THE COST OF STATE-BUILDING:
EVIDENCE FROM GERMANY

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ABSTRACT

I examine the potential of pro-development state (capacity) building projects to be coopted for repression. I leverage the natural experiment created by the differential build-up of capacity between formerly Prussian and formerly non-Prussian parts of unified Germany, and the radical policy shifts instigated by the Nazi regime. Across a geographical discontinuity, and across different stops of the same train transport to the East, I find that Prussian municipalities were significantly more efficient at deporting Germany's Jews. They were also better at providing public goods and at collecting taxes. Just before the Nazis came to power, Prussian municipalities also provided public goods more efficiently, but were not differentially involved with anti-Semitism. I show that democratic oversight and aspects of bureaucratic culture can mitigate the potential for future abuse of state building projects.

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1 Introduction

Based on a broad consensus about the beneficial effects of an effective government for development, policy makers have made 'state building' a key priority in poorer countries. For example, in recent years, close to one thousand World Bank projects explicitly aiming at state (capacity) building have been implemented around the world and the United States government initiated a 150-billion-dollar state building effort in Afghanistan. The benefits of state building projects are typically weighed against their costs of implementation. Yet capacity building has a potential future cost as well: As political objectives change governments may direct their capacity towards expropriation, or even repression. Stronger states built today may therefore be more repressive in the future than weaker states, and this may need to be considered when building state capacity. In this paper I study this idea by combining a natural experiment in past state building in Germany with data on (future) anti-Semitic repression, public good provision, and state capacity.[1]

This paper builds on two views on the effects of state building. The 'cautionary' view originates in anthropology and history and is close to the starting point of this paper. It states that societies, anticipating future abuse by 'strong' states, resist state (capacity) building (Clastres, 1978; Scott, 2010).[2] For example, historians have pointed to resistance to census-taking in Germany, citing the abuse of records in the Second World War, and in England, where the first census proposed in 1753 was delayed until 1801 due to concerns over future abuse. In recent years, Lebanon and Myanmar have suspended or postponed censuses for similar reasons.[3] This view shares with the motivation of this paper the idea that state building should proceed with caution. In economics, the closest statement of this view is a result of the theoretical analysis in Besley and Persson (2010) which states that future taxation by others may be a motivation for incumbents to not invest in state capacity today. The cautionary view contrasts with the 'optimistic' view. This view goes back to Weber (1920) and is more common in economics. It contends that stronger bureaucracies tend to act as a constraint on politician-instigated abuse. For example, mission-driven bureaucrats, or those with career concerns, will exert less effort in an organization that does not

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[1]The number of World Bank projects is taken from www.worldbank.org. I subsetted to projects in the theme "Public Sector Management" and subtheme "Data Development and Capacity Building". As of August 2025 this subtheme lists 897 projects. The figure for state building in Afghanistan comes from a report by the Special Inspector General for Afghanistan Reconstruction of the United States military and is accessible at https://www.sigar.mil/interactive-reports/what-we-need-to-learn/ (accessed April 2024). The consensus that effective government is good for development is built on the several findings. States with greater 'capacity' outperform their peers (Besley and Persson, 2014; Dell et al., 2018; Evans and Rauch, 1999). Introducing modern forms of government organization improves performance (Aneja and Xu, 2024) and setting rational incentives for individual bureaucrats improves performance as well (Besley et al., 2022; Hanna et al., 2017).

[2]Clastres (1978) documented that the prospect of abuse of power among native Amazonians led to the strategic choice to not build a state. Similar strategies have been reported for societies in North America (Lowie, 1948), Africa (Gulliver, 1971; Vansina, 1990), Europe (Thurston, 2016), and Asia (Scott, 2010).

[3]For Germany, see Frohman (2012); for England, see Taylor (1951); for the Lebanese census, see e.g. Maktabi (1999). On the Myanmar census see The Economist article 'Too much information', published March 28, 2014.

pursue its mission (Dewatripont et al., 1999; Wilson, 1989; Besley and Ghatak, 2005). In addition, they may be personally motivated by the welfare of citizens, who act as a second principal besides politicians (Prendergast, 2007). Finally, if directives are illegal, it is part of the 'Weberian' *definition* of a successful bureaucracy that impartial bureaucrats should drag their feet or refuse to implement them. The primary objective of this paper is to make progress in empirically distinguishing between these views.

To do so, I focus on Germany. I use the fact that Prussia unified Germany in 1871. The strong and bureaucratized Prussian state became the national German state, and I compare those parts of Germany that were formerly Prussian, to those that were not. Like most state building projects, Prussian 'capacity' was built over time and on along several dimensions, and therefore consists of a bundle of characteristics, each potentially exerting an effect after German unification. The historical evidence cited in the next section suggests that the stronger *central* government organization may manifest itself in differences in the capacity and organization of *local* government (Kiser and Schneider, 1994; Clark, 2006). At the level of the central state, Prussia's government was significantly more bureaucratized than its neighbors. After 1806, a decentralization program led to the development of an effective local bureaucracy. Other constituent states of Germany did not decentralize or were run feudally until the early twentieth century. After the National Socialist (Nazi) party took power in 1933, they directed the state towards repression of Germany's Jews, which after 1941 primarily occurred through deportations to ghettos and concentration camps in Poland and the Czech Republic, while simultaneously continuing regular public good provision. Although the physical transport of German Jews onto trains and out of Germany was enforced by the secret police (*Gestapo*), anecdotal evidence suggests that local *civil* government aided these efforts by recording and updating the whereabouts of local German Jews, by preparing deportee lists, and by organizing within city transports to the train stations from where the Gestapo transports left (Mecking, 2008; Spannenberger, 2022; Held, 2008). In this environment, having been Prussian may be directly associated with the success of the implementation of repression. I can therefore study the question of this paper through a historical natural experiment: By examining the effect of past investment in state capacity, and around transition to dictatorship in Germany, I study a capacity building project, and its potential future abuse.

Since exposure to Prussia varies discontinuously at its former borders, in my first empirical analysis I implement a geographical regression discontinuity design (RDD), comparing German municipalities that were Prussian before the unification of Germany to those that were part of other states, within 50 kilometers of Prussia's former borders. To study the effectiveness of Nazi repression, I use municipality-level data on deportations. These data are collected by modern historians, and their quality does not vary with Prussian status. Conditioning on the local German Jewish population in the census immediately

prior to the Second World War (WWII) in 1939, I find that deportations are implemented more effectively: Being formerly Prussian is associated with a 36% increase in the number of German Jews deported. This result is similarly strong when using the number of deported German Jews *per bureaucrat* as the dependent variable. These results are not affected by including covariates measuring the presence of other actors that were part of the organization of the deportations: Nazi party members in the local government, the presence of the *Gestapo*, and popular anti-Semitism. In addition, they hold in simple means, and do not depend on the regression discontinuity specification, or the choice of bandwidth.

To establish this approach as a valid empirical design, I pursue several strategies. First, I show that being Prussian is balanced with respect to several measures of the geographical spread of the German Jewish population, emigration, (cultural) anti-Semitism, and economic development before the founding of Prussia, as well as just before the onset of WWII. Second, I verify that migration rates are not different across the study boundary, and that differential migration out of parts of Germany where Jews were treated better during the Weimar Republic is not driving my main results. Third, I show that differences in policies implemented by local government are not driving the results. Fourth, I reconstruct the history of each of 53 territorial expansions that added land to Prussia, and reconstruct whether it was intentional, such as a conquest or a purchase, or unintentional, such as a ruler dying without an heir. The latter 'non-conquest' sample represents Prussia if all its historical expansions were unrelated to factors that may correlate with subsequent outcomes. I re-estimate the relationship between Prussian status and deportations restricting to 'non-conquest' Prussia and verify my results. Fifth, following Voigtländer and Voth (2012), I show that all results hold within a sample that did not experience an early pogrom.

My final strategy is to use fixed effects to account for all time-invariant differences between municipalities, deportation routes, and *individual* trains used for deportation. I collect new data to expand my dataset on deportations to not only include the number of deportees per municipality, but also the date of each individual deportation, and merge this panel dataset with new information on 425 train transports from Germany to the East. Within this dataset I implement two analyses. First, I implement a triple-differences design comparing municipalities that were visited by a train transport to those that were not, before and after the visit, inside and outside Prussia. I first verify that municipalities that will be visited are on parallel (pre-)trends relative to non-visited municipalities, both in Prussia and outside. I then show that, compared to non-visited municipalities, when visited, Prussian municipalities deport a larger fraction of the remaining German Jewish inhabitants than municipalities outside Prussia. Although the train schedules were set centrally, like before, local governments had a role in organizing deportations when the trains arrived. When a train did arrive, my results show, deportations proceeded more effectively in formerly Prussian municipalities. Second, I create a panel at the transport-stop level and compare stops

within Prussia to stops outside, adding fixed effects for routes (e.g. Nuremberg-Theresienstadt) and individual trains. Using a rich set of location controls and, crucially, train fixed effects, I show that *within* the same train, stops in Prussia deport local German Jews more effectively than stops outside. This last strategy holds fixed any aspect of the size of the train, its route, organization, and personnel.

The context of this study allows me to compare the effect of Prussia on deportations to other anti-Semitic harassment and violence before the Nazis took power in 1933. These range from smashing up storefronts, to public humiliation, to beatings. These incidents were primarily perpetrated by party thugs, such as the notorious SA (*Sturmabteilung*) 'brownshirts'. These groups were not municipal workers, nor were they aided or deterred in a systematic way by local government.[4] Using new data on 744 such incidents, I find no effect of Prussia on anti-Semitic harassment when the (local) government is not involved.

I then study mechanisms. I measure state capacity, following the literature, by *outcomes* and by features of the bureaucracy itself. Part of the Nazis' popularity came from the fact that they pursued extensive employment programs after the Great Depression's unemployment, which peaked in 1932, both through direct employment program, and through social security measures. Municipalities also continued ordinary public good provision. As my first measures of the effectiveness of local government, I collect new data on municipal unemployment program extension and public good provision before and after 1933 and test for an effect of Prussia in these datasets. Using my RD strategy, I find positive, significant, and economically meaningful effects on unemployment benefit extension, and on regular public good provision, such as road construction and trash collection. Crucially, because public good provision objectives did not change with the seizure of power by the Nazis, I find identical estimates before and after the Nazis came to power. This constant effect of having been Prussian contrasts with my findings above. Both before and after the Nazis came to power, public good provision was government policy. Repression of German Jews was only policy after. Second, I use administrative data on public administration to study the efficacy of tax collection, a standard measure of the capacity of the state (Besley and Persson, 2011b). I find that Prussian regions raise more tax (per capita), conditional on economic development. As before, I find the same result before the Nazis came to power. Third, I study organizational aspects of the local bureaucracy. I focus on the structure of local government (Dewatripont et al., 1999; Bandiera et al., 2021), and the characteristics of bureaucrats (Finan et al., 2017). To measure the structure of local government, I focus on a particular historically informed aspect of local government: Specialization. The historical literature suggests that Prussian local government was more decentralized, devolving more tasks and responsibility, potentially leading to greater *within*-municipality specialization and capacity (Dorn, 1931). Using newly

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[4]Gruner (2011) documents cases of municipal government enacting anti-Semitic legislation ahead of the national government. In contrast, Friedländer (2009) documents cases of municipal police trying to stop the gravest excesses.

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collected data on the structure of municipal governments I construct a Hirschmann-Herfindahl index of the concentration of bureaucrats across occupied job categories, conditional on the overall size of the local bureaucracy. I find that Prussian municipalities are significantly more specialized. In the Appendix I also test for differences in wages, total employment, and human capital. I find no evidence that employment or human capital are central mechanisms behind the effect of having been Prussian. I do find that Prussian municipalities use part of their higher tax revenues to pay higher wages. Taken together, I find evidence that Prussian municipal government is more effective, or has higher *capacity*, across outcomes and tax collection, and that differences in its organizational structure have persisted.

I then examine other potential mechanisms. I study cultural anti-Semitism (Voigtländer and Voth, 2012), social capital (Satyanath et al., 2017), radio ownership (Adena et al., 2015), religion (Spenkuch and Tillmann, 2018), as well as the potential differential impact of the Great Depression. I find that, *at the study boundary*, these measures balance suggesting that they vary smoothly over space. Although such factors may balance at the boundary, across all of Germany they may interact with local efforts of the bureaucracy. I study the interaction between each of these factors and having been Prussian across all of Germany and find that especially cultural anti-Semitism significantly increases the effect of having been Prussian, likely through increased popular support for, and collaboration with, the Nazi regime (Voigtländer and Voth, 2012).

These results are consistent with a cautionary view of state building: Prussian areas were more effective at policy implementation, before the Nazis came to power. When the objectives of the Nazi government included fighting unemployment and more generally public good provision, greater capacity to implement policy led to better outcomes. When the policy objectives included persecution and, ultimately, genocide, these objectives were executed more effectively as well, but only when the civil government was involved. One important mechanism for these results is the effectiveness or 'capacity' of local government.

The possibility of future abuse does not mean we should not pursue state building projects. The two views outlined at the start of this introduction are in opposition in theory, but in reality, there are probably conditions under which capacity building is 'future proof'. Such conditions naturally vary at the level of the country, but in the last part of this paper I study heterogeneous effects to identify some within-sample factors that may act as candidate conditions. I study factors external to the local government, and internal, organizational, factors. The most natural external candidate condition is electoral oversight or, more broadly, checks and balances (see e.g. Ferraz and Finan (2011); Casey et al. (2021); Garbiras-Díaz and Montenegro (2022)). Another candidate condition is hierarchy above the municipal level. In the German context, the fanaticism of the leading Nazis, the 'higher ups', in the Nazi hierarchy was often used as an

excuse for participation in the Holocaust. Within their respective local bureaucracies, Nazi perpetrators pointed to the effect of hierarchy as well, resulting in being given 'orders'. Finally, Hannah Arendt famously linked participation in the Holocaust to a more mundane bureaucratic culture of careerism and rigid assiduity (Arendt, 2006). I develop proxies for each of these factors. The Nazis abolished central and local elections when they took over, so I use the last national election, in which they were elected to power, and measure their vote share. I then, by using data on early deportations, measure how zealous district administrators were before the central state gave the deportation orders in 1941. Each municipality fell within such a district and the administrators acted as one of their principals. I measure hierarchy by the ratio of white to blue collar workers. I measure bureaucratic culture by the fraction of white-collar workers who became 'beamte', a tenured civil servant distinction, in part given as a reward for particular bureaucratic commitment. I find that my main results are concentrated in municipalities with weaker electoral oversight *against* deportations (i.e. with higher support for the Nazis), and in municipalities with more committed principals. Results are furthermore concentrated in more hierarchical municipalities, and in municipalities with a stronger bureaucratic culture. In other words, there are conditions that influence whether state capacity can be abused. Electoral oversight and leadership matter, as does bureaucratic culture.

The results in this paper suggest that state capacity investment, or state building, has a potential future cost. Politicians can tap into the civil service assigning tasks that are similar to those already executed but that directly support outright repression. There are countervailing forces, however, that mitigate the tendency of bureaucracies to simply become tools in the hands of politicians. The historical analysis in this paper therefore suggests that investments in capacity should be accompanied by a study of mechanisms that mitigate its future abuse. Such mechanisms are very well studied in relation to *politics* under the rubric 'checks and balances'. These are explicitly designed to prevent future abuse of office. Another way to view my results is therefore the extension of this logic to bureaucracy and state capacity. In addition, my results hold some interesting implications for the study of government more broadly. The Prussian bureaucracy had not developed capacity in deportations before. Instead, the capacity it had was fungible. The historical evidence in the next section suggests that fungibility is likely due to similarity of *tasks* across objectives, echoing a literature pointing out the centrality of tasks in the study of organizations (Autor, 2013). An important question concerns legality. The Nazis went through great lengths legalizing their repression (within Germany) before implementation. It is not clear whether bureaucrats would have acted the same, had directives not been legal. This question can be investigated further. In addition, a large literature going back to Arendt (2006) has linked more specialized organizations to greater abuse because specialization separates the individual contributor from the overall consequences of the organi-

zation's policies. This idea has received support in a lab setting (Bartling et al., 2014), and my results are consistent with it scaling up, suggesting a link between organizational structure and the implementation of potentially repugnant directives.

