they vocally opposed state-wide restrictions on Hitler Youth mobilization efforts in Hanoverian schools. The Prussian government, determined to curb Nazi mobilization, relieved Hammerstein-Gesmold and Rotberg of their duties, rendering the two bureaucrats idle. Official correspondence revealed that the Ministry of the Interior viewed the behaviour of the Landräte as a staggering act of disloyalty, revealing deep-seated resentment against the democratic government and support for anti-democratic agitation.[30]

A closer look at the personnel files on Hermann Rotberg available in the Prussian Privy State Archives provides additional evidence of the anti-democratic resentment of the Landrat. In an effort to commemorate the end of Imperial rule and transition to democracy, democratic reformers created an annual constitutional holiday (*Verfassungsfeier*). Festivities featured speeches by leading state officials and prominent display of the national emblems of Weimar Germany. In rural counties, the Landräte were meant to make a prominent appearance (Jasper 1963, 229–240). Unsurprisingly, Hermann Rotberg had no inclination to celebrate a democracy he deeply resented – in letters to the Interior Ministry, he repeatedly asked for medical leave, proposing that his deputy commemorate the republic instead of him.[31] While perhaps unremarkable to his superiors at the time, in retrospect, Rotberg's behaviour appears calculated. To anyone trying to mobilize against democracy, Rotberg's repeated absence on the most important holiday of the Republic would have clearly signaled the Landrat's true preferences and loyalties. Tellingly, Rotberg – the youngest of the sanctioned officials – was reappointed Landrat immediately after the 1932 Preussenschlag, and served as a Landrat in various counties during the Nazi regime.

A second illustration of disloyalty in the ranks of traditional elite Landräte comes from the county of Regenwalde in Pomerania. The conflict between its longtime Landrat, Herbert von Bismarck, and the democratic reformers in the Prussian central government has been chronicled in detail by McElligott (2013, 169-179). A great-nephew of former chancellor Otto von Bismarck, Herbert embarked on a typical landed elite career in the Imperial Prussian

28. See Figure 2 for location of these counties.

29. For a history of fraternities in German-speaking Europe and their role in reactionary politics, see Heither et al. (1997).

30. See GStA I. HA Rep. 77 Nr. 5067, Bl. 85-112.

31. See GStA I. HA Rep. 77 Nr. 5067, Bl. 85-87.

administration. After studying law, he was appointed Landrat in Regenwalde county in 1918, just prior to democratization. While he stayed in office after democratization, his behavior in office suggests disdain for the democratic reformers he was supposed to be an agent of. Von Bismarck made sure that any official correspondence sent by his office on his behalf continued to be printed on paper adorned with the emblems of the German Empire, instead of the symbols of Weimar democracy. Like Hermann Rotberg, von Bismarck disregarded his official duty to commemorate democratization in the context of constitutional holidays. The symbolic defiance of the democratic order by von Bismarck was *overt* – the prominent use of monarchical emblems would have come to the attention of anyone conducting official business in the county. Von Bismarck's reactionary preferences – until then explained away by old habits dying hard – came to the fore again in 1931, when he defied the authority of the Prussian Ministry of the Interior and openly campaigned for a referendum designed to recall the democratically elected government and trigger snap elections. Although he was promptly removed from office, there can be little doubt that the citizens in his county were aware of his loyalties throughout the preceding decade.

The above examples illustrate how reactionary Landräte overtly signaled their rejection of the democratic order through acts of symbolic resistance. The case of Adolf von und zu Gilsa, Landrat in Kirchhain county in the province of Hesse-Nassau, shows that they occasionally went even further. Von und zu Gilsa was another typical representative of the class of Imperial administrative elites. Born into a family of Hessian nobility, he studied law in Göttingen and joined a local fraternity, prior to his appointment as Landrat in Kirchhain in 1911 (Hessische Biografie 2023). He was removed from office in 1928 after the Prussian government found that he had given tacit permission to the *Schwarze Reichswehr* – an infamous far-right paramilitary organization – to conduct military-style drills and exercises in Kirchhain county (Runge 1965, 147). Linked to reactionary elements within the Germany Army, the paramilitary organization was an important reservoir for recruitment to the SA, the SS, and the NSDAP (Sauer 2004).

These cases illustrate that traditional administrative elites held deep anti-democratic resentment, which did not however hinder many of them from remaining in their offices after democratization. Acts of resistance or micro-aggressions against symbols of the Weimar Republic were commonplace. Because of their overt nature, acts of symbolic resistance could signal to anti-democratic forces that a particular official was sympathetic to their cause, and that they could expect tacit or even overt support. In some cases, illustrated by the actions of Landrat von und zu Gilsa in Kirchhain county, officials went so far as to knowingly defy the law to allow paramilitary activity of violent anti-democratic actors.

7 Conclusion

"[...] How was it possible for Germany to fall into the Hitler Dictatorship? I can only answer: Versailles and Moscow."

– Otto Braun, Prime Minister of Prussia (SPD), *von Weimar zu Hitler*, 1940, p. 5.

In 1932, the last democratically elected Prussian government was illegally removed from office in the *Preussenschlag*, eliminating one of the last serious obstacles to the Nazi dictatorship. Fearing for the life of his bedridden wife and almost certainly his own, the previously all-powerful social-democratic Prime Minister of Prussia, Otto Braun, hastily fled to the seaside town of Ascona on Lago Maggiore in Switzerland, where he would spend the next two decades in exile. In his memoir, penned under the watchful eye of Swiss military censors and published in New York[32], Braun blamed the catastrophic decline of Weimar democracy on external actors: The victorious allied powers of WWI for imposing crippling war reparations which suffocated the German economy, and Stalin for lending relentless support to the most anti-democratic elements of the German communist movement.