This paper aims to make three principal contributions. First, my results contribute to the literature on state capacity. The thrust of this literature has been to show, in various contexts, that more effective governments are good for development (Besley and Persson, 2010, 2011b; Dell et al., 2018; Acemoglu et al., 2015). This 'macro' literature has developed alongside a literature that studies individual incentives and policy changes that foster greater government effectiveness (Dal Bó et al., 2013; Khan et al., 2016; Finan et al., 2017; Xu, 2018; Khan et al., 2019; Colonnelli et al., 2020). I contribute to this literature by studying one case in which an effective state built in the past was abused by a new set of policy makers. These results build a bridge between the literature on state capacity and a large literature in anthropology and history pointing to 'resisting' state building as a strategy to prevent such future abuse (Clastres, 1978; Scott, 2010). In addition, my results relate to the theoretical literature on state capacity in which one result is that the anticipation of future taxation is a determinant of underinvestment in state capacity (Besley and Persson, 2010, 2011b).[5] My results can be viewed as consistent with this idea, although I cannot speak to the intentional weakening of the state. Second, the result that the future effect of an intervention may depend on supporting conditions is in line with a literature that points out the adverse consequences of well-intentioned policies. For example, Nunn and Qian (2014) show that food aid leads to civil conflict if it can be siphoned off. See also Nunn (2019). In line with this literature, my finding that state building can be abused, and the conditions under which this is more likely to happen is potentially informative for state building efforts today, especially in countries with weaker electoral oversight. Third, through studying deportations, I contribute to the literature on the organization of repression by pointing to the role of the civil bureaucracy, rather than rebels, militias, or the army (Besley and Persson, 2011a; Blattman and Miguel, 2010). In related work, I have provided evidence that the duration of having a centralized state leads to the buildup of cultural norms of obedience, and these norms affect development positively or negatively depending on government policy (Heldring, 2021). Due to the setting of this paper, my results complement a series of papers that document the rise and success of the Nazi party and the origins of anti-Semitism in Germany (Voigtländer and Voth, 2012; Satyanath et al., 2017; Adena et al., 2015; Spenkuch and Tillmann, 2018; Buggle et al., 2020). More of Germany's Jews emigrated from Germany than were ultimately deported. Buggle et al. (2020) study the decision of Germany's Jews to emigrate, and I discuss emigration in the next section, as well is in the balance section. My results make a separate contribution

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[5]In political science, some authors such as Christensen and Laitin (2019) qualitatively emphasize future repression, as in this paper, but do not provide an empirical approach or test.

in the forensic debate on the organization of the Holocaust. The roles of the Nazi party, its paramilitary arms (SA, SS), and the Gestapo are uncontroversial. However, the role of the civil government is debated. My results make progress in this debate by pointing to a significant role of the civil government in the implementation of the Holocaust, thereby contributing evidence in favor of the notion of 'desk killers' having a role in the Holocaust.[6] Finally, my results add to the study of the economy and society of Prussia (Becker and Hornung, 2020; Becker and Cinnirella, 2020; Hornung, 2014; Cinnirella and Streb, 2017).

The remainder of this paper is organized as follows. Section 2 provides the relevant historical and institutional background for this study. Section 3 describes the data used in this paper. Section 4 introduces the estimation framework which establishes the validity of the use of a geographical regression discontinuity before presenting the main regression model estimated in this paper. Section 5 presents regression discontinuity evidence. Section 6 presents results estimated in a municipality or transport-stop panel. Section 7 studies state capacity as mechanism and section 8 studies alternative mechanisms. Section 9 studies the conditions under which state capacity can be abused. Section 10 concludes.

2   Setting and context

This section introduces three aspects of Prussian and German history that are relevant for this paper. First, I discuss the history of the formation of Prussia. Second, I discuss the consensus in the historical literature on its 'capacity' at the national and local level. Third, I discuss the organization of the deportation of the German Jews by the Nazis between 1933 and 1945. Figure 1 provides timelines of the main events described in this section.

2.1   Prussia, and other polities

The political entity that became known as Prussia had its origins in the accession to the throne of Brandenburg - around modern Berlin - by the Hohenzollern family in 1415.[7] In 1618 the Hohenzollerns inherited the Duchy of Prussia, which was historically centered on Königsberg (modern Kaliningrad), and their territories were known from then on as Brandenburg-Prussia. From 1701, when the Hohenzollerns were allied with victorious France in the War of the Spanish Succession, Brandenburg-Prussia was elevated to the status of a kingdom and renamed as the Kingdom of Prussia. In 1871, after a victory over

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[6]For contributions claiming the civil - local - government was not involved in the organization of the Holocaust, see Mommsen (1966) and Matzerath (1970). Gruner (2011) points to the role of the municipalities in the marginalization of the German Jews, but does not implicate them in the organization of the Holocaust. Others, such as Mecking (2008), provide anecdotal evidence that the municipalities were involved. The continuity between the pre-Nazi and Nazi civil government point has been made in the historical literature by, among others, Hilberg (1961).

[7]The Hohenzollern family was the ruling family, as kings of Prussia and as emperors of the German Empire, after German unification in 1871.

France in the Franco-Prussian War, Prussia unified all of Germany into the German Empire. The king of Prussia became the German Emperor. Prussia itself, now a state within unified Germany, was the most influential of the constituent states of the new Empire, having the majority of territory and people, as well as the plurality of seats in federal governing council, the *Bundesrat*. After the defeat of the German Empire in the First World War, the emperor was ousted, and the Empire formally dissolved into the Weimar Republic. Prussia remained a state within the Weimar Republic, but lost independent policy making power in 1932 in the *Preussenschlag*, the 'coup in Prussia'.

At the time of German unification, Prussia was one of the most powerful states in Europe. There is complete agreement among historians that Prussia at this point was a highly functional and bureaucratized state (Clark, 2006; Kiser and Schneider, 1994). This consensus motivates the hypothesis that historical exposure to Prussia generates variation within unified Germany in the strength of the local state. In Appendix section 1 and in footnote 8 I provide more detail on the history of the development of the *central* state. A key aspect of the empirical part of this paper is to compare localities within unified Germany, and I therefore now discuss the history of the development of *local* government, inside and outside Prussia.[8]

2.2 The development of local administration, in Prussia and elsewhere

The key to understanding the development of local government is the demise of feudalism. Until about 1800, local government was essentially feudal, and centralization of the central state was achieved by chipping away at some of the privileges of the landed elite, mostly in military recruitment and some aspects of taxation. Local government was predominantly informal, unsalaried and tied to the local lord of the manor or other feudal overlord. After its defeat to Napoleon, Prussia started reforming local government. Other states reformed too, albeit in a different way and less thoroughly.[9] In Prussia, reform started with the 1808 Stein-Hardenberg reforms which abolished feudalism, rationalized central administration, and decentralized town administration. These reforms set the outline of local government that survived

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[8]As for the *central* government, its centralization and professionalization preceded that of local government. It started after the Thirty Years War, 1618-1648. The subsequent state building project essentially revolved around chipping away at the power of the feudal estates and centralizing the state around the king and the government in Berlin. By 1680 Prussia had a large standing army, achieved substantial territorial expansion, and even established a small colony in West Africa. By 1713, administration of royal possessions and tax collection were centralized into the 'General Finance Directory'. Initially, day-to-day tax collection was still run along provincial, rather than functional, lines, but around 1800 functionally defined ministries were made responsible for revenue. Universal conscription was introduced in 1714. At this time, Prussia could raise the fourth largest army in Europe, even though it was tenth and thirteenth in terms of territory and population (Clark, 2006, p. 98). By 1786, Prussia was still thirteenth in population and tenth in area but had third largest army. 5.8 million people sustained a professional army of 195,000 (Clark, 2006, p. 215). In 1794, the Prussian constitution and law code was introduced. The defeat to Napoleon in 1806 prompted further reform. Prussia abolished serfdom and reformed public education. High fiscal capacity and resulting military power led to Prussia's victory over Austria in 1866, over France in 1870 and its ability to force the rest of Germany into accepting its leadership in the German Empire in 1871.

[9]This section is based on several books and articles on local self-government (*Kommunale Selbstverwaltung*). For Prussia, see Clark (2006); Kiser and Schneider (1994); Goodnow (1889, 1890); Bornhak (1893) and Hintze (1901). For the comparative perspective between Prussia and other states, see Treffer (1996). For the reforms in the central government that facilitated the local government reforms, see Dorn (1931, 1932a,b).

until the Nazi period: A mayor, town council, police force, and the right and obligation to raise local taxes.[10] Several other states, especially those directly occupied by Napoleon, enacted reforms too.[11] Yet other states, like Mecklenburg, were essentially run feudally until the early twentieth century. For those that reformed, the nominal reforms were similar in spirit, involving civil local government, rather than a feudal structure. After unification, the main reform is the so-called Erzbergersche Reform of 1919/1920 which set the structure of taxation that was in effect during the Nazi period.

However, one recurring key difference in the historical literature between Prussian local government and local government in other states is that the decentralization process had been more sustained there (Nolte, 1990). This is directly visible in the law itself. In Prussia, the 1872 Kreis- und Provinzialordnungen set a large degree of freedom for local government leaving it to their initiative to enact policies that would advance their development or implement the tasks they were given, such as taxation and public order. This had been so since the first reforms in 1808 which established the "unlimited power through their election to represent the citizens in all matters relating to the community of the city, to take care of all community affairs for them, and to make binding declarations on behalf of the city and the citizens regarding the common assets, rights and liabilities of the city". Such full decentralization stands in sharp contrast to other states. In Bavaria for example, the 1808 municipal edict states: "Without ... approval ... neither they nor their representatives can ... not hire any staff; and not pass any valid community resolutions at all."[12]

The consensus among historians of German local government is that Prussia had centralized at the national level more so than other states, which resulted in military superiority leading to German unification. One consequence of the centralization at the top was that, at the local level, the Prussian state pushed decentralization further, potentially leading to differences in the capacity of the state at a local level. These observations form the motivation of using the unification of Germany and the discontinuous exposure to Prussian government in the empirical part of this paper. Before studying the role of local government in deportations, I discuss Prussia's expansion.

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[10]The key piece of legislation here is "Ordnung für sämtliche Städte der Preußischen Monarchie" that went into force in 1808. It was later expanded and extended to all rural municipalities. The subsequent pivotal pieces of legislation are 1831 Preußische Städteordnung which expanded the 1808 reforms, the 1850 Gemeindeordnung which harmonized and expanded municipal tasks, the 1853 Landgemeindeordnung which extended self-administration to all rural communities, and the 1872 Kreis- und Provinzialordnungen which set the set of tasks municipal government would follow until the Nazi period.

[11]For example, Bavaria enacted similar local government legislation for self-government as part of its 1818 Gemeindeedikt and its 1869 Bezirks- und Gemeindeordnung. Baden enacted a Gemeindeordnung in 1831 and Württemberg did the same in 1825.

[12]These passages are translated by the author from Treffer (1996, p. 254).

2.3 Prussia's outer borders

The outer border of Prussia as it stood in 1871 forms the basis for my empirical strategy. Prussia achieved its outer borders over time by the extension of its territory with several smaller polities. I map the outer borders of Prussia in 1871 in Figure 2, and I map each constituent polity in the Appendix, section 4. What is important is that Prussia had acquired most of its 1871 constituent states by 1806, when decentralization started.[13] Several territories were bequeathed to Prussia through strategic marriage. Others were purchased or simply conquered, like Silesia in 1742. However, an important aspect of Prussia's growth into its final extent was that expansions did not always occur intentionally. Some were driven by factors outside Prussia's control, such as unexpected deaths of local rulers without male heirs which bequeathed land to Prussia. Take the history of the territory of *Cleve*. In 1614, Cleve's ruler died without a male heir. Normally, feudal law would dictate that the territory passes to the feudal overlord. However, Cleve had obtained an exception from the Holy Roman Emperor to bequeath through the female line. The daughter of the ruler was married to the heir of the core area of what was then known as the Duchy of Prussia. There was a contest of power within Cleve but, in the end, Cleve went to Prussia because Brandenburg - the German territory that would be the heartland of the Kingdom of Prussia later - acquired the Duchy of Prussia, the polity the Cleve heiress had married into. The Brandenburg rulers did not set out to acquire Cleve. Furthermore, Cleve is about four hundred kilometers away from Brandenburg, and many polities lay in between the two territories. I use the motivation of each expansion as part of the empirical strategy of this paper. Therefore, for every new territory that became Prussian before 1871, I reconstruct its borders and record whether the expansion was intentional or not. For some expansions, like conquests, this is straightforward. For others, it requires detailing its specific history, like for Cleve. Appendix sections 4-5 detail every expansion, and whether I interpret it as accidental, or as intentional.

2.4 Local government and the repression and deportation of the German Jews

At the time of the 1933 census there lived about 520,000 Jews in Germany. By the time of the 1939 census, this number stood around 320,000. At the end of WWII, there were about 19,000 German Jews left in Germany, and the 1946 census counted about 26,000 (Blau, 1950).[14] The racialized politics of the Nazi regime changed the definitions of what it meant to be Jewish, distinguishing between Jews by 'blood' and Jews by religion. In particular, Germans with two or one Jewish grandparents were considered *Mischlinge*, a term for 'mixed ancestry' the Nazis favored, and were typically not persecuted; and not all people who

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[13]The largest state not yet incorporated was Hanover (German: *Hannover*), which became Prussian in 1866. Results are similar removing Hanover (available upon request).

[14]Note that figures for 1933 include parts of now-Poland with Germany. By 1946 this is no longer the case. Because in Poland, the Nazis killed essentially all Jews, the remaining German Jewish population are all within Germany.

were classified as Jewish were practicing Jews. I return to what this means for accurately measuring the effectiveness of deportations below.

Emigration and deportation were the two primary ways through which the decline in German Jewish population came about. About 150,000 German Jews were deported, and about 350,000 emigrated. Emigration took place between 1933 and 1943, with peak years in 1938 and 1939 and went on in small numbers after 1941.

Harassment, 1930-1933. After the Nazi party's initial election participation in 1928, party thugs, the *Sturmabteilung* (SA), engaged in various violent activities against German Jewish people and property. These included beatings, public humiliations, and defacing and destroying Jewish-owned shops (Friedländer, 2009). While initially shying away from mass violence, there were at least 744 reported incidents of such violence before the Nazis took power in 1933 (see the data section below for a discussion of these data).

Harassment and Emigration, 1933-1941. After taking power, the Nazi government legalized and implemented a systematic marginalization of Germany's Jews. The Nuremberg laws of 1935 stripped Jews of German citizenship, and removed them from the routine protection of law enforcement. Until the start of the War, German Jews from were removed from public office and from public life, now mostly by the SS (the Nazi party paramilitary arm, or *Schutzstaffel*) and the secret police of the Nazi government (Gestapo, or *Geheime Staats Polizei*) which had superseded the less bureaucratized SA. Finally, the impoverished Jews were 'relocated' into 'Jewish houses' (*Judenhäuser*), and systematically expropriated. Some were deported to concentration camps within Germany. As a result of this repression, the majority of German Jews emigrated until migration was officially banned in 1941. Buggle et al. (2020) study the decision of German Jews to migrate during this period.

Deportations, 1933-1945. Between 1933 and 1941, and often on local initiative, small deportations took place to concentration camps and ghettoes until the decision was made to *systematically* deport all Jews from Germany in 1941 (Gruner, 2004). The organization of the transports and deportations was headed by the central government, and in particular (from 1939), the Reich Main Security Office (the RHSA, or *Reichssicherheitshauptamt*). Adolf Eichmann led one of the responsible departments for the deportations within the RSHA. At the local level, the Gestapo was responsible for rounding up and transporting Jewish citizens.[15] Now I will discuss that, when a deportation was decided upon, municipal government would not only supply information, but would actively participate in the organization of the transport.

Deportations were implemented by the Gestapo, the Nazi party, and the local municipal government,

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[15]This formal responsibility has led an earlier generation of historians to contend that the local civil administration was not meaningfully involved in the deportations of German Jews. The local mayors and town council members, as well as their bureaucrats, were thought to be relatively impartial implementers of the law (Mommsen, 1966; Matzerath, 1970).

supported by denunciations by the local population. In Münster for example: "The deportation of the Westphalian Jews to Riga in December 1941 was organized during a top-secret conference in Münster on November 19, 1941. The conference was primarily concerned with the distribution of "vacated" dwellings, the liquidation and utilization of property left behind, and assembling the city's remaining Jews. Mayor (*Oberbürgermeister*) of Münster Albert Hillebrand, Senior Legal Counselor (*Rechtsrat*) Wilhelm Sasse, the director of the regional tax office, and representatives of the Party regional leadership (*Gauleitung*) and the Gestapo attended the conference." (Mecking, 2008, pp. 478-479).

When the Gestapo moved in for a transport, the municipality was responsible for bringing Jews to a central place, often near the train station. The Gestapo would typically set a quota of the number of Jews to deliver for a particular transport, and would request a list of names from the local representatives of the Jewish community, the Jewish council (*Judenrat*) to fulfill this quota.[16] While the quotas were set to fill up transportation capacity, often there would be too few or too many deportees, and all major cities were visited by several transports. An informative case study from Leipzig shows what would happen next (Held, 2008, pp. 15-20). The local police were informed of a transport passing through on January 9th, 1942. On January 17th, the Mayor gathered the head of the local police, the heads of the finance and labor offices, and four members of the public administration of the municipality. The administration made a school building with a gymnastics hall available as a rounding up point for the local Jews. When this building was destroyed by a bombing raid, it switched to the municipal labor office, and made the local tram system available for transport to this office. The heads of housing and finance were at this meeting because, having been responsible for expropriation and re-housing, they were the most informed about the local Jewish community's whereabouts. When the transport started, the Gestapo took the list prepared by the Jewish council to the municipal government, and the government would add names that they thought were missing and remove names of people it thought to be deceased or otherwise not available for transport.[17]

Spannenberger (2022) discusses a remarkable paper trail he discovered in *Marburg* that traces the deportation of German Jews step by step. He discusses that in Marburg, too, "The relationship between the municipal administration and the NSDAP offices, especially the municipal leadership, was predominantly characterized by cooperation..." (Spannenberger, 2022, p. 138; translated).[18] What was distinctive about this transport is that preparations for the deportations and transport to the trains appear to have been entirely organized and implemented by the local government. On the 28th of August, 1942 the *Lan-*

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[16]The Jewish councils were mandated by the Nazi government to have a direct means of communication between the regime and the Jewish communities. Hilberg (1961) first pointed out the role of the councils in the implementation of the Holocaust.

[17]Sometimes, the municipal labor office would intervene and remove names because they wanted to use Jewish forced labor.

[18]I have translated the German word Kreis, a local administrative unit, here as municipality. I will discuss local administrative units in the next section.

drat, a local administrator, wrote to the mayor that the Gestapo had ordered a deportation for September 6, 1942, and outlined regulations on luggage deportees were allowed to take. He also asks for municipal transport for 'immobile' Jews to the train station. On September 7th, 1942, he wrote to the Gestapo in Kassel, the responsible office, that the deportation had been successfully finished.

The administrative cooperation between Nazi party, the Gestapo, and local government, was far from perfect. Miscoordination between the municipal government and the Gestapo sometimes led to German Jews' survival: In September 1942, the Gestapo in Leipzig assumed that the Leopold family had committed suicide as they did not show up for transport, whereas they had gone into hiding. Despite transports in May, July, and September 1942 which reduced the number of Jews in Leipzig to about 500 from about 6,000 before the start of the deportation, the Gestapo did not meet its goal to deport all Jews. In Magdeburg, a local German Jew called Gerry Levy was summoned to the Gestapo headquarters after a pogrom in 1938 for deportation to Buchenwald concentration camp. He delayed, and although he did report to the Gestapo, the train for Buchenwald had left. He was held in a regular prison for three weeks and released (Abrahams-Sprod, 2007, pp. 259-260). In yet other cases, the informational advantage of the local municipal government relative to the Gestapo played out explicitly in favor the German Jews. Friedrich Lehmann, who as head of the finance department of the municipal government of Frankfurt had been an integral part of the Nazi local government, went out of his way to get Jews removed from transport and even hid Erna Buttermilch, who was Jewish, in his house in 1944 (Stemmler, 2020). These case studies illustrate the role of the key actors in the deportation process: The Gestapo, the municipal government, the local Nazi party representatives. In the empirical section of this paper, I measure the presence of each of these actors. In addition, I measure the fanaticism of the local population which was often involved in denunciations of German Jews to the authorities (Mallmann and Paul, 1994; Voigtländer and Voth, 2012).

Deportations may have been more successful if the German Jews could more easily be administratively identified, would be effectively tracked, and already moved to the Jewish houses. Therefore, more effective local government may have aided the implementation of the deportations of the Gestapo.[19,20] This section has given an overview of the historical and institutional background behind the hypothesis of this paper. Prussia was the origin of the German bureaucratized state, and I hypothesize that govern-

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[19]This hypothesis is not in line with the consensus in the historical literature, which remains split between local studies pointing to the role of the municipal government in deportations (Held, 2008; Abrahams-Sprod, 2007) whereas others claim that the municipal governments were only involved in the repression of Jews, from the appropriation of their assets, to re-housing, to removal from public life, but that the deportations were done by the Gestapo (Gruner, 2011). The empirical part of this paper can be thought of as one way to make progress in this debate.

[20]In this footnote I briefly review policy towards German Jews just before the accession to power of the Nazis, during the Weimar period. The Weimar period was marked by relative openness towards the Jewish population in Germany. Many Jews were in higher ranking positions in the government and in business (Niewyk, 2018; Brenner, 1998). Jews were overrepresented in science as well, and their subsequent emigration to the United States spurred innovation there (Moser et al., 2014). This is not to say that anti-Semitism did not exist, but government and judiciary formally protected the rights of the Jews and prosecuted anti-Semitic acts.

ment policy after German unification is more effectively implemented in former Prussian regions. The next section presents the data that I use to test this hypothesis.

3   Data

In this section I describe the main variables used in the empirical part of this paper. I report summary statistics for all variables used in this paper in Appendix Tables A1-A3 and I include a complete description of data sources in Appendix section 8.

The unit of observation in this paper is the *Kreis*, which roughly translates as county. Counties oftentimes coincided with other units, such as municipalities (*Gemeinde*) or cities. Cities and their hinterlands were furthermore often split, so that Magdeburg *Stadt* and Magdeburg *Land* are two separate units of observation. I will refer to my unit of observation throughout as a municipality to capture its various forms. I obtain digital maps of these municipalities from the Max Planck Institute for Demographic Research (MPIDR, 2011). Several outcomes vary at the level of a small town or a city, and I attribute these to the municipality the city is in. This exercise is relatively straightforward since, like for Magdeburg, most cities are a single municipality.[21]

The expansion of Prussia. My main treatment is an indicator equal to one if a municipality is within the outer borders of Prussia as of 1871. To obtain the final boundaries of Prussia, I reconstruct the expansion of Prussia from Fix (1860), Wolff (1877), Koebler (2007) and Nüssli (2008). Appendix section 4 describes this process and discusses each individual expansion in detail. Figure 2 shows Prussia, modern Germany, and the discontinuity created by the former Prussian boundaries within Germany. It also displays the municipalities as of 1930. In the previous section, I discussed that most local government had been developed in the nineteenth century. Duration exposure to Prussia may therefore not matter before 1800, but might after. I return to this point after I present my main results.

The Nazi period. The main outcome variable for this paper is a measure of the deportations of German Jews by the Nazis between 1941 and 1945. These data come from German Federal Archives (*Bundesarchiv*), which collected background information on 174,997 deported Jews.[22] Importantly, this source includes the location where a deportee was deported from, and the date of deportation. After subsetting

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[21] I modify this map in one important way. From April 1st, 1937, the Greater Hamburg Act effectuated the annexation to Hamburg three Prussian cities — Altona, Wandsbek, and Harburg-Wilhelmsburg — along with various rural communities nearby. This change resulted in the centralized administration of all deportations in this larger area from the Hamburg city. For regressions that include post-37 data, I unify the 1930 units that constitute then-Hamburg. For regressions that do not, I use the 1930 map of municipalities. The number of municipalities in all of Germany falls from 1009 to 1006 when I implement this change. To harmonize data from the 1925 and 1933 censuses with the 1930 territorial boundaries, I had to account for mergers and splits of several municipalities. I describe these cases, and the process I followed for changes in administrative borders between data sources in the Appendix, section 2.

[22] These data can be accessed here: https://www.bundesarchiv.de/gedenkbuch/. Current as of May 2025.

to deportees from Germany with the requisite information I obtain a dataset of 119,479 deportees between 1941 and 1944. From this dataset, I compute the total number of German Jews deported per municipality.[23] For some deportees, the data record a different place of residence than of deportation. This occurs if someone moved or was moved to a nearby larger town and deported only significantly later. I assign every deportee to their location of final deportation and measure, for each municipality, the number of deportees that were not previously resident.[24] I use that measure as a covariate, and I vary the measurement of pre-deportation movements in Appendix Table A5. I augment this dataset with one further new data source. I reconstruct each individual train transport that deported German Jews from Germany to the East from *Yad Vashem*, Israel's Holocaust Remembrance Center. I measure whether and when a municipality was either directly visited by a transport or whether (and when) German Jews were transported to a train station visited by a transport. My final dataset records the number of deportees from a municipality to the East and whether a municipality was visited by a train transport, by month of each year of Nazi rule. For my regression discontinuity analyses I further aggregate these data into a cross-section to obtain the number of deportees per municipality after deportation became official policy in 1941.[25]

The focal mechanism in this paper is the capacity of municipal government. To measure state capacity, and to understand if state capacity is higher in Prussian parts of Germany, I collected administrative data for towns and cities from the Statistical Yearbooks of German Cities. These data include measures of public good provision, taxation, and expenditure. I use several additional covariates and as well as outcomes of interest in the main results and mechanisms sections. I introduce these variables I use as they become relevant in the analysis.

The Weimar period. Before the Nazis took over, Germany's government was referred to as the Weimar republic. I construct my main outcome variable for this period, violence against Jews before the Nazis won the 1933 election from the Jewish Museum of Berlin's project the 'Topography of Violence'. For this project, researchers collected mentions of anti-Semitic harassment and violence from newspapers and Jewish community publications. In total, they record 744 events before the Nazis took power in 1933. I sum these incidents to create a count variable of violence against Jews. In Appendix Table A24 I study different definitions of incidents and different perpetrators. I also collect data on public good provision

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[23]These data have previously been used by Voigtländer and Voth (2012). Their reconstruction of this data does not include information on timing of deportations, and they use a different unit of observation. Because I use the date of individual deportations in various analyses, I reconstructed this dataset from its source.

[24]There are 40,694 individuals who were deported from a different location than their location of residence and for whom we have both pieces of information. Of these, 19,750 were deported from outside Germany, due to intermediate camps or because they had emigrated in the meantime. The remaining 20,944 were instead moved within Germany. The primary reason for this is that in some cases the Nazis moved large groups of people from one city to another within Germany. For example, Düsseldorf shows 4,291 Jews moved from various nearby large cities like Essen, Krefeld, or Wuppertal, held there for a period of time, and then collected and moved together to their final destination camp

[25]For both samples, I finally add the number of emigres from each municipality. These data are originally collected by Buggle et al. (2023).

and the functioning of local government, again from the Statistical Yearbooks of German Cities.

Several data sources, such as the reconstruction of the individual train transports matched to deportations, the reconstruction of pre-Nazi anti-Semitic events, the reconstruction of the presence of the Gestapo across municipalities, the measurement of local public goods and state capacity, and the detailed reconstruction of process identifying who was Jewish (see below) are, as far as I am aware, data contributions of this paper.

4   Estimation framework

In this section I introduce the estimation framework of this study. I discuss my econometric model, before discussing challenges to identification. I first estimate a cross-sectional regression discontinuity model. I then estimate a triple differences model, and a fixed effects model, which I will introduce below.

4.1   Estimating equation

I exploit the discontinuous change in historical exposure to the Prussian bureaucratized state in latitude-longitude space created by German unification in 1871. Some municipalities had been Prussian before German unification and some had not, and I capture the hypothesized treatment effect of this difference. To do so, I estimate the following cross-sectional model:

$$Y_m = \alpha + \beta Prussian_m + f(location_m) + \sum_{i=1}^{3} segment_m^i + \sum_{i=1}^{6} largecity_m^i + X_m\gamma_x' + \epsilon_m \tag{1}$$

Y_m is an outcome of interest for municipality m. I estimate my main results using outcome data measured during the Nazi regime, but for some outcomes I collect pre-Nazi measures as well. In these cases, I estimate separate models for the two periods; using 1930 municipalities as a common geographical unit. α is a constant, and $Prussia_m$ is an indicator variable equal to one if municipality m was part of Prussia at the of German unification in 1871. $\hat{\beta}$ is the coefficient of interest, the measured effect of being historically Prussian on outcome Y. $f(location_m)$ is a function of location, controlling smoothly for the position of municipality m in latitude-longitude space. Following Dell et al. (2018) I include a linear polynomial in latitude and longitude in all regressions. I vary this specification in Appendix Table A8. $\sum_{i=1}^{3} segment_m^i$ is a vector of boundary segment fixed effects. In all specifications, I include three fixed effects, one for units along the central boundary separating Prussia from southern Germany, indicated with the number 1 in Figure 2, one for units in the south, and one for units in the north, indicated respectively by the numbers 2 and 3 in Figure 2. These fixed effects ensure that I am comparing Prussian municipalities to their

neighbors across the relevant boundary segment. $\sum_{i=1}^{6} largecity_m^i$ is a vector of city fixed effects. I restrict these to the three largest cities within and the three largest outside the baseline bandwidth.[26] The reason to include separate fixed effects for these cities is that they were major outliers in terms of population in 1939. I refer to these covariates as my 'baseline' covariates. X_m is a vector of additional covariates. It includes the total number of non resident deportees, and measures of economic development, the share of population employed in industry and the share employed in services in the 1933 census. This vector also includes several measures of the presence of and support for the Nazi party, which I will introduce with the relevant regression. I vary the inclusion of all covariates after my main analysis and show that the main result is robust to excluding covariates. ϵ_m is a heteroskedasticity robust error term. In the main sample, I include all municipalities within 50 kilometers from the study boundary (n=525). I map these in Figure 2 as well. Given the two-dimensional running variable, rather than computing an optimal bandwidth, in Figure 3 and Appendix Table A6 I show robustness to varying the bandwidth between 30 and 70 kilometers in steps of five kilometers for my main results.[27] I estimate equation 1 using OLS. In order for me to interpret the estimates of β in equation 1 as causal, several assumptions need to be met. The most important is that all relevant variables, pre-treatment, besides Prussian status and any potential mechanisms driving the effect of having been Prussian, vary smoothly at the boundary. To study this assumption, I follow several strategies. First, I report results from a balance test. Second, in Appendix Table A9 I show that the estimated coefficient of interest $\hat{\beta}$ is very similar with and without including covariates, lending further credence to the requirement that besides Prussian status, (un)observables vary smoothly. Finally, I test for sorting across the boundary below.

Balance. Table 2 studies balance, using observables from the late medieval/early modern period, before the establishment of Prussia as a political unit. The objective of this exercise is to show that municipalities that will become Prussian are, on average, not different from those that will not. To assess balance on development, I rely on city size from McEvedy and Jones (1978). I merge data on cities to my dataset of municipalities and remove all municipalities that cannot be merged. The variable of interest is the Prussian indicator, $Prussian_m$. If bigger cities are concentrated in areas that will become Prussian in the future, then it may be that (a correlate of) development varies non-smoothly at the RD boundary. In column (1) of Table 2, I use total population in the year 1600 as dependent variable (in 1000s). In column (2) I use city growth between 1500 and 1600. Both point estimates are small and insignificant. In columns (3) and (4) I follow Voigtländer and Voth (2012) and study medieval Jewish life and persecution.

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[26]These are Frankfurt, Hannover and Magdeburg inside Prussia, and Hamburg, Dresden and Leipzig outside. Berlin and Munich are outside the main estimation sample.

[27]There is no consensus on optimal bandwidth for two-dimensional running variables. Contributions have focused on mean square errors or confidence intervals, but typically for the one-dimensional case. See Cattaneo and Titiunik (2022) for an overview of bandwidth choice methods. I estimate several of their suggested models in Appendix Table A8.

The authors show that Black Death pogroms are correlated with early twentieth century anti-Semitism. It may be the case that future Prussian areas are more anti-Semitic or even that Prussian expansion explicitly targeted more anti-Semitic parts of Germany. The sample in these columns is the regular sample of German municipalities. Column (3) uses as outcome variable an indicator equal to one if a municipality had a Jewish community in 1349. Column (4) uses an indicator equal to one if a municipality experienced a pogrom during the Black Death of 1349.[28] Across both outcomes, I find small and insignificant effects of future Prussian status.

In column (5) and (6) I study geography. In column (5) I use ruggedness of the landscape as the dependent variable and in column (6) I use an indicator equal to one if there is a river close by. The idea of these columns is to study whether the placement of the border is in part determined by features of the landscape that may correlate with subsequent outcomes. As before, I find small and insignificant point estimates. Finally, in columns (7) and (8) I study politics. Germany was fragmented before the establishment of Prussia but some cities were granted 'free city' status by the Holy Roman Emperor or joined the Hanseatic league, a trade organization. I find that future Prussian municipalities are not differentially likely to contain cities that were 'free' or a member of the Hanseatic league. I take the last four outcome variables from Voigtländer and Voth (2012). These results suggest that, before the establishment of Prussia as a state, places that would become Prussian do not look different from places that would not.

A very important concern is the history of Jewish life and anti-Semitism in Germany. The results in Table 2 suggest that medieval anti-Semitism is not correlated with Prussian status (locally around the discontinuity). I now test for differences in Jewish life and anti-Semitism before the deportations started. I report results in Table 3. The idea of a successful regression discontinuity design is that important (un)observables vary smoothly over the boundary while treatment, and any mechanisms sustaining treatment, do not. In columns (1), (2), and (3) I use the German Jewish population in a municipality in 1925, 1933, and 1939, the pre-War census years, as dependent variables. In columns (3) and (4) I use total emigration 1933-39 and 1939-40, emigration was banned in 1941, as the dependent variables. I then test for two more aspects of German Jewish life. In the previous section I discussed that the identification of who was Jewish as a major concern for the Nazi government. In addition to being religiously Jewish, Nazi ideology prescribed that someone not practicing could be 'ethnically' Jewish. A 1935 decree established that someone with three or more Jewish grandparents was Jewish, as was someone with Jewish parents. This created the notion of a 'Mischling'; someone with either two Jewish grandparents (a so-called Mis-

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[28]I use the Black Death pogroms because they align in time with the measurement of the presence of a Jewish communities I use in column (3). However, Voigtländer and Voth (2012) provide measures of several other medieval pogroms, as well a a measure of whether a Jewish community was destroyed as part of a Black Death pogrom. I can use either the sum of all pogroms that occurred in a municipality or an indicator for whether a community was destroyed as dependent variable in this balance test, and I find similar balance (results available upon request).

chling first degree) or one (second degree). I record the number of Mischlinge in both degrees and use the resulting variables in column (6) and (7). These data have not been systematically used before in social science and I hand-digitized them from the 1939 census returns. I then use data on anti-Semitism before the founding of the Weimar republic. In column (8) I use the presence of *Judensau* sculptures from Voigtländer and Voth (2012). These are demeaning sculptures that, in some cases, survived until the twentieth century, having been created in previous centuries. I then use, in column (9), the number of pogroms that took place as part of the 'Hep-hep' riots in 1819, also from Voigtländer and Voth (2012). In columns (10)-(13) I use the logarithm of the vote shares for the Nazi party in all elections they obtained more than five percent of the total vote. Point estimates in columns (1)-(5) of Table 3 are standardized. I find small and insignificant coefficients in each column. For example, in 1939, the German Jewish population is 0.06 standard deviation higher in Prussia (s.e. 0.08) and the vote share of the Nazi party in the last election they stood in, in 1933, is one percentage points higher in Prussia (s.e. 0.03) compared to a mean vote share of 48%.[29]

It is impossible to test for balance on all potential (un)observables. A standard approach to assessing the potential impact of unobservables is to measure the extent to which including covariates moves an estimated coefficient of interest. In a regression discontinuity setup, (un)observables that do not vary smoothly are candidates to move observed regression discontinuity coefficients, and violate its identifying assumption. I show below that removing an extensive set of covariates do not move estimated effect sizes, providing more assurance that (un)observables vary smoothly over the boundary.