Contemporary scholarship likewise attributes little democracy-saving agency to Weimar's democratic reformers. Analysts have pointed to century-old legacies of antisemitism (Voigtländer and Voth 2012), the radicalizing aftereffects of World War I (De Juan et al. 2024) or paralyzed fiscal and economic policymaking as a result of war reparations (Galofré-Vilà et al. 2021; Doerr et al. 2022) as drivers of democratic decline.

My paper challenges this fatalist perspective. A central feature of the post-WWI transition to democracy was that the democratizing coalition did far too little to dismantle a large, complex, and hostile administrative apparatus. Institutions insulating the civil service from political interference and accountability were kept intact. Rather than promoting good governance, the timidity of civil service reform perpetuated the dominance of a class of staunchly monarchist men, drawn disproportionately from landed nobility, over the state apparatus. The middle or working classes, Jewish Germans, or women remained marginalized from positions of power within the bureaucracy. Democratizers failed to use windows of opportunity – such as the failed Kapp Putsch or mass mobilization in the wake of the assassinations of Erzberger and Rathenau – for decisive reform of the state apparatus.

I zoom in on a particular facet of administrative reform – the replacement of traditional elite *Landräte* with outsider candidates in Prussian rural counties – to demonstrate its stabilizing effects for democratic governance. With armed party organizations threatening to derail elections and election campaigns with violence, the *Landrat* – principal law enforcement officer in his county – played a pivotal role in democratic life. Decisive law enforcement action to pacify election campaigns could limit the incidence of political violence and curb mobilization efforts of anti-democratic forces. On the flip side, the office gave reactionary law enforcement officials the opportunity to tolerate or even actively participate in the undermining of democratic governance.

32. For biographical detail on Otto Braun, see Wichers (2004).

Leveraging original data on law enforcement staffing and candidate entry combined with secondary data on voting and the incidence of political violence in a difference-in-differences design, I demonstrate that replacing a traditional elite Landrat with an outsider Landrat exerted democracy-stabilizing effects. Compared to control counties where law enforcement remained under control of traditional elites, counties in which an outsider *Landrat* was appointed subsequently saw (i) lower levels of lethal political violence, (ii) reduced entry by Nazi candidates for office, and (iii) higher vote shares for pro-democratic parties at the expense of anti-democratic parties, and in particular at the expense of the NSDAP. These results are consistent with qualitative accounts emphasizing how traditional elite law enforcement officials tolerated anti-democratic mobilization and violence in their counties, and underscore the democracy-stabilizing effects of purging anti-democratic bureaucrats.

Centering the lack of democratizing reform of the state apparatus does not imply that external constraints did not matter. As I point out in the paper, the strain on public finances accentuated by the punishing provisions of the Treaty of Versailles combined with the legal framework governing civil service appointments to render democratization of law enforcement more difficult. Had Versailles not put such pressure on the public purse, democratic reformers could have found it easier to sideline disloyal bureaucrats, even if doing so meant paying them for being idle. Nonetheless, a full accounting of the failure of Germany's first foray into modern democracy would be incomplete without noting the lack of serious reform of domestic administrative institutions. In addition to Versailles and Moscow, Berlin deserves a fair share of the blame.

My argument also raises new questions for Ziblatt's (2017) influential account of democratic erosion in Weimar Germany as a consequence of the weakness of Germany's main conservative party, the DNVP. The lack of support by German conservative elites for the DNVP – the pernicious consequences of which Ziblatt throws into sharp relief – may well be a downstream *consequence* of the fact that said elites retained important levers to influence the use of state power through their positions in the administrative apparatus. Conservative elites may not have invested in the development of a strong, electorally competitive conservative party because they did not have to, as bureaucratic insulation provided them with a more direct channel of influence.

In addition to shedding new light on the downfall of Weimar democracy, my paper holds general implications for scholarship on democratization, bureaucracy, and political violence. The creation of a politically insulated bureaucracy is often seen by scholars and practitioners as a bedrock "good governance" reform which limits patronage and corruption, improves public sector performance and thus catalyses development and democratization. My close inspection of the Weimar case reveals the dangers of insulated bureaucracies for democratization. When access to bureaucratic appointments is stratified and socially exclusionary educational institutions act as gatekeepers, insulating bureaucracies in effect means cementing the influence of traditional elites over the state apparatus. Limiting political influence over the bureaucracy then exerts destabilizing effects on democratization and development, rather than promoting

good governance.

My paper also underscores the critical role law enforcement plays in democratization. In polarized societies at risk of political violence, law enforcement plays a key role in negotiating tensions between the maintenance of public order and the safeguarding of political expression. Effective policing of political violence and violent, anti-democratic actors depends critically on the loyalty of law enforcement officials to the democratic order. For reformers inheriting complex coercive bureaucracies molded under autocratic rule, democratizing law enforcement presents a formidable and critical challenge.

References

Alexander, Thomas. 1992. *Carl Severing: Sozialdemokrat aus Westfalen mit preussischen Tugenden.* Bielefeld: Westfalen Verlag.