Sorting. A further challenge to identification in RD designs is differential sorting across the study boundary. The first thing to note is that overall migration is low. Less than 10% of the people in a sample of municipalities for which I have migration data have migrated. In Appendix Table A4 I directly test for differential migration by estimating versions of equation 1 using data on migration from the Weimar and Nazi periods as dependent variables. Columns (3) and (6) use net migration (immigration - emigration) as the dependent variable and columns (1) and (2) and (4) and (5) use total immigration and total emigration as dependent variables. Across all regressions, there is no effect of being Prussian. In Appendix Table A11, I also show that my main results are robust to controlling for changes in the number of German Jews between the censuses of 1925, 1933, and 1939. While there may perhaps be compositional effects, there is no difference in the intensity of migration between Prussian and non-Prussian regions.[30]

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[29]For the vote share columns, the number of observations varies slightly due to changes in municipal boundaries. I describe municipal boundary changes in the Appendix, section 2.

[30]As a final check, I run a McCrary (2008) test, using distance to the RD boundary as the running variable. The McCrary (2008) test for density heaping in the running variable is close to the cut-off, which may suggest sorting. In my setting, this may occur through people moving across the boundary to settle inside (or outside) Prussia. Using straight-line distance to the study boundary, I reject the hypothesis of sorting (p=0.005).

The results in this section lend credence to the claim that the discontinuity created by the unification of Germany meets the assumptions for a regression discontinuity analysis. Since it is never possible to test for balance on all potentially important factors, I implement an alternative research design below that exploits time variation, and has different identifying assumptions. Before reporting my main results, I now provide graphical intuition for the relationship between Prussian status and deportations. I report summary statistics for the main variables used in my analysis in Table 1.

5 Results: Deportations: Regression Discontinuity

In this section I present the main results of this paper. I find that deportations of German Jews are more effectively implemented in formerly Prussian municipalities.

Graphical intuition. Before presenting the main regression results, I build intuition using the raw data in Figure 1, Panel IIIa. I subset to the 50KM bandwidth sample, and measure cumulative German Jewish population in 1939 on the horizontal axis, and cumulative deportations between 1941-45 on the vertical axis. I plot the cumulative number of deported German Jews for levels of the pre-War population, separately by Prussian and non-Prussian municipalities. Note that both lines are away from the parity line.[31] The vertical difference mainly reflects emigration between 1939, when the population data are measured, and 1941, when large-scale deportations started. The main result of this figure is that the cumulative deportations for the Prussian side of the boundary lie above the deportations for non-Prussian areas, for virtually every level of 1939 German Jewish population. I now study this finding by providing estimates of equation 1.

Main results. Table 4 reports estimates of equation 1 using two main dependent variables. In column (1) and (2), I use the natural logarithm of the number of deportations as the dependent variable. In columns (3) and (4), I normalize total deportations by the number of bureaucrats employed in a municipality when the Nazis came to power in 1933, before taking logs. Furthermore, I include several covariates in addition to the baseline covariates.

In the background section, I noted that there were several actors that have been documented to have been involved in deportations besides local government: The local Nazi party, the population and most importantly, the Gestapo. I use several covariates to capture their presence. To capture the presence of the Nazi party, I record the number of Nazi party members that are employed as bureaucrats in the local government or are active in local politics. I record these variables from a dataset of Nazi party members from Falter and Brustein (2015). The authors created a representative 2% sample of all Nazi

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[31]Since I plot two subsamples which together add up to the full sample, this is not the 45-degree line. I plot a line at 22.5 degrees.

party members. Importantly, these data record the year of membership of each party member as well as their profession. Using this information, I can count, within the limits of their sample, the number of Nazi party members in local politics and local government in 1939, before the start of the deportations. To capture popular anti-Semitism, I control for the vote share for Nazi party in 1930, the first election in which they attained a significant electoral foothold.[32] To capture the presence of the Gestapo, I collected archival data on the number of Gestapo offices in a municipality.[33] To account for the pre-War distribution of German Jews, I control for the number of German Jews present in a municipality in the last pre-War census, which was held in 1939. Note that the measures of the presence of Nazi party members, Nazi party votes, and the presence of German Jews are all pre-determined with respect to the start of large-scale deportations in 1941. The presence of the Gestapo is not. I therefore include all covariates except the measures of Gestapo activity in column (1) and add Gestapo activity in column (2).

Row 1 of Table 4 contains the estimated effects of being formerly Prussian. In column (1) I find a positive significant effect of being Prussian on the natural log of the number of deported German Jews between 1941 and 1945 (conditional on the number of German Jews present in 1939). Consider the point estimate in column (1), row 1, 0.37 (s.e. 0.10). Being formerly Prussian is, at a local level, with a 37% increase in the number of German Jews deported. In the countryside where the Prussian borders were located, the number of German Jews was small. The 37% difference corresponds to a predicted difference in levels of 38 deportees (88 outside Prussia, 126 inside Prussia).[34] To interpret these levels it's important to understand that the majority of German Jews lived in Berlin, Munich, Hamburg, and other big cities, which are either outside the bandwidth or are captured by fixed effects for large cities. This estimate therefore focuses on smaller towns and rural municipalities around the Prussian border. In column (2) I add the number of Gestapo offices and an indicator for having been visited by a Gestapo transport. The point estimate in row 1 is stable at 0.37.[35] In regressions that use the natural logarithm of a dependent variable that contains observations with zero values, the interpretation of economic significance is sometimes challenging (Chen and Roth, 2024). In Appendix Table A19, I report the extensive and intensive margin of the effect of having been Prussian on deportations, and verify the effect size to be around 31% on the intensive margin. In column (3) I directly study the effectiveness of the bureaucracy in this process by normalizing total deportations between 1941 and 1945 by the number of bureaucrats in a municipality

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[32]In Appendix Table A10, I control for the vote share of the Nazi party in other election years. Results are similar.

[33]The data source for the offices is the Gestapo records held in the Arolsen archives. See the Appendix, section 8, for full data documentation.

[34]37% is an approximation. The exact percentage change is $e^{0.37} - 1 = 44.7\%$. I compute level differences holding all covariates at their sample means.

[35]An interesting challenge to this result is the observation that after the War, there were only about 20,000 German Jews left in Germany. This could be inconsistent with a difference in deportations in levels. I discuss this in the next subsection, 'achieving policy objectives'.

when the Nazis came to power in 1933. I find that deportations per bureaucrat (x100) are sixteen percent higher. This result, too, is statistically significant and similar when controlling for the presence of the Gestapo (column (4)). In column (5), I make the comparison between the Nazi period and the Weimar republic which existed until the Nazi's rise to power in 1933. I use natural logarithm of the number of anti-Semitic violent events between 1930 and 1933. In total, I measure 744 such events. Conditional on votes for the Nazi party as a measure of the anti-Semitism of the local population, I find no impact of being Prussian on anti-Semitic violence before the Nazis came to power and made persecution of German Jews official government policy.

The identifying assumption of an RD design is that all factors besides treatment vary smoothly over the boundary. To further assess this assumption, in Figure 1, Panel IIIb, I report predicted means of the log of deportations with and without all covariates from column (1). The predicted means with and without covariates are very similar. To assess whether the main result is due to the boundary, or holds more broadly, I also report means using all of Germany as my sample. The first two bars therefore simply show a difference in means by Prussian status across all of Germany. Comparing to the same sample with covariates shows very similar means, just as adding covariates within the bandwidth sample does. These simple comparisons show that the main result is not driven by the discontinuity sample, or the inclusion of covariates. The fact that including covariates doesn't change the estimated coefficient in additional provides confidence that additional (un)observables do not jump at the discontinuity.

Comparing the results in columns (1) and (3) to column (5) gives the first main result of this paper. Prussian municipalities, when government policy shifts to persecution, are more effective at deportations. Violence against Jews is uncorrelated with Prussian status before the Nazis come to power. I interpret this finding in light of aspects of the organization of (local) government. Before providing direct evidence for this mechanism, I review some challenges to the interpretation of the main result in this section. I then provide results using a triple-differences design.

Accidental expansions of Prussia. In Table 2 It may be the case that *motivations* to expand to one territory may correlate with factors that have a long-run impact on anti-Semitism. In that case, while covariates vary smoothly over the boundary, the location of the boundary itself may be an issue. In Appendix Table A7, I therefore restrict the Prussian side of the discontinuity to only those territories incorporated unintentionally. I reach the same conclusions using this approach: Deportations are more effective in the Nazi period in formerly Prussian regions, and violence against Jews is balanced in the Weimar period.

Achieving policy objectives. In September 1941, the decision was made to deport all German Jews from Germany. The fact that I can estimate a positive treatment effect in columns (1) and (2) suggests that

this objective was not reached everywhere. I now provide two ways of studying whether the policy objective of making Germany 'free of Jews' (*Judenrein* or *Judenfrei* was the term the Nazis used). In Appendix Table A21, I rerun the model in column (1) for each year in the War and find that the positive effects are concentrated early on in the War. In Prussian parts of Germany, deportations happened more rapidly and because not all of Germany was made 'free of Jews', I find evidence for the resulting level difference in deportations in this section. In Appendix Table A23 I instead code an indicator equal to one if, given the 1939 German Jewish population, it is plausible that most German Jews were deported. I find that being on the formerly Prussian side of the boundary doubles the probability of reaching the stated policy objective.

Rule-following. A challenge to my preferred interpretation is due to rule-following behavior. I may observe more deportations when the rules change as perhaps citizens are more inclined to denounce their Jewish neighbors or bureaucrats are more likely to comply with new directives. I implement several exercises to study rule-following. The most direct one is that I use data on the number of individuals that were tried for in various ways for treason by the Nazi regime. The data on about 4,000 such cases have been recorded by Geerling and Magee (2017). I complement these with data on crime from Voigtländer and Voth (2012). In Appendix Table A26 I find that the estimated effect of having been Prussian is not affected by accounting for resistance against the Nazis or crime as measures of rule-following. In Appendix Tables A27 and A49, I implement two further exercises. First, I split up the Prussian part of my sample by how long different parts of Germany had been Prussian. If compliance builds up by exposure to the state, I would expect effects to be concentrated where Prussia had been established for a longer time (Heldring, 2021), bearing in mind that the reforms that built local state capacity started after most of Prussian territory had already been acquired. Second, the primary tool for building compliance by the government would be education. It may be that Prussian government socialized individuals into compliance. I find that effects are similar for early and late additions to Prussia, and that education does not explain my main results. I return to the adherence to rules for *bureaucrats* in section 9.

Local policy differences. Another challenge to the interpretation of my main results are policy differences. The outer borders of Prussia in 1871 continued to be the outer borders of the Free State of Prussia within the federal Weimar Republic. This means that the provincial government of the Free State of Prussia may have implemented policies that varied at the discontinuity. I may therefore, instead of identifying the effect of differences in former Prussian status, pick up differences in policies that were implemented by the Prussian provincial government. In 1932, however, Prussia as a local administrative unit was abolished in the so-called *Preussenschlag*, or the 'coup in Prussia'. Chancellor Franz von Papen revoked all policy making privileges of the provincial Prussian government and incorporated its administration into the administration of the Weimar Republic. After 1932, Prussia was no longer able to make independent

policy decisions. Policy differences are not driving my main results.[36]

Migration into Prussia before 1933. A final challenge comes from (differential) Jewish migration. It may be the case that Jews moved away from non-Prussian municipalities into Prussia, and from there they were subsequently deported. While plausible, this effect is not driving my result for deportations for two reasons. First, I control for the German Jewish population in 1939, after such migrations would have happened. Second, rather than controlling, I normalize deportations by the German Jewish population in 1939 in Appendix Table A22, with similar results.[37]

Further robustness. In Figure 3 I verify that the results in this section hold when restricting to municipalities closer to the boundary. I vary the bandwidth in steps of five kilometers from thirty to seventy kilometers. The estimated effect of having been Prussian is stable and significant for all bandwidths. In the Appendix I implement several other robustness checks, which I'll introduce briefly here. In Tables A5 and A22 I vary the measurement and normalization of the deportations. In Table A8 I vary the $f(location)$ function in Equation 1 and use the estimators proposed by Calonico et al. (2020). In Tables A10-A13 I vary the measurement and inclusion of covariates. In Table A14 I include the vote shares of other parties, including the anti-Semitic/right-wing DNVP and DNP parties. In Table A16 I control for religion and in Tables A10, A11, and A17 I control for emigrations between 1939 and 1941, Jewish population movements between 1925 and 1933, and 1933 and 1939, and vote shares for the Nazi party in each election leading up to 1933. In Tables A18 and A20 I then account for the distribution of *Mischlinge* and the fraction of recorded German Jews who actually practiced their faith. In Table A28 I vary the number and definition of large cities I include indicator variables for. I also show that my main result obtains in a sample that never experienced a Black Death pogrom (Table A25).

In this section I related former Prussian status to deportations, both in levels and *per bureaucrat*. The main identifying assumption in this section has been that only exposure to Prussia jumps at its former borders. I presented balance checks to support this claim, as well as results subsetting to a 'non-conquest' sample. I also showed that removing or adding all covariates leaves the estimated point estimate unchanged. Before moving to understanding mechanisms, I now use a panel of municipalities to study not just the number of deportees, but also the deportations themselves. This exercise serves as a complementary identification strategy as it relies on a different identifying assumptions.

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[36]I can re-estimate my result for pre-Nazi violence using only data from after the coup in Prussia. Results, which only affect pre-Nazi violence results, are similar (and available upon request).

[37]A more subtle variant of migration involves locations of deportations. For smaller municipalities, German Jews were relocated to larger nearby cities and deported from there. It could be that these are disproportionally in Prussia. Therefore, I control for the number of non resident deportees in all regressions.

6 Results: Deportations: Panel methods

In this section, I use the fact that the trains that deported Germany's Jews did not arrive everywhere at the same time. Trains traveled along the rail network and deportees were brought to nearby train stations in trucks and vans. I exploit this fact in two ways. I first measure whether a municipality was visited in a month (either as a train stop or as a stop of a van). This measurement admits a difference-in-differences model, comparing municipalities that were visited to municipalities that did not, before and after a visit, separately in Prussia and outside, resulting in a triple-differences comparison. Second, I focus on individual trains that crossed the study boundary. I construct a panel at the transport-stop level in which I assign the number of deportees to a stop that were deported in the month a train (or a van) stopped in a municipality. This specification admits adding route and train fixed effects, comparing, within the same route or train, the effectiveness of deportations from stops within Prussia, to stops outside. Since the data on transports and deportees come from different sources, the exercise in this section also triangulates both data sources.

To build intuition for the data, consider Figure 4. In this Figure I plot three trajectories of individual transports. I provide details on each transport in the Figure notes. Each transport departs in Germany and ends in a ghetto in Poland or the Czech Republic and transports deportees from its rail stops (such as Aachen in subfigure (a)) and several municipalities in the surrounding area. I consider a municipality visited by either train or truck if it is mentioned in Yad Vashem's reconstruction of the transport, available in 'The Deportations of Jews Research Project and Digital Database ("Transports to Extinction")' on www.yadveshem.org. In Appendix Section 10 I provide summary statistics for these transports.

A municipality-month panel. Transports departed at different times from different municipalities, and I compare the effectiveness of the deportations facilitated by these transports when trains stopped inside Prussia, compared to outside. In the language of modern panel data methods, treatment is staggered, non-absorbing (as a municipality can receive multiple transports), and treatment effects are potentially heterogeneous. I therefore assess pre-trends using the method of De Chaisemartin and d'Haultfoeuille (2020).[38] Results are in Figure 5. The Figure plots estimated coefficients of the De Chaisemartin and

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[38]Other modern panel data estimators are less well suited for estimated non-absorbing treatments. I estimate the following panel event-study model, separately for Prussian and non-Prussian regions:

$$Y_{mq} = \rho + \nu + \beta Visited_{mq} + \beta_J JewishIn_{m,q-1} + X_{mq}\beta'_c + \epsilon_{mq} \qquad (2)$$

Where Y_{mq} is the number of deportees from municipality m in month q. I include all months from 1941 to to q2, 1945, inclusive. ρ and ν are municipality and month fixed effects and $Visited_{mq}$ is an indicator equal to one if municipality m was visited by a train transport (or truck/van). $JewishIn_{m,q-1}$ is the number of German Jews remaining at the end of the previous month.[39] X_{mq} is a vector of time-invariant covariates. The most important covariate is an indicator equal to one for municipalities that were ever visited by a transport. This indicator captures connectedness to the rail network. I also include indicators for each large city, defined as before. I interact each covariate with month indicators. Finally, I include the number of non resident deportees, which is now time-varying. I express all treatment effects relative to the last pre-treatment period. I cluster standard errors at the municipality level.

d'Haultfoeuille (2020) estimator of equation 2, separately for Prussian and non-Prussian regions. Co-efficients prior to period 0 measure pre-trends and are informative about the untestable parallel-trends assumption underlying triple differences analyses.[40]

Figure 5 fails to reject no pre-trends in both subsamples, and coefficients are statistically indistinguish-able for pre-visit periods *between* subsamples. In addition, treatment effects are positive and significant in period 0, the month in which a municipality is visited by a transport and drop back afterwards. Finally, the period-0 treatment effect is larger for Prussian than for non-Prussian municipalities. I now proceed to estimate a standard triple-differences model to gauge whether, across all transports, stops that occur in formerly Prussian areas deport a larger fraction of the remaining German Jewish population than stops that occur outside. To do so, I estimate the following model, using OLS:

$$Y_{mq} = \rho_m + \nu_q + \beta_{td} Prussian_m * Visited_{mq} + \beta_V Visited_{mq} +$$

$$\beta_J JewishIn_{m,q-1} + X_{mq}\beta'_c + \epsilon_{mq} \quad (3)$$

Here Y_{mq} is the number of deportees from municipality m in month q. ρ_m and ν_q are municipality and period fixed effects. $Prussian_m * Visited_{mq}$ is the variable of interest: The interaction between having been part of Prussia and a time-varying indicator equal to one if a municipality was visited by a transport in month q. β_{td} is the *triple differences* coefficient of interest: It measures, conditional on covariates, the average difference in the change in the number of deportees between Prussian and non-Prussian munici-palities when a transport comes through. It therefore captures a similar idea to the previous cross-sectional analyses: If positive, this coefficient shows that Prussian municipalities deported more effectively. How-ever, a key aspect of this setup that is different is that virtually all deportations did happen through train and truck/van transports. This means that the 'first' difference, comparing visited to non-visited munic-ipalities, contributes less to the final estimate of the effect of having been Prussian than in an ordinary triple-differences setup. Therefore, this model primarily captures the difference in the effectiveness of deportations, when visited by a transport, among municipalities visited at least once, inside and outside Prussia. I adapt my discussion of results accordingly. I include a measure of the number of German Jews remaining at the start of each month. I construct this by starting with the German Jewish population in the 1939 census and subtracting the number of deportees and emigres up to and including the previous

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[40]Olden and Møen (2022) establish that for the triple difference estimator it is sufficient to meet a weaker version of this require-ment. Rather than parallel trends holding in both subsamples that constitute the third difference in a triple-differences setup, it is sufficient that pre-trends are parallel between subsamples. I test here for the stronger version of this requirement, where pre-trends are parallel within each subsample.

month. I also include the time varying number of non resident deportees, as before, as well as the requisite double interactions and a separate time trend for Prussia. These measures are part of X_{mq}, a vector of time-varying covariates. In addition, it includes the following covariates: An indicator equal to one if a municipality was ever visited in any period, capturing remoteness or accessibility of transport networks, and large city indicators, all interacted with month indicators. I cluster standard errors at the municipality level.

Table 5 reports results. In Column (1) and (2) I use the natural logarithm of the number of deportees as the dependent variable and in Columns (3) and (4) I use the number of deportees normalized by the size of bureaucracy. These are the time-varying versions of the same dependent variables as in Table 4.[41] Row 1 reports estimates of β_{td} in equation 3. Consider the point estimate in column (1), 0.30 (clustered s.e. 0.10). Compared to non-Prussian municipalities, Prussian municipalities, when visited, deport 30 percent more German Jews than non-Prussian municipalities, relative to before a transport came through. The size of this effect is comparable to the estimated effect in my RD exercise (0.37). As before, Column (3) shows that deportations per bureaucrat are higher as well. These effects are identical when including a trend in whether a municipality was ever visited (Columns (2) and (4)).

A train-stop panel. The previous analysis includes fixed effects to account for unobserved heterogeneity at the municipal level. In this section, I construct a panel at the level of the transport-stop. For each transport, I record the stops the train makes, and each place deportees were transported to a train from. This results in a panel dataset in which the cross-sectional unit is an individual transport, such as the ones in Figure 4. The time-dimension is a stop, where a stop is now either a stop at a train station, or a place from which deportees were brought to a station. Since many transports either crossed over from Prussia to non-Prussian parts of Germany, or vice-versa, this dataset admits the use of origin-destination pair fixed effects, and train fixed effects. The latter compare, within the same train and *only* for visited municipalities, the effectiveness of deportations from stops that are in Prussia to those that are not. Since most trains traveled from their origin to their destination in a few days, within origin-destination or train, this approach effectively makes a cross-sectional between Prussian and non-Prussian stops, holding fixed any aggregate differences between the month in which the transport happened and other months. To do so, I estimate the following model:

$$Y_{tsq} = \rho_t + \nu_q + \beta_{ts}Prussian_s + \beta_J JewishIn_{s,q-1} + X_{sq}\beta_c' + \epsilon_{tsq} \tag{4}$$

Here Y_{tsq} is an outcome Y for transport t and stop s in month q. I assign deportation outcomes to a

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[41]The size of the bureaucracy is not time-varying and is fixed in 1933. Deportations normalized by the number of bureaucrats is therefore time-varying only due to time variation in the number of deportees.

transport stop by the month in which a transport visits stop s. To give an example: If a train stopped in Aachen and then went to Frankfurt, before going East in January 1943, I assign the number of deportees recorded in Aachen and Frankfurt in that month to these stops. ρ_t and ν_q are fixed effects. ρ_t is either a vector of fixed effects for origin-destination pairs, or a vector of fixed effects for each individual transport. The inclusion of ρ_t ensures that I compare the effectiveness of deportations *within* the origin-destination pair (between and within trains traveling that route) or within an individual train transport. ν_q is a month fixed effect. As before, $JewishIn_{s,q-1}$ measures the number of German Jews remaining at the start of the month of the transport visit. X_{sq} is a vector of additional covariates. It includes an indicator variable for whether a stop was a train station or a location from which deportees were brought to a train stop. I also include the number of non-resident deportees from each stop, as before, as well as a control for the stop-order among stops. This variable is zero for all truck/van stops and increases for each subsequent train-station stop.[42] It accounts for the fact that I mechanically expect fewer deportations from later train stops. I cluster standard errors at the transport level. β_{ts} is the coefficient of interest: It captures the measured effect of being a stop in a former Prussian part of Germany compared to a former non-Prussian part of Germany, within the same origin-destination pair or within the same train.[43]

Results are in Table 6. I report estimates of β_{ts} in row 1. I vary fixed effects in Columns. In Column (1) and (3) I include origin by destination fixed effects. In Columns (2) and (4) I include the more stringent train fixed effects. As before, columns also vary the dependent variable between the natural log of the number of deportations, or deportations normalized per bureaucrat. In Column (2), I find that, using within train variation, that when trains stop in Prussia they transport 74% more deportees than from stops outside of Prussia, conditioning on the remaining Jewish population.[44] These fixed effects are especially important because they capture, among other things, the size of the train, its number of stops, the train personnel, and any strategic routing of trains. Since the panel is composed of transport stops, there is no selection into being visited. I again find a similarly strong effect of deportations per bureaucrat (in Column (4)), and using origin-destination fixed effects (Columns (1) and (3)).

Robustness. In the Appendix, I present several robustness exercises, which I only discuss briefly here. In Table A31 I show that the results in this section are robust to removing covariates, keeping only munic-

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[42]In Appendix Table A40 I add stops in Poland that are ordinarily outside the sample but were part of some trains.

[43]There are 95 unique origin-destination pairs in my dataset. The most common origin is Berlin (188 transports started here) and the most common destination is the ghetto in Theresienstadt (268 transports end there). Of course, within the same origin-destination pair, trains can make multiple stops. There are 415 individual transports. Singleton origin-destinations and trains are transports that stop once in Germany (since locations outside Germany are not in my sample); and are collinear with fixed effects. In my final analyses, I have 82 fixed effects for origin-destinations and 298 for trains. For example, there was only one transport from Frankfurt (am Main) to Kaunas (Yad Vashem deportation id 9437977).

[44]The fact that this coefficient is large relative to earlier estimates is that the sample in these regressions includes all of Germany, including larger cities, like Berlin. In Appendix Table A42 I estimate results without Berlin, and the coefficient is is comparable to earlier results.

ipality and month fixed effects. In Table A34 I include a trend in German Jewish 1939 census population, rather than computing the remaining number of German Jews. In Table A35 I remove municipalities in (now) Poland that saw a lot of stop-over traffic and in Table A36 I include indicator variables for cities in Germany that acted as rendez-vous points for trains. In addition, I subset to trains that traveled to Theresienstadt, the main ghetto outside Germany, in Appendix Table A41 and I remove trains departing from Berlin in Table A42

In this section, I presented an alternative identification strategy which relies on different identification assumptions than the RD strategy in the previous section. I find similar results. I now study mechanisms.

7   Mechanisms: Government effectiveness

There may be many mechanisms connecting former Prussian status to deportations. The hypothesis of this paper focuses on the effectiveness of local government and the historical literature on the development of German (local) government suggests that Prussian municipalities may have been better organized. Before studying competing mechanisms, this section provides direct evidence in favor of the government effectiveness mechanism in three parts. I start by studying public goods, as the output side of local government. In this section, I measure the success of unemployment assistance because it was a policy that the Nazis emphasized more than the government of the Weimar republic. In Appendix Table A43, I extend my analysis to several other public goods. Second, I measure tax collection as the input side of the local government. In Appendix Table A49, I extend this analysis to information processing capacity, as an input to both effective taxation and, as discussed in the background section, the preparation and execution of the deportations. In Appendix Tables A46 and A47 I also study local government expenditure to understand where tax revenue is allocated. I finally measure specialization within local government as an organizational component of effective local government. In the Appendix I extend this approach to cover several other aspects of local government organization. I find that Prussian municipalities provide more public goods, raise more taxes, process information more effectively, spend more, and are more specialized. Crucially, I find similar effects before and after the Nazis come to power. Most effects are identical, except for unemployment benefit extension which was a Nazi policy priority, like deportations. For this outcome, I find more precisely estimated effects during the Nazi period. In sum, this section establishes the broad effectiveness of local government as one mechanism sustaining the effect of having been Prussian. Since politicians did not change their objectives on most of these outcomes, results do not differ before and after the Nazis came to power.

7.1 Results: Public Goods

In this section I study the output side of local government through locally provided public goods. I focus on two types of public goods: First, one that became a policy priority under the Nazi government, and then public goods that were provided before and after the Nazis came to power, but that were not particularly prioritized.

The Great Depression coincided with the rise to power of the Nazis. Unemployment rates reached 25% in 1932 (Dimsdale et al., 2006). To combat unemployment, the Nazis pursued two policy routes: Employment programs - such as highway construction - and unemployment assistance. At the local level individuals could get assistance through unemployment insurance, special emergency programs, or through regular municipal unemployment payments.[45] At the end of 1931, the three programs each covered about a third of the 1.7 million unemployed Germans. I measure the total number of unemployment welfare recipients that were supported by these municipal programs, which I record from the Statistical Yearbooks of German cities before and after the Nazis came to power, and control for total unemployment and economic development. To measure regular public good provision, it is important to distinguish local public good provided by the central government, such as highways passing through a municipality, and public goods provided by the local government. In Appendix Table A43, I report results for several local public goods, such as local public school graduation rates, road construction, the efficiency of trash collection, and municipal water supply.[46]

Results for unemployment assistance are in Table 7. In columns (1) and (2) I use the number of unemployment welfare recipients as the dependent variable, before and after the Nazis came to power. I report standardized coefficients to facilitate comparisons across column/periods. Before the Nazis come to power, I find a positive effect of having been Prussian. After the Nazis come to power, I find a similar but more precisely estimated positive effect of having been Prussian on the number of unemployment recipients, conditioning on the total number of unemployed people in a municipality, as well as measures of economic development. Having been Prussian increases the number of unemployment recipients by about 0.1 standard deviation. In Appendix Table A43, I find similarly positive and significant results for other locally provided public goods. Importantly, all results for public good provision are stable before and after the Nazis come to power. This last result is important because it shows that Prussia left a legacy of more effective local government, and this legacy does not change after the Nazis took power. This is

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[45]In German, these are: *Hauptunterstützungsempfänger in der Arbeitslosenversicherung* for recipients of unemployment insurance; *Hauptunterstützungsempfänger in der Krisenfürsorge* for those in emergency assistance, and *Erwerbslose in gemeindlicher Fürsorge* for those in regular municipal unemployment programs.

[46]Note that, for these more complex public goods, it is significantly harder to collect historical data. I have been able to reconstruct these figures for about 250 larger cities/municipalities in Germany. The number of observations in the bandwidth in these regressions is therefore lower.

likely because public provision was a priority for both governments.

7.2   Results: Taxation

In Table 7, Columns (3) and (4) I study conventional measures of the capacity of the state. The most frequently studied aspect of the capacity of the local state to implement policy is 'fiscal capacity', the ability of a government to raise taxes (Besley and Persson, 2011b). I record the total amount of local tax raised per capita, before and after the Nazis come to power, again conditioning on economic development. The relevant currency units are the *Reichsmark* before the war and the *Deutschmark* after the war. I express all relevant quantities in 1929 prices [47]. I find that Prussian municipalities raise more taxes per capita, both before and after the Nazis come to power. The estimated coefficients are large (being Prussian increases tax raised per capita by half a standard deviation), and virtually identical between the two periods.[48]

In Appendix Table A49, I construct a forensic measure of the quality of information processing in a municipal government. In section 2 I discussed that the historical literature on municipal involvement in deportations points to information aggregation and processing as one of the primary ways in which municipal bureaucrats aided the deportations. For example, by keeping lists of German Jews' addresses municipal bureaucrats facilitated relocation to designated housing, as well as their rounding up when the Gestapo transports came. This task was the kind of task that would also be implemented before the Nazis came to power but was used to implement deportations after the Nazi government directed municipal government towards that policy. I rely on the fact that the Statistical Yearbooks I use for the bureaucracy data in this paper are composed from reports of each individual municipality to a central organization. This organization annotated data if a municipality delivered out-of-date information. For example, for inclusion in the 1937 yearbook, data should have been up to date up to March 1st, 1937. Some municipalities did not have their information up to date and instead reported information that was current up to December 31, 1936. I code an indicator equal to one if a municipality reports out of date information at any point in 1927-1930, and 1936-1939. I find that Prussian municipalities are less likely to

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[47]I use the CPI values for Germany from Piketty and Zucman (2014)

[48]In Appendix Tables A44 and A45 I report several additional analyses. I first report results for differences in tax raised in levels. The estimated effects are positive, but not statistically significant. This result is consistent with the idea that the effect of Prussia is stronger for the effectiveness of tax collection but that there is no reason to think that Prussian municipalities should collect more taxes in levels. I then break up total tax into its constituent parts, such as property taxes and taxes on business. I estimate the effect of being Prussian on both the total amount raised, as well as the total amount raised per capita. In line with the results in Table 7 I find no effects for the total amount raised and positive and significant effects for each category when I normalize by population for both the Weimar and Nazi periods. I also study local government expenditure. Besides being an alternative way to measure state capacity, studying government expenditure shows what Prussian municipalities spent their higher tax revenue on. First, I study expenditure per capita. Prussian municipalities expend more per capita, in both the Weimar and Nazi periods. Second, I consider expenditure in levels. Across all expenditure categories I study, there are no significant expenditure differences when I do not normalize by population. Finally, I break up total expenditure into its constituent spending categories. For the Weimar period, I find that the increased tax revenue is directed towards education. In the Nazi period, increased tax revenue is still spent on education, but I also observe a significant increase in local expenditure on police. I observe an increase in wages as well, which I'll return to below.

report out of date information.

7.3 Results: Government organization

In this section I consider the internal organization of government. The more effective provision of public goods and more effective tax collection are ultimately due to some aspects the organization or workforce of the individual local governments (on these aspects of government, see e.g. Dewatripont et al. (1999); Bandiera et al. (2021); Finan et al. (2017)). The data that are available on historical local government are not detailed enough to fully pin down the ultimate source of higher effectiveness, but I pursue several directions in this section. I first study a historically informed aspect of organization: Specialization. In the Appendix I then ask whether incentives are different by studying wages. I also study the overall size of government, and differences in the human capital of the local bureaucrats.