Andersen, David, and Jonathan Doucette. 2022. "State First? A Disaggregation and Empirical Interrogation." *British Journal of Political Science* 52, no. 1 (January): 408–415. ISSN: 0007-1234.

Berlin-Brandenburgische Akademie der Wissenschaften. 2002. *Acta Borussica: Die Protokolle des Preussischen Staatsministeriums 1817-1934/38.* Edited by Jürgen Kocka and Wolfgang Neugebauer. Hildesheim, Zürich, New York: Olms-Weidmann.

Biewer, Ludwig. 1983. "Der Preussenschlag vom 20. Juli 1932. Ursachen, Ereignisse, Folgen und Wertung." *Blätter für Deutsche Landesgeschichte* 119:159–172.

Birch, Sarah. 2020. *Electoral Violence, Corruption, and Political Order.* Princeton: Princeton University Press.

Birch, Sarah, Ursula E Daxecker, and Kristine Höglund. 2020. "Electoral Violence: An Introduction." *Journal of Peace Research* 57 (1): 3–14.

Bitter, Rudolf von. 1928. "Landrat." In *Handwörterbuch der Preussischen Verwaltung,* 2nd ed., 35–37. Berlin: de Gruyter.

Braun, Otto. 1940. *Von Weimar zu Hitler.* New York: Europa Verlag.

Caprettini, Bruno, Marcel Caesmann, Hans-Joachim Voth, and David Yanagizawa-Drott. 2022. "Going Viral: Protests and Polarization in 1932 Hamburg."

Carsten, Francis Ludwig. 1964. *Reichswehr und Politik, 1918-1933.* Köln: Kiepenheuer & Witsch.

Chiu, Albert, Xingchen Lan, Ziyi Liu, and Yiqing Xu. 2024. "What To Do (and Not to Do) with Causal Panel Analysis under Parallel Trends: Lessons from A Large Reanalysis Study." *SSRN Working Papers,* ISSN: 1556-5068. https://www.ssrn.com/abstract=4490035.

Cook, Scott J., and David Fortunato. 2023. "The Politics of Police Data: State Legislative Capacity and the Transparency of State and Substate Agencies." *American Political Science Review* 117, no. 1 (February): 280–295. ISSN: 0003-0554.

Cornell, Agnes, and Victor Lapuente. 2014. "Meritocratic administration and democratic stability." *Democratization* 21, no. 7 (November): 1286–1304. ISSN: 1351-0347.

D'Arcy, Michelle, and Marina Nistotskaya. 2017. "State First, Then Democracy: Using Cadastral Records to Explain Governmental Performance in Public Goods Provision." *Governance* 30, no. 2 (April): 193–209. ISSN: 0952-1895.

Dahlström, Carl, and Victor Lapuente. 2017. *Organizing Leviathan: Politicians, Bureaucrats, and the Making of Good Government.* Cambridge: Cambridge University Press.

Dahlström, Carl, Victor Lapuente, and Jan Teorell. 2012. "The Merit of Meritocratization." *Political Research Quarterly* 65, no. 3 (September): 656–668. ISSN: 1065-9129.

De Juan, Alexander, Felix Haass, Carlo Koos, Sascha Riaz, and Thomas Tichelbaecker. 2024. "War and Nationalism: How WW1 Battle Deaths Fueled Civilians' Support for the Nazi Party." *American Political Science Review* 118, no. 1 (February): 144–162. ISSN: 0003-0554.

De Juan, Alexander, and Jan H. Pierskalla. 2015. "Manpower to coerce and co-opt—State capacity and political violence in southern Sudan 2006–2010." *Conflict Management and Peace Science* 32, no. 2 (April): 175–199. ISSN: 0738-8942.

Doerr, Sebastian, Stefan Gissler, Jose-Luis Peydro, and Hans-Joachim Voth. 2022. "Financial Crises and Political Radicalization: How Failing Banks Paved Hitler's Path to Power." *The Journal of Finance* 77, no. 6 (December): 3339–3372. ISSN: 0022-1082.

Elsbach, Sebastian. 2019. *Das Reichsbanner Schwarz-Rot-Gold: Republikschutz und politische Gewalt in der Weimarer Republik.* Stuttgart: Franz Steiner Verlag.

Erger, Johannes. 1967. *Der Kapp-Lüttwitz-Putsch. Ein Beitrag zur deutschen Innenpolitik 1919/1920.* Düsseldorf: Droste Verlag.

Eschenburg, Theodor. 1954. *Die improvisierte Demokratie der Weimarer Republik.* Laupheim: U. Steiner Verlag.

Evans, Peter, and James E. Rauch. 1999. "Bureaucracy and Growth: A Cross-National Analysis of the Effects of "Weberian" State Structures on Economic Growth." *American Sociological Review* 64, no. 5 (October): 748. ISSN: 00031224.

Fadlon, Itzik, and Torben Heien Nielsen. 2021. "Family Labor Supply Responses to Severe Health Shocks: Evidence from Danish Administrative Records." *American Economic Journal: Applied Economics* 13, no. 3 (July): 1–30. ISSN: 1945-7782.

Falter, Jürgen W., and Dirk Hänisch. 1990. *Election and Social Data of the Districts and Municipalities of the German Empire from 1920 to 1933.*

Flom, Hernán. 2024. "The politics of police accountability: Police expulsions in Buenos Aires, Argentina." *Governance* 37, no. 2 (April): 453–473. ISSN: 0952-1895.

Galofré-Vilà, Gregori, Christopher M. Meissner, Martin McKee, and David Stuckler. 2021. "Austerity and the Rise of the Nazi Party." *The Journal of Economic History* 81, no. 1 (March): 81–113. ISSN: 0022-0507.

Gay, Peter. 1968. *Weimar Culture. The Outsider as Insider.* London, New York: W.W. Norton & Company Inc.

Geddes, Barbara. 1996. *Politician's Dilemma: Building State Capacity in Latin America.* Berkeley: University of California Press.

Gerschenkron, Alexander. 1946. *Bread and Democracy in Germany.* Ithaca, NY: Cornell University Press.

Hannover, Heinrich, and Elisabeth Hannover-Drück. 1966. *Politische Justiz, 1918-1933.* Frankfurt am Main, Hamburg: Fischer-Bücherei.

Heither, Dietrich, Michael Gehler, Alexandra Kurth, and Gerhard Schäfer. 1997. *Blut und Paukboden: Eine Geschichte der Burschenschaften.* Frankfurt am Main: Fischer.

Herlemann, Beatrix, and Helga Schatz. 2004. *Biographisches Lexikon niedersächsischer Parlamentarier 1919–1945.* Hannover: Verlag Hahnsche Buchhandlung.

Hessische Biografie. 2023. *Gilsa, Friedrich Wilhelm Adolf von und zu.* https://www.lagis-hessen.de/pnd/1102952966.

Hue de Grais, Robert. 1926. "Landrat, Kreis und Stadtausschuss." In *Handbuch der Verfassung und Verwaltung in Preussen und dem Deutschen Reiche,* 99–102. Berlin: Springer.

Jasper, Gotthard. 1963. *Der Schutz der Republik. Studien zur staatlichen Sicherung der Demokratie in der Weimarer Republik, 1922-1930.* Tübingen: J.C.B. Mohr.

Jones, Mark. 2016. *Founding Weimar: Violence and the German Revolution of 1918-1919.* Cambridge: Cambridge University Press.

Kocher, Matthew A., and Nuno P. Monteiro. 2016. "Lines of Demarcation: Causation, Design-Based Inference, and Historical Research." *Perspectives on Politics* 14, no. 4 (December): 952–975. ISSN: 1537-5927.

Lessmann, Peter. 1989. *Die preussische Schutzpolizei in der Weimarer Republik: Streifendienst und Strassenkampf.* Düsseldorf: Droste Verlag.

Linz, Juan J., and Alfred Stepan. 1996. *Problems of Democratic Transition and Consolidation.* Baltimore: Johns Hopkins University Press.

Liu, Licheng, Ye Wang, and Yiqing Xu. 2024. "A Practical Guide to Counterfactual Estimators for Causal Inference with Time-Series Cross-Sectional Data." *American Journal of Political Science* 68, no. 1 (January): 160–176. ISSN: 0092-5853.

Loewenstein, Karl. 1937. "Militant Democracy and Fundamental Rights, II." *American Political Science Review* 31, no. 4 (August): 638–658. ISSN: 0003-0554.

Luebbert, Gregory M. 1987. "Social Foundations of Political Order in Interwar Europe." *World Politics* 39 (4): 449–478.

Mares, Isabela. 2015. *From Open Secrets to Secret Voting. Democratic Electoral Reforms and Voter Autonomy.* Cambridge: Cambridge University Press.

Mazzuca, Sebastian L., and Gerardo L. Munck. 2014. "State or Democracy First? Alternative Perspectives on the State-Democracy Nexus." *Democratization* 21 (7): 1221–1243.

McElligott, Anthony. 2013. *Rethinking the Weimar Republic: Authority and Authoritarianism, 1916-1936.* London: Bloomsbury.

Mizuno, Nobuhiro, and Ryosuke Okazawa. 2025. "A dynamic theory on clientelism and bureaucratic development." *Journal of Development Economics* 174 (May): 103438. ISSN: 03043878.

Moore, Barrington. 1966. *Social origins of dictatorship and democracy: Lord and peasant in the making of the modern world.* Boston: Beacon Press.

MPIDR and CGG. 2011. *MPIDR Population History GIS Collection (partly based on Hubatsch and Klein 1975 ff.)* Accessed December 11, 2023. https://mosaic.ipums.org/historical-gis-datafiles.

Müller-Crepon, Carl, Philipp Hunziker, and Lars-Erik Cederman. 2021. "Roads to Rule, Roads to Rebel: Relational State Capacity and Conflict in Africa." *Journal of Conflict Resolution* 65, nos. 2-3 (February): 563–590. ISSN: 0022-0027.

Muncy, Lysbeth. 1970. *The Junker in the Prussian Administration.* Providence: Brown University Press.

Nalepa, Monika. 2022. *After Authoritarianism: Transitional Justice and Democratic Stability.* Cambridge: Cambridge University Press.

Nalepa, Monika, and Barbara Piotrowska. 2024. "Purging the Police in Nascent Democracies."

Pikart, Eberhard. 1958. "Preussische Beamtenpolitik 1918-1933." *Vierteljahreshefte für Zeitgeschichte* 6 (2): 1191–37.

Ribhegge, Wilhelm. 2007. *Preußen im Westen. Kampf um den Parlamentarismus in Rheinland und Westfalen.* Münster: Aschendorff Verlag.