Historians of the Prussian bureaucracy argue that specialization is one of the dimensions that set the Prussian bureaucracy apart (Dorn, 1931, 1932a,b; Kiser and Schneider, 1994; Clark, 2006).[49] They point to historical reasons for pursuing specialization, but a drive towards specialization may simply come from a natural desire to improve the efficiency of government (see e.g. Rosen (1983)).[50] I measure *within* municipality specialization using data from the Statistical Yearbooks of German Cities. I code the number of employees in different employment categories. The yearbooks record a fixed number of fifteen job categories, but not all cities employ bureaucrats in each category, and some only in a few. I then construct a Hirschmann-Herfindahl index measuring the concentration of bureaucrats across occupied job categories.[51]

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[49]For example, when discussing a 1766 administrative reform, Dorn (1932a, p. 80) writes: "he [the King] measurably increased the rapidity of administrative procedure and introduced the principle of intensive specialization into Prussian administration". He concludes: "This progressive specialization among the central branches of Prussian administration improved not only the quality of the administration but the bureaucracy itself" (p. 81). Why Prussian rulers and administrators were more focused on specialization than other German states is a question that is beyond the scope of this paper, but may be sought in the initial bureaucratic reforms by Frederick William (see the historical background section in the Appendix, section 4). Frederick William reformed the General Finance Directory and the General Commissariat - the main government ministries at the time - and his "ultimate objective was to forge an organic, pan-territorial body of expertise out of a plurality of separate specialist knowledges" (Clark, 2006, p. 88). His desire to reform the bureaucracy in this fashion originated from his education in the Netherlands, at that point the most prosperous country in the world, which "had developed a robust fiscal regime and a distinctive military culture with recognizably modern features: the regular and systematic drilling of troops in battleground maneuvers, *a high level of functional differentiation* and a disciplined professional officer corps."[emphasis added] (Clark, 2006, p. 88).

[50]There is a large literature in economics that points out why specialization may lead to efficiency improvements, reviewed in Garicano and Van Zandt (2012). In organizations, for example, where agents generate ideas, Hart and Moore (2005) show that the optimal degree of specialization depends on the returns to coordinating among activities. Specialization may also arise from optimal information processing (Bolton and Dewatripont, 1994), decision making procedures (Sah and Stiglitz, 1986), resource allocation (Cremer, 1980), or from a desire to improve monitoring (Calvo and Wellisz, 1979). Finally, specialization may lead to an increased ability to respond to market changes (Thesmar and Thoenig, 2007), and provides increased incentives for agents with career concerns (Dewatripont et al., 1999). For an overview of the empirical work that has followed these theoretical contributions, see Bloom et al. (2014).

[51]To be precise, I compute:

$$HH_i = \sum_{n=1}^{c} (employment_{ci}/totalemployment_i)^2 \tag{5}$$

I compute these measures separately for two groups of employees. The first group encompasses all job categories; and the second encompasses only the job categories that correspond to the types of managerial jobs in administration, local transport, and management of assets such as trucks, and buildings that I identified in the background section as relevant for the organization of the deportations. In the data, the corresponding job categories are: General administration, police administration, public works and construction, fleet and street cleaning, and tax management. In Appendix section 9, I provide an overview table of all job categories.

If formerly Prussian areas are indeed more (horizontally) specialized, I expect a lower index of concentration, conditional on size of the bureaucracy, on the Prussian side of the study boundary. I report results in Table 8, Columns (5) and (6). I use the Hirschmann-Herfindahl index of occupational specialization as the dependent variable, controlling for the size of the bureaucracy. As before, row 1 contains the estimated effects of having been Prussian on these outcomes. I find that Prussian municipalities have less concentrated, and therefore more (functionally) specialized bureaucracies, both overall and in deportation-relevant job categories. This result is consistent with the idea that one source of increased efficiency is organizational structure. In Appendix Tables A48 and A49 I test for differences in total employment, total wages paid out to municipal employees, and human capital differences in the work force. I find that wages are systematically higher in Prussian municipalities.[52] In addition to organizational structure, wages may therefore be another aspect of organization that sustains better outcomes, in this case through financial incentives. I find no differences in the overall size of the bureaucracy or in bureaucratic human capital.[53]

In this section I have brought together evidence on public good provision, taxation, and organizational structure to test the hypothesis that part of what sustains the effect of having been Prussian is greater local state capacity. There may, of course, be other mechanisms. The next section therefore studies the most plausible alternative mechanisms.

8   Alternative Mechanisms

The literature - and especially the literature studying Nazi party membership and the persecution of German Jews - suggests several alternative mechanisms. For example, the Nazis came to power in the

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HH_i is the Hirschmann-Herfindahl index for city i which is computed as employment in category c for city i divided by total employment in city i. $totalemployment_i = \sum_c employment_{ci}$. The Appendix, section 9, lists all job categories.

[52] Average wages paid are higher, and wages are particularly higher in administration and education, consistent with the higher expenditure on popular education I discussed in footnote 43.

[53] The results discussed in the previous footnote, footnote 43, and the Appendix Table A43 show that *popular* education was better organized, better funded and that bureaucratic officials in education received higher wages. My results in Table A49 show that the education of bureaucrats in the government was not systematically different.

middle of the Great Depression in Germany and the incidence of the economic downturn may correlate with the intensity of anti-Semitism. I use data on local employment between 1925 and 1933, and the level of unemployment in 1933 to test whether economic headwinds were particularly strong in Prussian areas. Second, Satyanath et al. (2017) show that in German towns with a denser network of social clubs, Nazi party membership grew more rapidly. It may be the case that these networks were particularly dense in Prussia. I measure the number of civic and military clubs per capita using the authors' data. Third, Adena et al. (2015) study a related hypothesis: pro-Nazi propaganda on the radio increased Nazi party membership. I test whether there were significantly more radio listeners in Prussia in 1932 and 1933, using the authors' data. If so, the effect of Prussia may be driven by the reach of the communication of new policies, rather than by the capacity of the local state implementing the policies. Finally, Spenkuch and Tillmann (2018) point to the role of religion. I use data from the 1925 census to measure the distribution of Catholics and Protestants within Germany.

Table 9 studies these mechanisms within a 50 kilometer bandwidth on either side of the study boundary. In columns (1) and (2) I study the Great Depression. I find that both employment growth and unemployment are uncorrelated with Prussia status (at the boundary). In columns (3) and (4) I study social capital. In column (3) I use the number of civic clubs (x1000) per capita as the dependent variable, and in column (4) I use the number of military clubs. Following the terminology in Satyanath et al. (2017) civic clubs refer to associations such as hiking clubs, choirs, and women's clubs. Military clubs are either veteran's associations or 'Stahlhelm' paramilitary clubs.[54] Both measures balance. Columns (5) and (6) focus on radio ownership. In these columns, I condition on signal strength, using data from Adena et al. (2015). I find a precise zero effect of being Prussian. These results suggest that differential radio ownership is not driving the observed effect of being Prussian. Finally, religious composition is balanced across the study boundary (columns (7) and (8)). I nevertheless show robustness to including measures of religion in the main specification in Appendix Table A16, and I find that it does not affect the main result. The results in this section suggest that several other potential mechanisms either balance or are less important than state capacity for transmitting the effect of having been Prussian *at the study boundary*.

These results at the boundary do not mean that these factors did not matter for the implementation of the Holocaust more broadly.

In Appendix Table A50, I therefore perform a correlational heterogeneous effects exercise to study complements and substitutes to more effective local government. I use a sample that covers all of Germany, without restricting to any bandwidth, and interact my Prussia indicator with the outcome variables

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[54]See the appendix to Satyanath et al. (2017) for details. The reason to split these is that Prussia's militaristic culture may lead to different effects of civic and military clubs (see Clark (2006) on militarism in Prussia).

from the previous section, complemented with the measures of medieval anti-Semitism from Voigtländer and Voth (2012) that I used in the balance section. I then re-estimate equation 1 adding each factor and its interaction as additional variables, using deportations as the dependent variable. I then classify factors as complements if the interaction is positive and substitutes if negative. The idea of this exercise is that although these variables may balance at the study boundary, in the aggregate it seems reasonable to expect that the effect of being Prussian on deportations is concentrated where, for example, social capital is higher. I find that the strongest interaction is with medieval anti-Semitism. The effect of having been Prussian is amplified in these places.[55] Voigtländer and Voth (2012) show that medieval anti-Semitic incidents capture a tradition of anti-Semitism that culturally persisted. It is therefore likely that this interaction shows that the effect of having a more effective state is amplified if the local population collaborates.

9   The conditions under which state building can be abused

The results of this paper so far suggest that state (capacity) building can be abused by future policy makers. As such, they echo similar findings for, for example, the abuse of well-intentioned foreign aid (Nunn and Qian, 2014). This does not mean, of course, that we should give up on state building policies (leaving aside the generalizability of the findings of this study). It is more likely that there are aspects of government that determine whether capacity can be abused. The most obvious aspect is some form of electoral oversight. Several studies empirically establish the effect of checks and balances reducing political abuse (Ferraz and Finan, 2011; Casey et al., 2021; Garbiras-Díaz and Montenegro, 2022), and it seems reasonable that these results extend to the bureaucracy. In this section, I study conditions under which the main result of this paper obtains in a heterogeneous effects exercise. I study factors external to the local government and internal, organizational factors.

The first factor I study is electoral oversight. The Nazis abolished national and local elections after they took power, so I focus on the last national election, in 1933. I focus on municipalities where the Nazis became the largest party and code an indicator equal to one if the Nazi party had an above median vote share, as a measure of the degree of popular approval of (future) Nazi policies. The second factor I consider is the role of 'higher ups' in the Nazi bureaucratic hierarchy. All municipalities were part of larger administrative units. One such unit was the *Gau*, which was established by the Nazi party as a way to directly control local government within Germany. In 1934, there were 32 Gaue in Germany, each led by a Nazi party official, the *Gauleiter*. These Gauleiter were the direct principals of the municipal governments.

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[55]Recall that the effect of having been Prussian is not *driven* by these places. In the main results section I showed that I can remove all municipalities that experienced some medieval anti-Semitism and the effect of having been Prussian is very similar.

Some of these Gauleiter oversaw early deportations, mostly of German citizens, to concentration camps within Germany, rather than outside Germany. These deportations, such as of Gerry Levy described in the background section, were not national policy, and allowed for significant local initiative. These initiatives are documented in detail in Gruner (2004, 2011). I record the total number of individuals deported before 1939, and split the sample by the median of this proxy of ideological commitment of the leaders. The Gauleiter data is aimed to proxy the common defense by prominent Nazis that the 'higher ups' were the fanatics and that they were simply following orders. For example, Adolf Eichmann, the highest ranking SS officer to be tried after the War for his involvement in the Holocaust wrote a letter to the Israeli president asking for clemency.[56] He wrote:

> "There is a need to draw a line between the leaders responsible and the people like me forced to serve as mere instruments in the hands of the leaders..."

Electoral oversight and ideological fanaticism are factors that operate outside of the individual municipal bureaucracies. I now consider two measures that are internal to local bureaucracies. Within bureaucracies, orders would come from higher-ups in the municipal hierarchy. I measure how hierarchical individual municipalities were by the ratio of white to blue collar workers in the municipal government.[57] I finally measure one particular aspect of the local bureaucracies based on Arendt (2006). Hannah Arendt famously discussed the 1967 trial of Adolf Eichmann; and coined the phrase the 'banality of evil', to describe the implementation of the Holocaust as mostly done by assiduous careerist bureaucrats, rather than by fanatical Nazis. For example, in one of the more dramatic moments of the trial, Eichmann stepped out of his defendant's booth to show an organizational chart which showed him as a mere 'cog in the wheel' of the Nazi bureaucracy (cited in Breton and Wintrobe (1986)). I measure a particular commitment to bureaucratic procedure by a feature of the job structure of German public service. White collar jobs are recorded in two types, *Beamte* and *Angestellte*. Both do similar jobs, but Beamte can be tenured, with appointments for life, and these civil servants are considered a special 'class', distinguished by particular commitment to implementation of rules and procedures. I measure the ratio of Beamte to all white collar employees as a measure of the assiduity of the local bureaucrats.

I report results in Table 9. This table uses the log of total deportations between 1941 and 1945 as the dependent variable, like in Table 4. In column (1) I repeat the main result from that table, which appears in column (1) as well. In subsequent pairs of columns, I split the sample by the median of measures of factors that may be informative about conditions under which state capacity is 'future proof'. In Panel

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[56] The letter is publicly available, see here: https://www.documentcloud.org/documents/2698866-Handwritten.html (accessed November 2024).

[57] In the employment data I have, white collar workers are referred to as *Beamte* and *Angestellte*; and blue collar workers are referred to as *Arbeiter*.

I, columns (2) and (3) I split the sample by the support for the Nazi party. The effect of Prussia on deportations is concentrated in municipalities with strong Nazi electoral support, or weak oversight against deportations, suggesting that electoral oversight, or more broadly, checks and balances are an important factor determining the future effect of state building projects. In columns (4) and (5) I split the sample by whether the Gauleiter was an ardent Nazi or not. I find that effects are concentrated where principals are more ideologically committed, suggesting a role for leadership. In Panel II, columns (2) and (3) I split by the ratio of white to blue collar workers, capturing within-bureaucracy hierarchy. I find that the effect of Prussia is stronger in more hierarchical municipalities. And finally, in columns (3) and (4) of Panel II I split by the ratio of Beamte to Angestellte. I find that the effect of having been Prussian is concentrated in municipalities with a higher fraction of tenured civil servants, suggesting a role of bureaucratic assiduity/commitment.

In this section I have implemented a simple exercise to understand if there are factors that determine when state capacity can be abused. I find a prominent role of democracy/electoral oversight, and leadership. Hierarchy and bureaucratic culture matter too.

Together, these results suggest that there are political and cultural conditions under which the main result of this paper obtains: For the local government to be co-opted into the organization of the Holocaust, the Nazis had to abolish democracy, and stack leadership with committed principals. This is in line with the historical literature on the organization of the Holocaust cited in the background section. The Nazis went to great lengths to ensure that all expropriation, exploitation, and deportation was legal by their own laws, and implemented bureaucratically and orderly. The results in this section suggest that, combined with fanaticism and absence of oversight, it is this embedding of repression as a regular task within ordinary procedures that may have enabled the scaling up of the implementation of the Holocaust within Germany to genocidal proportions. Additionally, the results in this section suggest that there are ways to mitigate the potential future abuse of state capacity to implement repressive policies.

10   Conclusion

Observers of the Holocaust have often remarked on its bureaucratic nature (Arendt, 2006). This paper studies the effect of a stronger state, measured by the legacy of Prussia, before after the Nazis came to power. Formerly Prussian municipalities deport Germany's Jews more effectively when the Nazis pursue this policy. In domains where the Nazis did not switch policy radically - like public good provision and tax collection - formerly Prussian municipalities perform better both before and after the Nazis came to power.

Taken together, the results in this paper suggest that state capacity, once established, may be abused by a country's politicians. The reduced form effect of having a more effective state bureaucracy on social outcomes is conditional on the policy objectives pursued. There are potentially mitigating factors of this effect, and state capacity building project may take these into account. An important aspect of my setting is that the Nazis legalized every repressive action before executing it. It is an open question what bureaucrats would have done had the Nazi government directed local government to implement illegal policy objectives. This is an avenue worth exploring in further research. A similarly interesting avenue of further research concerns the 'unbundling' of state capacity. Local government had no experience with deportations but could aid doing what it had specialized in before: Keeping records and organizing within city transports. This is in line with the view of state capacity as a concept reflecting the general ability of the state to get things done (Besley and Persson, 2010). Empirically it is not obvious that expertise in one domain translates to another, and I think this could be profitably explored further.