Roth, Jonathan, and Pedro H. C. Sant'Anna. 2023. "Efficient Estimation for Staggered Rollout Designs." *Journal of Political Economy Microeconomics* 1, no. 4 (November): 669–709. ISSN: 2832-9368.

Runge, Wolfgang. 1965. *Politik und Beamtentum im Parteienstaat: die Demokratisierung der politischen Beamten in Preussen zwischen 1918 und 1933.* Stuttgart: Ernst Klett Verlag.

Rux, Johannes. 2002. "Direkte Demokratie in der Weimarer Republik." *Kritische Vierteljahresschrift für Gesetzgebung und Rechtswissenschaft* 85 (3): 273–297.

Sabrow, Martin. 1994. *Der Rathenaumord. Rekonstruktion einer Verschwörung gegen die Republik von Weimar.* München: R. Oldenbourg Verlag GmbH.

Sauer, Bernhard. 2004. *Schwarze Reichswehr und Fememorde.* Berlin: Metropol Verlag.

Schumann, Dirk. 2001. *Politische Gewalt in der Weimarer Republik 1918-1933. Kampf um die Strasse und Furcht vor dem Bürgerkrieg.* Essen: Klartext Verlag.

Seeberg, Merete Bech. 2021. "How State Capacity Helps Autocrats win Elections." *British Journal of Political Science* 51, no. 2 (April): 541–558. ISSN: 0007-1234.

Selb, Peter, and Simon Munzert. 2018. "Examining a Most Likely Case for Strong Campaign Effects: Hitler's Speeches and the Rise of the Nazi Party, 1927–1933." *American Political Science Review* 112, no. 4 (November): 1050–1066. ISSN: 0003-0554.

Shefter, Martin. 1977. "Party and Patronage: Germany, England, and Italy." *Politics & Society* 7 (4): 403–351.

Siemens, Daniel. 2017. *Stormtroopers: A New History of Hitler's Brownshirts.* New Haven: Yale University Press.

Stelter, Robert, David de la Croix, and Mikko Myrskylä. 2021. "Leaders and Laggards in Life Expectancy Among European Scholars From the Sixteenth to the Early Twentieth Century." *Demography* 58, no. 1 (February): 111–135. ISSN: 0070-3370.

Triepel, Heinrich. 1921. "Das preußische Gesetz über die Einführung einer Altersgrenze." *Archiv des öffentlichen Rechts* 40 (3): 349–377.

Voigtländer, Nico, and Hans-Joachim Voth. 2012. "Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany*." *The Quarterly Journal of Economics* 127, no. 3 (August): 1339–1392. ISSN: 0033-5533.

Volkov, Shulamit. 2012. *Walther Rathenau. Weimar's Fallen Statesman.* New Haven: Yale University Press.

Way, Lucan A. 2005. "Authoritarian State Building and the Sources of Regime Competitiveness in the Fourth Wave: The Cases of Belarus, Moldova, Russia, and Ukraine." *World Politics* 57, no. 2 (January): 231–261. ISSN: 0043-8871.

Weyland, Kurt. 2021. *Assault on Democracy. Communism, Fascism, and Authoritarianism During the Interwar Years.* Cambridge: Cambridge University Press.

Wichers, Hermann. 2004. *Otto Braun, Historisches Lexikon der Schweiz Online.* https://hls-dhs-dss.ch/de/articles/027967/2004-06-07/.

Wig, Tore, and Andreas Foro Tollefsen. 2016. "Local institutional quality and conflict violence in Africa." *Political Geography* 53:30–42.

Winkler, Heinrich August. 1988. *Der Schein der Normalität. Arbeiter und Arbeiterbewegung in der Weimarer Republik 1924 bis 1930.* Berlin/Bonn: Dietz Verlag.

Wunder, Bernd. 1977. "Die Rekrutierung der Beamtenschaft in Deutschland: Eine historische Betrachtung." *Leviathan* 5 (3): 360–377.

Ziblatt, Daniel. 2017. *Conservative Parties and the Birth of Democracy.* Cambridge: Cambridge University Press.

Supplementary Materials

A Diagnosing Pre-Trends

As is the case with all observational causal inference, the causal interpretation of the quantities estimated in this paper hinges on fundamentally untestable identification assumptions. In the analysis presented here, two crucial assumptions are that the model to impute counterfactual outcomes is correctly specified, and that in expectation, untreated potential outcomes of all units move in parallel. The last assumption is known as the parallel trends assumption Liu, Wang, and Xu (2024, 163–164).

We cannot provide a dispositive test of the validity of these assumptions, given the fundamental problem of causal inference. However, the plausibility of the assumptions can be evaluated by testing for a pre-trend in the data prior to treatment onset. If identification assumptions are plausible, on average, the difference between the outcomes of later-treated units and the estimated control potential outcomes of later-treated units should be zero. If the difference is not zero, we might have to worry about a pre-trend – a divergence in trends between treated and control units prior to treatment onset.

I implement a set of novel diagnostic tests proposed as a global test for pre-trends (Liu, Wang, and Xu 2024, 169–170). The first test is a joint F-test with the null hypothesis that average differences between outcomes and imputed control outcomes of later-treated units are zero in all pre-treatment periods (the average prediction error). Rejecting this null hypothesis (a small p-value for the F test) would suggest a possible violation of identification assumptions. In line with the recommendations in the literature, I complement this test with an equivalence test. The equivalence test tests against the null that average prediction errors in pre-treatment periods fall outside of a pre-specified equivalence range. Rejection of this null is evidence for equivalence, and *lends support* to identifying assumptions. Absent priors about relevant effect sizes, I use the default equivalence range proposed by Liu, Wang, and Xu (2024).

Figures 12, 13, 14, 15 and 16 display the results of this set of diagnostic tests. P-values for the F-test and the equivalence test are shown on the top left. Recall that larger p-values for the F-test and smaller p-values for the equivalence test indicate that identification assumptions are plausible.

None of these tests give rise to concerns about the plausibility of identifying assumptions. In all cases, the F test fails to reject the null that average prediction errors in any pre-treatment periods are different from zero. Reassuringly, the equivalence tests in all cases do reject the null hypothesis of inequivalence of average prediction errors. The upshot of this analysis is that there are no statistically detectable pre-trends in the data.

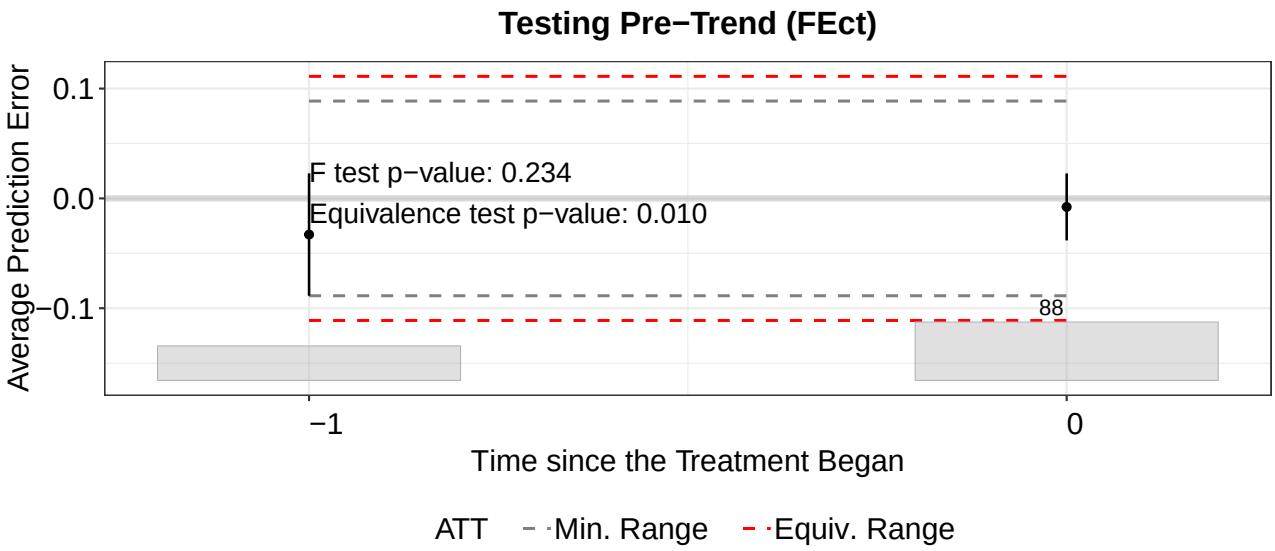

Figure 12: Diagnosing Pre-Trends, Candidate Entry, Weimar Coalition

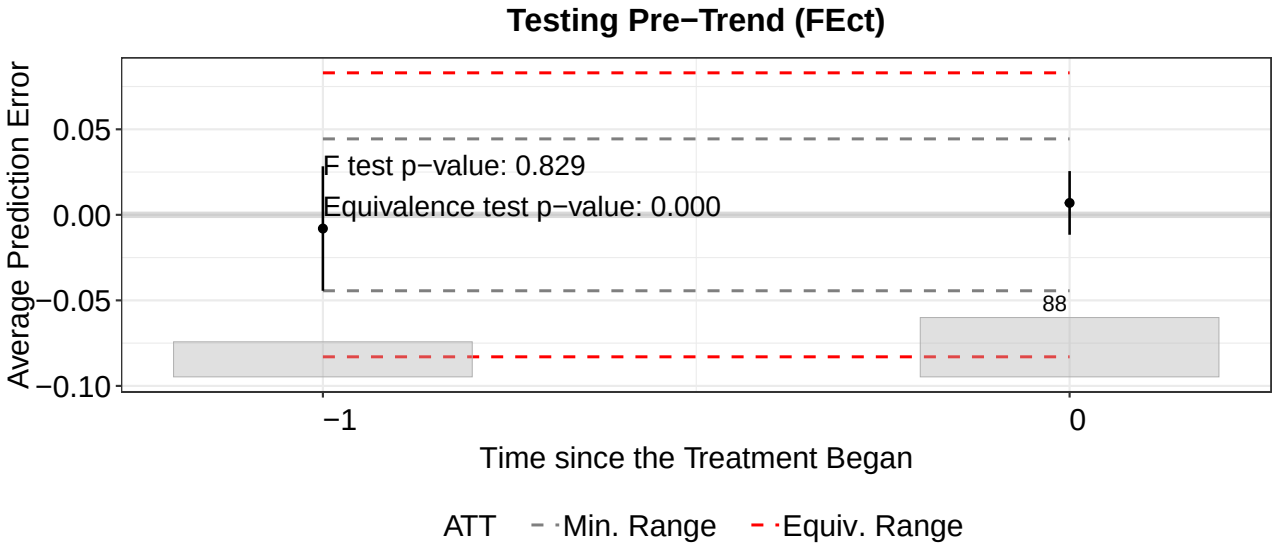

Figure 13: Diagnosing Pre-Trends, Candidate Entry, Nazi Party

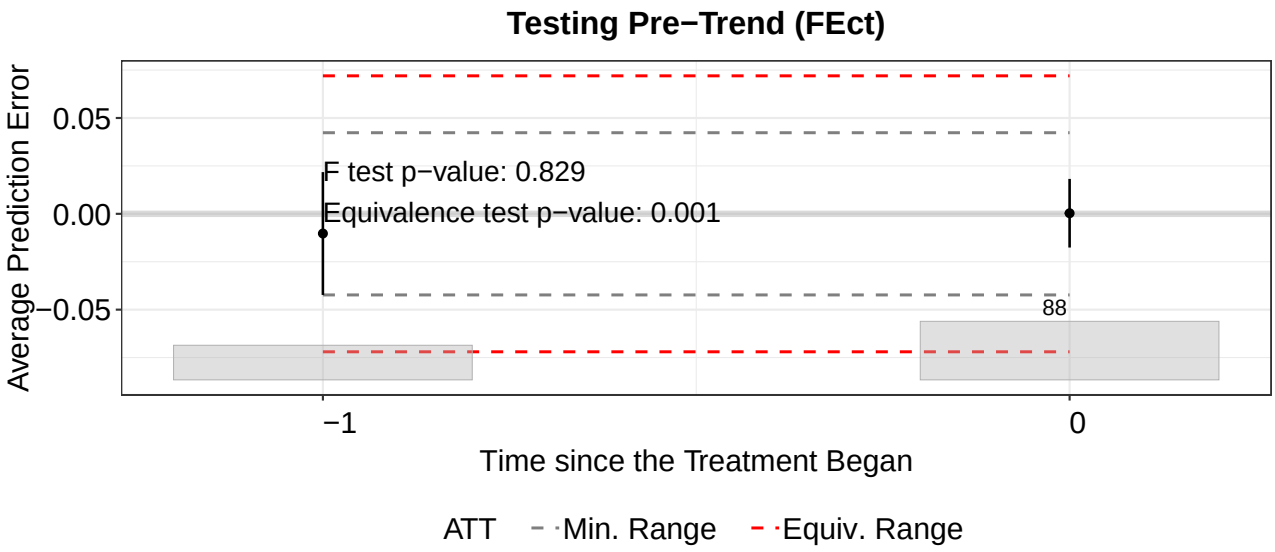

Figure 14: Diagnosing Pre-Trends, Candidate Entry, KPD

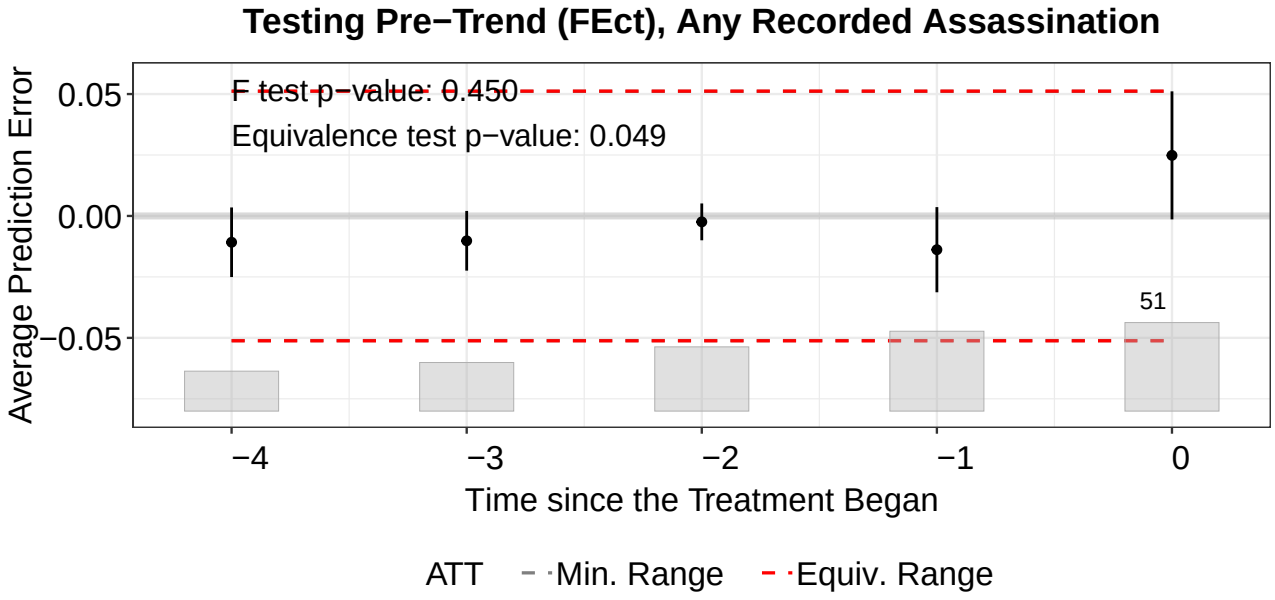

Figure 15: Diagnosing Pre-Trends, Political Assassinations

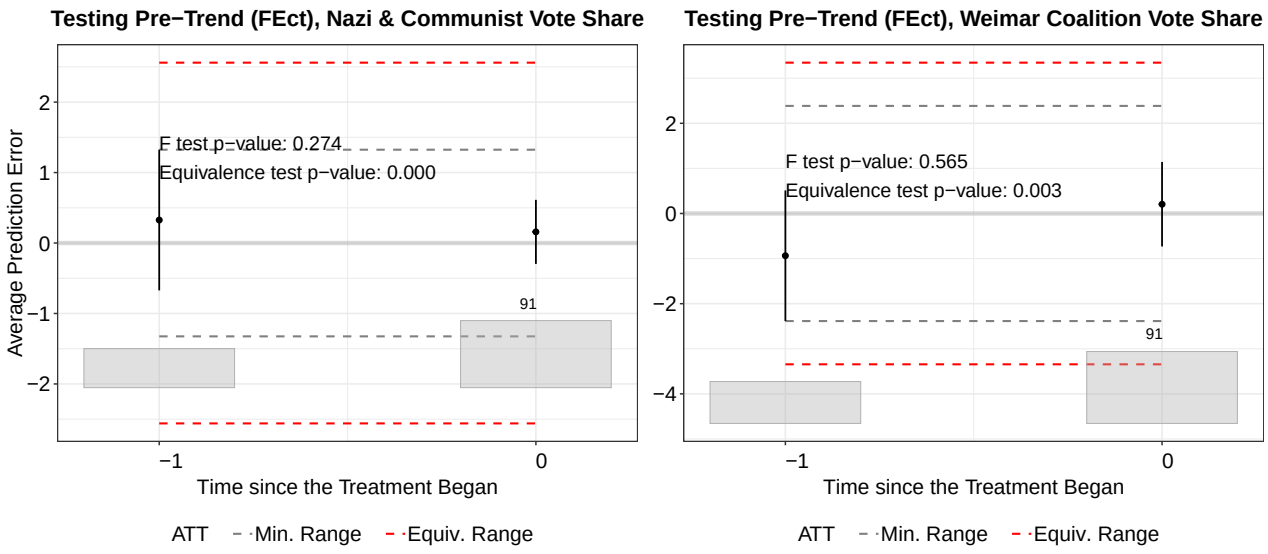

Figure 16: Diagnosing Pre-Trends, Electoral Outcomes

B Robustness Checks

B.1 Mobilization Effects

In estimating the mobilization effects of traditional elite Landrat replacement, I use the number of candidates entering from a particular party in a county adjusted by county population as the dependent variable. For example, I estimate the effect of the treatment on the number of Nazi candidates per 10,000 voters in a county. Population data come from Falter and Hänisch (1990).