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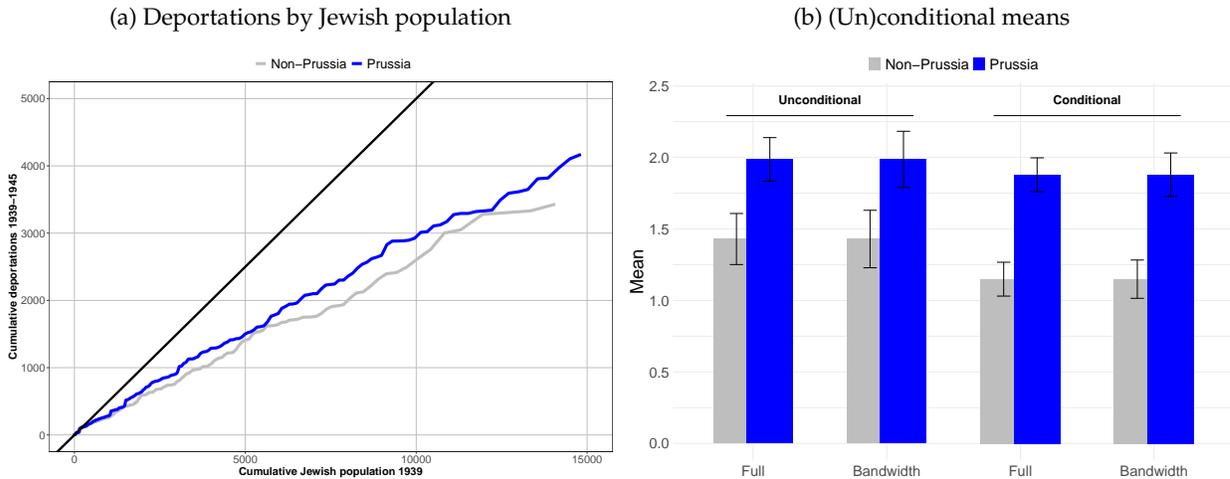## Figure 1: TIMELINES OF MAIN EVENTS AND MAIN RESULTS

Panel I: Timeline of main events in Prussia and Germany, 1415-1945
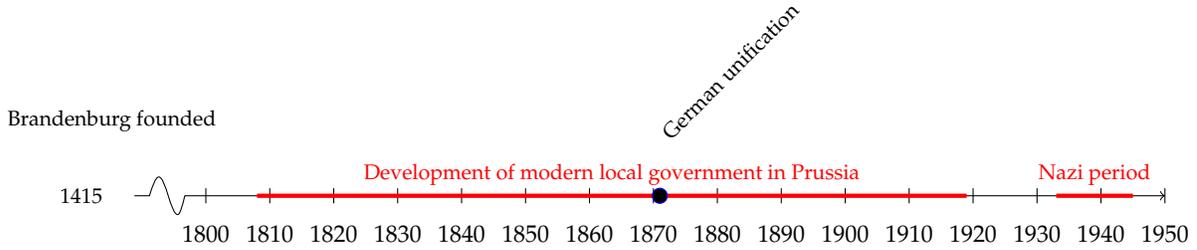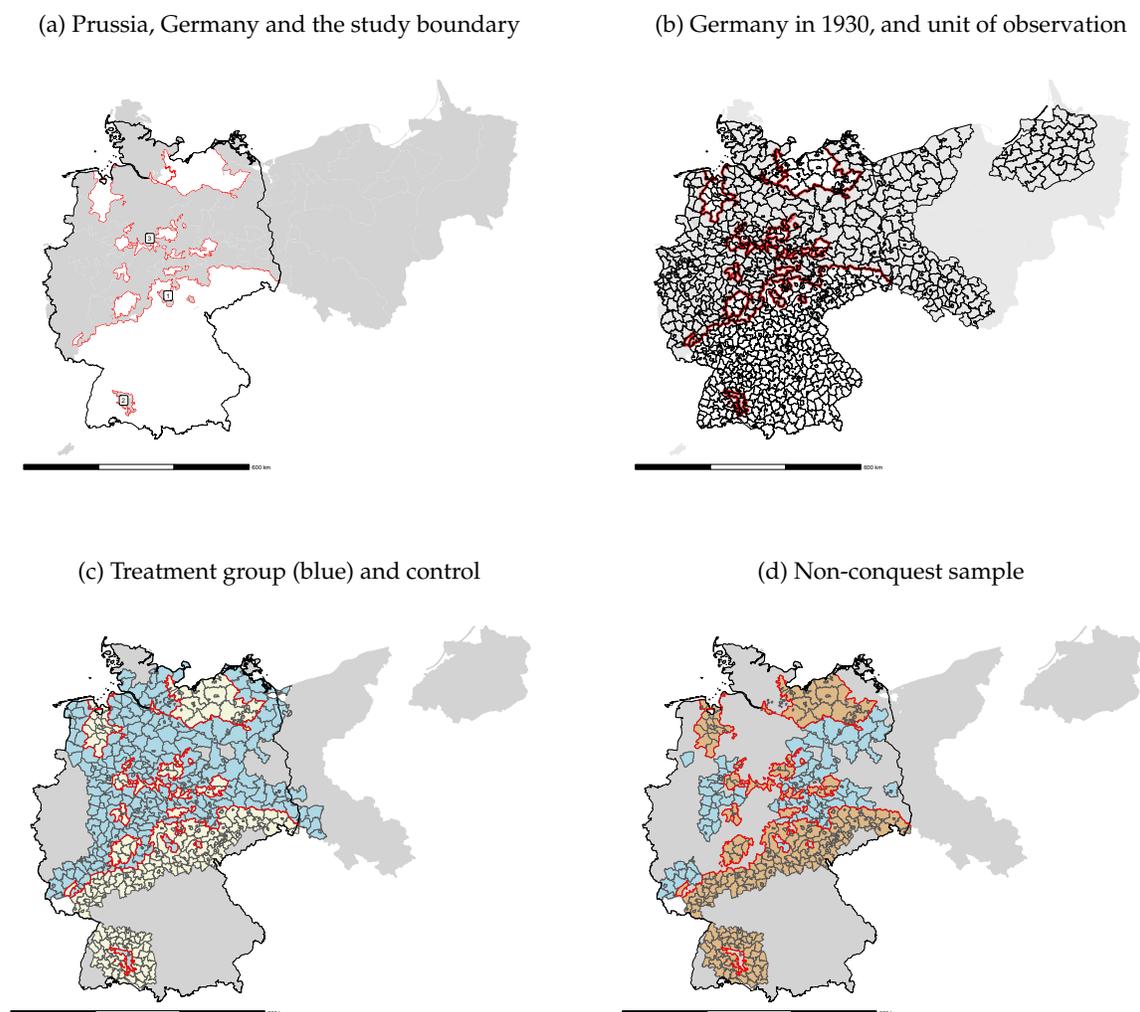

Panel II: Timeline of main events 1927-1945
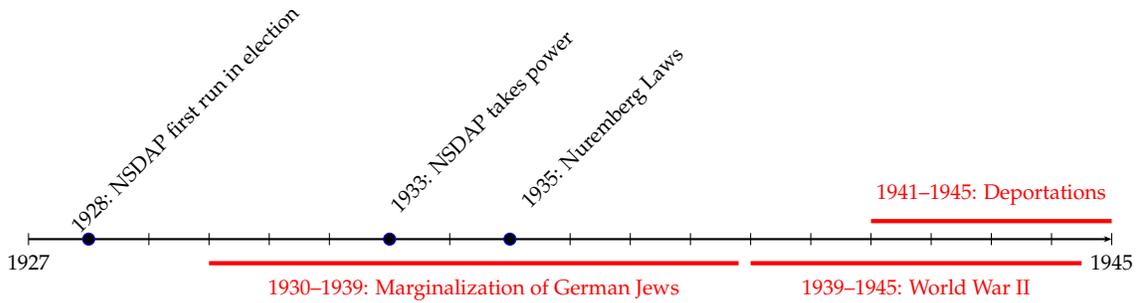
Panel III: Deportations 1941-1945, CDFs and (un)conditional means

(a) Deportations by Jewish population

(b) (Un)conditional means

Notes: Panel I provides a timeline indicating four key events for this paper. First, the founding of Brandenburg, the polity that would become Prussia. Second, the period after the 1808 "Ordnung für sämtliche Städte der Preußischen Monarchie" in Prussia, which rationalized the relationship between the state and local city government. I end this period with the 'Erzbergersche Reform' which set up the modern tax regime as it existed during the Nazi period. Third, the unification of Germany by Prussia in 1871. Fourth, the Nazi period in Germany which lasted from 1933-1945. In Panel II, I indicate the main events just before and during the Nazi period. The Nazi party (NSDAP) takes power in 1933, and passes the anti-Semitic Nuremberg Laws in 1935. Deportations start in 1941. In Panel III, sub-figure (a), relates the pre-War German Jewish population in 1939 to the total number of deportations between 1939 and 1945. The x-axis measures the cumulative population of German Jews within 50 kilometers from the Prussian border, after ordering municipalities low to high. The y-axis measures total deportations of these municipalities. The blue and gray lines measure German Jews population and deportations inside and outside Prussia. Sub-figure (b) graphs the conditional and unconditional mean of the dependent variable both in the full sample and in the bandwidth of 50 kilometers from the former Prussian borders. The conditional estimates are conditioned on the full set of covariates in Table 4. The conditional, within bandwidth, difference is the main result of this paper, and corresponds to Table 4, column (1).

Figure 2: MAPS SHOWING MODERN GERMANY, PRUSSIA, THE STUDY BOUNDARIES, AND SAMPLES

(a) Prussia, Germany and the study boundary

(b) Germany in 1930, and unit of observation

(c) Treatment group (blue) and control

(d) Non-conquest sample

Notes: Figure 2a displays the extent of Prussia in 1871 (in gray), the outline of modern Germany (in black), and the study boundaries (in red). Numbers indicate boundary segments. Segment (1) comprises the continuous border separating Prussia, including its district exclaves of Ziegenrück and Schmalkalden-Schleusingen, as well as the principalities of Anhalt, Schwarzburg-Sondershausen, Waldeck, Landesteil Birkenfeld, and the Grand Duchy of Hesse from Southern Germany, which are included due to their proximity to the the long Prussian border. Segment (2) represents the boundary between the Prussian principality of Hohenzollern-Sigmaringen and the non-Prussian regions of Southern Germany. Segment (3) encompasses the collection of boundaries separating Prussia from the northern territories of Mecklenburg, Oldenburg, the Hanseatic city of Hamburg, Braunschweig, and Schaumburg-Lippe, all non-Prussian states in Northern Germany. Figure 2b shows the unit of observation for this paper: German municipalities. Municipalities exist only for the extent of Germany as of 1930, which does not include then-Poland and is smaller than the extent of Prussia in 1871 (in gray). Figure 2c displays the regression sample. Municipalities within 50 kilometers of the study boundary are outlined. Municipalities in blue are historically within Prussia and form my treatment group. Municipalities in yellow are the control group. Germany in 1930 is now indicated in gray, rather than the extent of Prussia in 1871. Figure 2d restricts the treatment group to the 'non-conquest' sample, or the sample of municipalities that became part of Prussia due to historical happenstance rather than intentional expansion. The map is otherwise identical to Figure 2c.

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Figure 3: ROBUSTNESS TO BANDWIDTH CHOICE: MAIN OUTCOMES
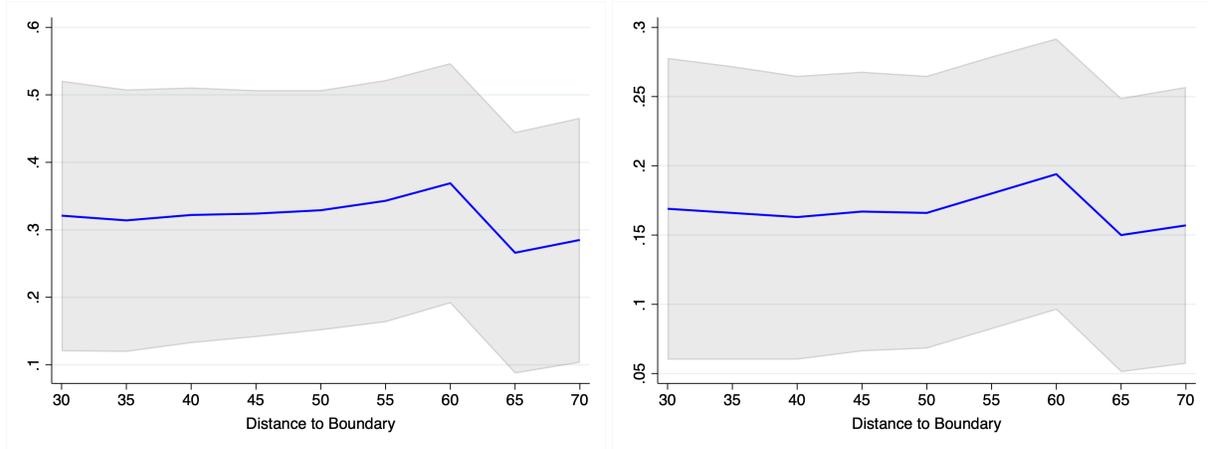
Notes: Both sub-figures plot the point estimates of β from equation (1) on the y-axis for different bandwidth values between 30 and 70 kilometers in 5 km increments (x-axis). Gray outlines show 95% confidence intervals. The left sub-figure uses the natural logarithm of deportations between 1941-1945 as the dependent variable. The regression specification corresponds to column (1) of Table 4. The right sub-figure uses the natural logarithm of the ratio between deportations between 1941-1945 and the number of bureaucrats in 1933 as the dependent variable. The regression specification corresponds to column (3) of Table 4.

Figure 4: Three train transports with deportee home municipalities

(a) Aachen-Theresienstadt (b) Munich-Lublin (c) Frankfurt-Theresienstadt
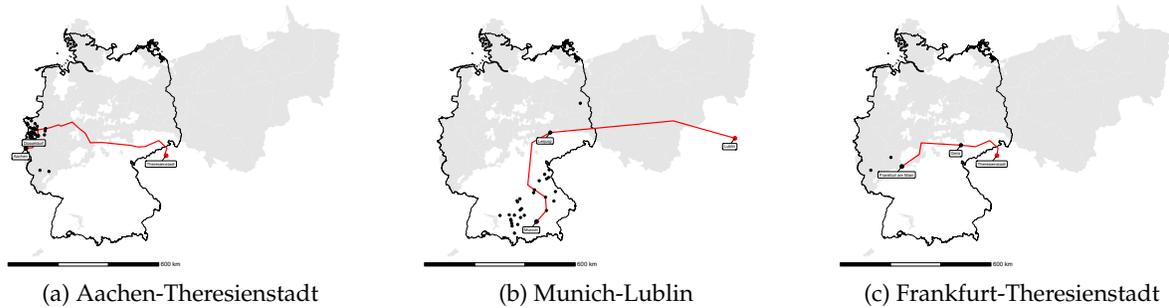
Notes: This figure plots the route of three deportations along with the municipalities deportees on each transport came from. The black outline indicates modern Germany, and the gray shade is the outline of Prussia in 1871. In sub-figure (a) I plot a deportation that left Aachen on 07/25/1942 and arrived in Theresienstadt the next day. The transport included German Jews from neighboring municipalities, indicated on the map. Several German Jews that were supposed to be on the transport could not be found by the local Gestapo. In sub-figure (b) I plot a deportation that took place between 04/03/1942 (departure date Munich) and 04/06/1942 (arrival date Lublin). This deportation included German Jews from many municipalities around Munich, indicated in the Figure, who were deported to Munich up to two weeks before the actual transport. The transport was organized by the local Gestapo, in cooperation with the ministry of security (RSHA), the police, as well as local bureaucrats, such as local counselor (*Regierungsrat*) Schrot. Several deportees that were on a transport list were crossed out and spared, and others were picked up along the way, in for example Regensburg. The route of the train in Poland is difficult to reconstruct, and I have therefore indicated it with a straight line. In sub-figure (c) I plot a transport that left Frankfurt am Main on 08/18/1942 and arrived at Theresienstadt ghetto the next day. This transport too deported German Jews from different surrounding municipalities. For a full description of each deportation see the 'The Deportations of Jews Research Project and Digital Database ("Transports to Extinction")' on www.yadveshem.org. The deportations referred to here are deportation numbers 5092393, 5604922, and 5092427.

Figure 5: NO PRE-TRENDS IN DEPORTATIONS BY TRAIN INSIDE OR OUTSIDE PRUSSIA
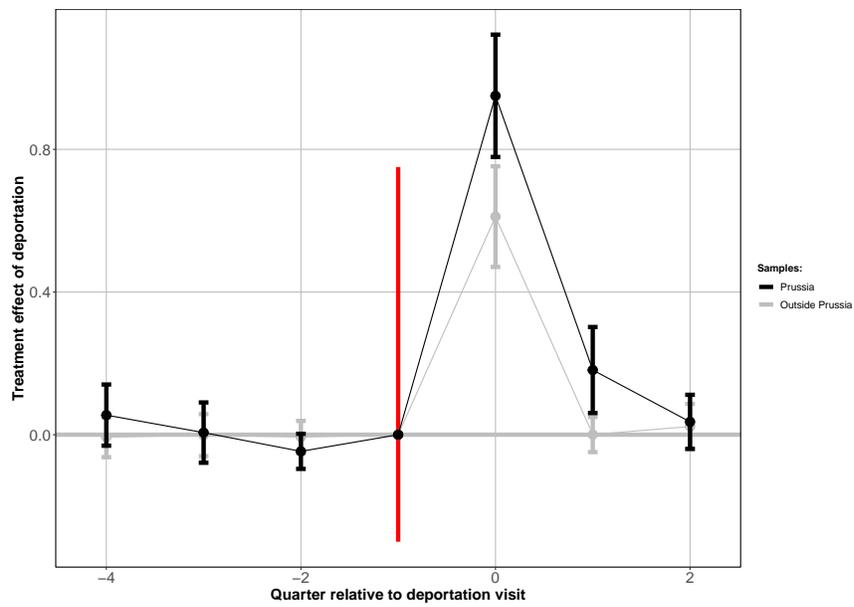
Notes: This figure plots estimated coefficients and 95% confidence intervals for treatment effects relative to a treatment period 0 when a transport arrived in a municipality using the De Chaisemartin and d'Haultfoeuille (2020) estimator. I plot these coefficients separately for formerly Prussian parts of Germany and non-Prussian parts.

Table 1: DESCRIPTIVE STATISTICS

	Full Sample					Bandwidth				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Prussian	1009	0.52	0.50	0	1	528	0.50	0.50	0	1
					Nazi					
Log Deportations 1941-1945	1006	1.52	1.68	0	11	525	1.71	1.66	0	9
Unemployment Welfare Recipients 1935	250	3293	10334.27	9	137676	130	2480	6458.61	9	56735
Log Local Tax p.c. 1936	439	4	0.61	1	11	239	4	0.76	1	11
					Weimar					
Log Pre-Nazi violence 1930-1933	1006	0.74	1.28	0	12	525	0.77	1.31	0	12
Unemployment Welfare Recipients 1930	234	2581.68	8246.47	13	115558	125	1821.04	3344.37	13	20791
Log Local Tax p.c. 1929	217	3.84	0.51	2	6	113	3.92	0.53	2	6

Notes: The unit of observation is a German municipality. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. Log deportations 1941-1945 is the natural logarithm of the number of German Jewish citizens deported between 1941 and 1945. Pre-Nazi violence is the natural logarithm of the number violent acts perpetrated against German Jews between 1930 and 1933. Unemployment Welfare Recipients is the sum of all municipal welfare recipients as of 31 December 1930 for the Weimar period and 28 February 1935 for the Nazi period. Local Tax is the natural logarithm of total local taxes per capita in 1929 for the Weimar period and in 1936 for Nazi. Data are in 1929 prices.