Here, I show that the substantive results do not change when I do not adjust for county population. Table 7 presents the results. The treatment on average reduces entry by Nazi candidates, and has no statistically significant effect on entry by Weimar coalition or communist candidates.

B.2 Pacification Effects

In estimating the pacification effects of traditional elite Landrat replacement, I use a binary variable indicating any recorded assassination in a county-year as the dependent variable. A different approach would be to specify the number of individuals assassinated in a given county-year adjusted for county population as the dependent variable. To compute this variable, I use population data from Falter and Hänisch (1990). These data are available for federal election years, and I linearly interpolate county population data for non-election years in the panel.

Table 8 presents the results. Consistent with the main analysis, the treatment on average

	DV: Weimar Coalition candidates			*DV: Nazi candidates*			*DV: Communist candidates*		
	TWFE	FEct	FEct	TWFE	FEct	FEct	TWFE	FEct	FEct
Replace Traditional Elite	0.168	0.154	0.086	-0.162^{**}	-0.156	-0.184^{*}	0.032	0.037	0.054
	(0.103)	(0.116)	(0.106)	(0.079)	(0.096)	(0.102)	(0.072)	(0.075)	(0.079)
obs equally weighted		✓			✓			✓	
units equally weighted			✓			✓			✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
RMSE	0.281	0.544	0.544	0.221	0.367	0.367	0.199	0.419	0.419
N Clusters	393	202	202	393	202	202	393	202	202

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 7: Replacement of Traditional Elites and Entry, by Partisan Group, Unadjusted

	DV: Recorded Assassinations/10k voters		
	TWFE	FEct	FEct
		obs equally weighted	units equally weighted
Replace Traditional Elite	-0.012^{**}	-0.014^{*}	-0.015^{*}
	(0.006)	(0.008)	(0.008)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
RMSE	0.153	0.031	0.031
N Clusters	387	163	163

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 8: Replacement of Traditional Elites and Violence, Population-Adjusted

reduces the incidence of politically motivated violence.

C Turnout Effects

Here, I estimate the average effect of the treatment on aggregate turnout. Estimation follows the same approach as for all other effects. Table 9 displays the results.

Table 9 reveals that there is no statistically significant average effect of the treatment on aggregate turnout.

	DV: Aggregate Turnout		
	TWFE	FEct	FEct
Replace Traditional Elite	−0.363	0.329	0.114
	(0.487)	(0.472)	(0.534)
obs equally weighted		✓	
units equally weighted			✓
County FE	✓	✓	✓
Year FE	✓	✓	✓
RMSE	2.803	1.965	1.965
N Clusters	395	206	206

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TWFE estimator uses always-treated units, which FEct estimator discards. Hence, N Clusters is larger for TWFE.

CR2 (TWFE) and block bootstrap (FEct) standard errors clustered at the county level in parentheses.

Table 9: Replacement of Traditional Elites and Aggregate Turnout