Table 2: BALANCE CHECKS: PRUSSIA DOES NOT LOOK DIFFERENT BEFORE ITS ESTABLISHMENT

Dependent variable:	Population		Jewish life		Geographic Isolation		Economic & Political Openness	
	Population 1600 (1)	Growth rate 1500-1600 (2)	Jewish comm. 1349 (3)	Pogrom 1349 (4)	High ruggedness (5)	River nearby (6)	In Hanseatic League (7)	Free Imperial City (8)
Prussian	-1.76	-0.15	0.06	0.05	0.01	0.08	0.02	-0.01
	(1.66)	(0.15)	(0.06)	(0.06)	(0.05)	(0.07)	(0.04)	(0.04)
Mean dep. var.	7.14	0.37	0.56	0.33	0.57	0.51	0.10	0.10
Observations	88	68	307	307	307	307	307	307
R^2	0.66	0.24	0.15	0.20	0.56	0.03	0.17	0.14
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. Population in 1600 is the total population of a municipality in 1600 (in 1000s). Population growth rate 1500-1600 is the population growth rate in a municipality between 1500 and 1600. Jewish community indicator 1349 is an indicator equal to 1 if a Jewish community existed in 1349. Pogrom 1349 is an indicator equal to one if at least one city in the municipality experienced a pogrom in 1349. High ruggedness is equal to 1 if the municipality features above median ruggedness. River nearby is an indicator variable equal to one if the municipality was on or near a river. In Hanseatic League is an indicator variable equal to one if the municipality was part of the Hanseatic League. Free Imperial City is an indicator variable equal to one if the municipality was a Free Imperial City. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 3: BALANCE CHECKS: PRUSSIA IS UNCORRELATED WITH JEWISH PRESENCE, NAZI VOTES

Dependent variable:	German Jewish population, faith, and repression										Nazi support			
	Inhabitants			Emigration			Mischlinge 1939		Pre-Nazi violent act		Log Vote share Nazi party			
	1925 (1)	1933 (2)	1939 (3)	1920-32 (4)	1933-39 (5)	1939-40 (6)	1st Dg. (7)	2nd Dg. (8)	Judensau sculptures (9)	Hep-Hep riots (10)	1930 (11)	1932 (*Jul*) (12)	1932 (*Nov*) (13)	1933 (14)
Prussian	0.08	-0.00	0.06	-0.08	0.01	0.03	-0.01	-0.01	-0.00	-0.02	-0.07	-0.03	-0.04	0.01
	(0.08)	(0.09)	(0.08)	(0.07)	(0.08)	(0.08)	(0.06)	(0.07)	(0.02)	(0.02)	(0.05)	(0.04)	(0.04)	(0.03)
Mean dep. var.(levels)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.03	.2	.42	.37	.48
Observations	525	525	525	525	525	525	525	525	525	525	524	520	488	488
R^2	0.45	0.29	0.40	0.45	0.39	0.37	0.62	0.54	0.17	0.27	0.18	0.11	0.08	0.04
POPULATION	✓	✓	✓	✓	✓	✓	✓	✓						
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. All estimated coefficients in columns (1)-(8) are standardized. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. German Jewish inhabitants is the natural logarithm of the number of German Jewish citizens in 1925, 1933, and 1939. Emigrations is the number of German Jewish citizens who emigrated between 1920-1932, 1933-1939 and 1939-1940. Mischlinge is the natural logarithm of the number of Mischlinge of degree 1 and 2 in 1939. Judensau sculptures is an indicator for the persistence of a Judensau sculpture. Hep-hep riots is the number of pogroms that occurred during the 1819 Hep-Hep riots. Nazi support is the natural logarithm of the fraction of people who voted for the NSDAP in the Reichstag elections of September 1930, July 1932, November 1932 and March 1933. Population denotes the natural logarithm of total population in 1925 for the 1925 German Jewish population data and for German Jewish emigration data between 1920–1932; the natural logarithm of total population in 1933 for the 1933 and 1939 German Jewish population data as well as for German Jewish emigration data between 1933–1939 and 1939–1940; and the natural logarithm of German Jewish population in 1939 for the Mischlinge data. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 4: THE EFFECT OF PRUSSIA ON DEPORTATIONS AND VIOLENCE

Dependent variable:	Nazi Deportations				Pre-Nazi violence
	Log deportations 1941-1945		Log dep. per bur. 1941-1945 ($\times 100$)		Log events 1930-1933
	(1)	(2)	(3)	(4)	(5)
Prussian	0.37***	0.37***	0.16***	0.16***	0.05
	(0.10)	(0.09)	(0.05)	(0.05)	(0.05)
Mean dep. var. (levels)	70	70	2	2	.77
Observations	524	524	524	524	524
R^2	0.71	0.71	0.55	0.55	0.33
GESTAPO PRESENCE		✓		✓	
NON RESIDENT DEPORTATIONS	✓	✓	✓	✓	
NSDAP IN GOVERNMENT	✓	✓	✓	✓	
NSDAP VOTES	✓	✓	✓	✓	✓
JEWISH POPULATION	✓	✓	✓	✓	✓
ECONOMIC STRUCTURE	✓	✓	✓	✓	✓
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Log deportations 1941-1945 is the natural logarithm of the number of German Jewish citizens deported between 1941 and 1945. Dep. per bur. ($\times 100$) 1941-1945 is the natural logarithm of total deportations between 1941-1945 divided by the size of the beamte measured in the 1933 census. Pre-Nazi violence is the natural logarithm of the number violent acts perpetrated against German Jews between 1930 and 1933. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. Gestapo presence is an indicator equal to one if a Gestapo office was located in that municipality. Non resident deportations is the number of German Jewish deportees who were deported from a municipality, but originated elsewhere. They were often first forcibly moved within Germany before being deported. NSDAP in government is the number of NSDAP members employed in bureaucratic or political jobs who joined the party before 1939. NSDAP votes is the natural logarithm of the of people who voted for the NSDAP in the Reichstag elections of September 1930. Jewish population is the natural logarithm of the number of German Jewish citizens in 1925 for violent events and in 1939 for deportations. Economic structure comprises two variables. The first is the share of citizens employed in the secondary, industrial, sector and the second is the share of citizens employed in the tertiary, service, sector. I use data from the 1925 census for violent events and from the 1933 census for deportations. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 5: THE EFFECT OF PRUSSIA ON DEPORTATIONS IN A MUNICIPALITY PANEL

Dependent variable:	Nazi Deportations			
	Log deportations 1941-1945		Log dep. per bur. 1941-1945 ($\times 100$)	
	(1)	(2)	(3)	(4)
Prussian $\times$ visited in month m	0.30***	0.29***	0.08***	0.08***
	(0.10)	(0.10)	(0.03)	(0.03)
Mean dep. var. (levels)	2.41	2.41	.03	.03
Observations	47282	47282	47282	47282
R^2	0.56	0.59	0.37	0.38
INFRASTRUCTURE TREND		✓		✓
GEOGRAPHIC TREND	✓	✓	✓	✓
JEWISH POPULATION REMAINING	✓	✓	✓	✓
NON RESIDENT DEPORTATIONS	✓	✓	✓	✓
FIXED EFFECTS	✓	✓	✓	✓

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality in a month. Months range from the June 1941 up to and including April 1945. Prussian × visited in month m is an indicator equal to one if a municipality was visited by a transport in month m and is located in former Prussia. Log deportations 1941-1945 is the natural logarithm of the number of German Jewish citizens deported in each month between October 1941 and March 1945. Dep. per bur. (×100) is the total deportations in a month divided by the size of the beamte measured in the 1933 census. Infrastructure trend is the interaction of the total number of visits received by each municipality during the sample period with time indicators. Geographic controls include individual trends for the three largest cities inside Prussia, Berlin, Köln and Essen, and outside Prussia, Hamburg, Leipzig and München. Jewish population remaining is the natural logarithm of the number of German Jewish citizens remaining at the start of the prior month based on 1939 census, deportation and emigration data. Values below zero, i.e. indicating more individuals were deported from a location than the resident population, are set to zero. Non resident deportations is the number of German Jewish deportees who were deported from a municipality in a month, but originated elsewhere. They were often first forcibly moved within Germany before being deported. Fixed effects include municipality fixed effects, month-year fixed effects and Prussia x month fixed effects. Heteroskedasticity robust standard error clustered at the municipality level are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 6: THE EFFECT OF PRUSSIA ON DEPORTATIONS IN A TRAIN PANEL

Dependent variable:	Nazi Deportations			
	Log deportations 1941-1945		Log dep. per bur. 1941-1945 ($\times 100$)	
	(1)	(2)	(3)	(4)
Fixed effects:	Route	Train	Route	Train
Prussian	0.71***	0.74***	0.12***	0.12***
	(0.13)	(0.13)	(0.04)	(0.04)
Mean dep. var. (levels)	258.54	219.86	1.03	1.02
Observations	2082	1980	2082	1980
R^2	0.83	0.83	0.68	0.69
INFRASTRUCTURE	✓	✓	✓	✓
JEWISH POPULATION REMAINING	✓	✓	✓	✓
NON RESIDENT DEPORTATIONS	✓	✓	✓	✓
FIXED EFFECTS	✓	✓	✓	✓

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a train stop. Train stops are either an actual train station, or a place form which deportees were transported to a train by a truck or a van. Months range from the October 1941 up to and including March 1945. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. Log deportations 1941-1945 is the natural logarithm of the number of German Jewish citizens deported in each month between June 1941 and April 1945. Dep. per bur. ($\times 100$) is the total deportations in a month divided by the size of the beamte measured in the 1933 census. Infrastructure is the total number of visits received by each municipality in each month. Jewish population remaining is the natural logarithm of the number of German Jewish citizens remaining at the start of the prior month based on 1939 census, deportation and emigration data. Values below zero, i.e. indicating more individuals were deported from a location than the resident population, are set to zero. Non resident deportations is the number of German Jewish deportees who were deported from a municipality in a month, but originated elsewhere. They were often first forcibly moved within Germany before being deported. Fixed effects include route fixed effect for columns (1) and (3), train fixed effect for columns (2) and (4) and train stop characteristics (whether a stop was a train stop or a truck/van stop, and the stop order number among all stops), and month-year fixed effects for all columns. Heteroskedasticity robust standard error clustered at the train level are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 7: PRUSSIA AND STATE CAPACITY

Dependent variable:	Unemployment Welfare recipients		Tax raised per capita		Specialization (*HH Index*)	
	Weimar (1)	Nazi (2)	Weimar (3)	Nazi (4)	Full (5)	Relevant (6)
Prussian	0.1*	0.1***	0.5**	0.5***	-0.6***	-0.6**
	(0.0)	(0.0)	(0.2)	(0.1)	(0.2)	(0.2)
Observations	125	130	113	239	90	92
R^2	0.92	0.98	0.18	0.19	0.36	0.17
EMPLOYMENT	✓	✓			✓	✓
ECONOMIC STRUCTURE	✓	✓	✓	✓	✓	✓
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. All estimated coefficients are standardized. Unemployment Welfare Recipients is the sum of all municipal welfare recipients as of 31 December 1930 for the Weimar period and 28 February 1935 for the Nazi period. Local Tax is the natural logarithm of total local taxes per capita in 1929 for the Weimar period and in 1936 for Nazi. Data are in 1929 prices. Specialization (*HH Index* for Herfindahl-Hirschman Index) is a measure of employment concentration constructed using the number of employees across the full or relevant bureaucracy within the local government. Employment categorize two different variables. For HH Index calculations, I use the total number of local government employees across all occupation categories, both white and blue collar. For the relevant HH Index, I restrict to white collar employees employed in police administration, tax, finance and property administration, general administration, and construction management. For the Unemployment Welfare recipients, employment measures the total number of unemployed normalized by population in the 1925 census for the Weimar period and in the 1933 census for the Nazi period. Economic structure comprises two variables. The first is the share of citizens employed in the secondary, industrial, sector and the second is the share of citizens employed in the tertiary, service, sector. I use data from the 1925 census for Weimar period data and from the 1933 census for Nazi period data. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 8: ALTERNATIVE MECHANISMS AT THE STUDY BOUNDARY

Dependent variable:	Great Depression:		Social Clubs:		Radio Listeners:		Religion:	
	Employment growth 1925-1933 (1)	Unemployment 1933 p.c. (2)	Civic clubs p.c. (x1000) (3)	Military clubs p.c. (x1000) (4)	In 1932 p.c. (5)	In 1933 p.c. (6)	Catholic share 1925 (7)	Protestant share 1925 (8)
Prussian	-0.04	-0.00	0.00	0.06	0.00	-0.00	0.03	-0.03
	(0.05)	(0.00)	(0.10)	(0.05)	(0.01)	(0.01)	(0.02)	(0.02)
Mean dep. var.	1.54	0.07	0.53	0.27	0.23	0.27	0.23	0.74
Observations	469	469	119	112	462	464	528	528
R^2								
INFRASTRUCTURE					✓	✓		
ECONOMIC STRUCTURE	✓	✓	✓	✓	✓	✓	✓	✓
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. Employment growth is percentage growth in total employment between the 1925 and 1933 census. Unemployment 1933 p.c. is the number of unemployed individuals in 1933 divided by total population in 1933. Civic clubs p.c. ($\times$1000) is the ratio of non-military clubs per 1000 inhabitants in 1925. Military clubs p.c. ($\times$1000) is the ratio of military clubs per 1000 inhabitants in 1925. Radio listeners per capita (x10k) in 1932 is radio subscriptions per 10,000 people in 1932. Radio listeners per capita (x10k) in 1933 is radio subscriptions per 10,000 people in 1933. Catholic share 1925 is the fraction of the population that is Catholic in the 1925 census. Protestant share 1925 is the fraction of the population that is Protestant in the 1925 census. Infrastructure is the radio signal strength for that respective year. Economic structure is the share of citizens employed in the industry and in the tertiary sector in 1925. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

Table 9: CONDITIONS UNDER WHICH STATE CAPACITY CAN BE ABUSED

Dependent variable:	Log deportations 1941-1945				
	(1)	(2)	(3)	(4)	(5)

PANEL I: EXTERNAL FACTORS

		ELECTORAL OVERSIGHT High Nazi vote margin		IDEOLOGY OF PRINCIPAL Early deportations	
Sample:	Full sample	High	Low	High	Low
Prussian	0.37***	0.28**	0.13	0.52***	0.31**
	(0.10)	(0.12)	(0.16)	(0.13)	(0.13)
Mean dep. var. (levels)	70	9	142	27	115
Observations	524	219	220	270	254
R^2	0.71	0.64	0.78	0.70	0.76

PANEL II: INTERNAL FACTORS

		HIERARCHY White/Blue collar 1933		ASSIDUITY Beamte/White collar 1933	
Sample:	Full sample	High	Low	High	Low
Prussian	0.37***	0.44***	0.26**	0.59***	0.22
	(0.10)	(0.16)	(0.11)	(0.13)	(0.14)
Mean dep. var. (levels)	70	133	6	34	105
Observations	524	263	261	262	262
R^2	0.71	0.73	0.58	0.67	0.76
NON RESIDENT DEPORTATIONS	✓	✓	✓	✓	✓
NSDAP IN GOVERNMENT	✓	✓	✓	✓	✓
NSDAP VOTES	✓	✓	✓	✓	✓
ECONOMIC STRUCTURE	✓	✓	✓	✓	✓
JEWISH POULATION	✓	✓	✓	✓	✓
GEOGRAPHIC CONTROLS	✓	✓	✓	✓	✓
Bandwidth	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Log deportations 1941-1945 is the natural logarithm of the number of German Jewish citizens deported between 1941 and 1945. Prussian is an indicator variable equal to one for each municipality that was part of Prussia at the time of the establishment of unified Germany in 1871 and zero otherwise. For Electoral Oversight I measure whether a municipality has an above median vote share for the Nazi party in 1933. I restrict the sample to municipalities where the Nazi party was the largest party. For Ideology of the Principal I measure pre-1940 deportations, normalized by 1933 German Jewish population, in that Gau. For Hierachy I compute the ratio of Beamte and Angestelle to Arbeiter in 1933. For Assiduity I measure the ratio of Beamte to Angestelle in 1933. Non resident deportations is the number of German Jewish citizens deported from a different place than their residence. NSDAP in bureaucracy is the number of NSDAP members employed in bureaucratic or governmental jobs who joined the party before 1939. NSDAP votes is the natural logarithm of the fraction of people who voted for the NSDAP in the Reichstag elections of September 1930. The electoral oversight specifications do not control for this variable. Economic structure is the share of citizens employed in the industry and in the tertiary sector in 1933. Jewish population is the natural logarithm of the number of German Jewish citizens in 1939. Geographic controls include latitude and longitude at the centroid of a municipality, indicator variables for the three largest cities within the bandwidth inside Prussia, Hannover, Frankfurt and Magdeburg, and outside Prussia, Hamburg, Leipzig and Dresden, and indicator variables for the nearest Prussian boundary segment. Bandwidth shows the distance to Prussia's outer border in 1871 below which a municipality is included in the regression sample. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.