BUREAUCRACY AS A TOOL FOR POLITICIANS: EVIDENCE FROM GERMANY

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Abstract

This paper studies the impact of a well-functioning bureaucracy on the effectiveness of repression, in the context of Germany's Nazi regime. I compare former Prussian to non-Prussian municipalities within unified Germany in a regression discontinuity framework. When the Nazis persecuted the German Jews, Prussian areas implemented deportations of Jews more efficiently. During the Weimar republic, when Jews were legally protected, violence against Jews is lower in former Prussian areas. In both periods, Prussian local governments had greater 'capacity': They were more effective at raising taxes and collecting trash. Capacity derived from greater specialization and better information processing rather than from effort. Specialization may have created the moral wiggle room to implement repugnant directives.

Keywords: State capacity, bureaucracy, Nazi Germany **JEL classification**: N47, N57, N45

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1 Introduction

Governments that are better at implementing public policy are typically found in more developed countries (Finer, 1999; Migdal, 1988). This observation forms the basis of a literature studying the impact of 'state capacity' on economic development (Besley and Persson, 2010, 2011b; Acemoglu et al., 2015; Dell et al., 2018), as well as a literature studying the incentives successful bureaucracies create (Colonnelli et al., 2020; Khan et al., 2019). Naturally, politicians governing such states may direct a bureaucracy towards repression of its citizens, rather than pursue pro-development policies. In this paper, I study the effect of a well-functioning bureaucracy when politicians pursue repression.

Bureaucrats may act as a check on a repressive government by prioritizing the interests of citizens over their principal's interests (Prendergast, 2007; Ashraf and Bandiera, 2018), or by refusing to implement repugnant directives. Alternatively, an influential 'cog in the wheel' view emphasizes how seemingly mundane and routine bureaucratic tasks may add up to large scale repression (Arendt, 2006).

The connection between state capacity and repression is difficult to study because it requires a setting in which repression is government policy. Furthermore, anticipating non-cooperation, politicians may stack a bureaucracy with ideologues, making it hard to distinguish capacity from ideology or effort (Glaeser, 2005). Finally, repression may be illegal and therefore outside the mandate of the bureaucracy, forcing politicians to turn to the army or party militias (Besley and Persson, 2011a).

This paper studies the effect of a well-functioning bureaucracy in a quintessential case of government-organized repression: The seizure of power by the Nazis in Germany. This setting allows me to exploit two natural experiments in Germany's history. First, I use the fact that Prussia unified Germany in 1871. The strong and highly bureaucratized Prussian state became the national German state, and I hypothesize that this created local variation in state capacity between those parts of Germany that were formerly Prussian, and those that were not. Second, I capture policy variation by focusing on the fact that in 1933, when the Nazis came to power, government policy towards Germany's Jews changed radically. After stripping the German Jews of their citizenship, the Nazi regime organized large-scale deportations to ghettoes and concentration camps, especially from 1941 onwards. Before 1933, German Jews were equal before the law and were legally protected.[1]

Since exposure to Prussia varies discontinuously at its former borders, 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, before and after the accession to power by the Nazi party, within 50 kilometers of Prussia's former borders.

[1] Technically, Jews were considered a separate social class during the Weimar Republic. They lost their citizenship rights as part of the infamous 1935 Nuremberg laws.

To study the effectiveness of Nazi repression, I use newly collected municipality-level data on deportations. Conditioning on the 1939 local German Jewish population, I find that deportations are implemented more effectively: Being formerly Prussian is associated with more deportations, both in levels and *per bureaucrat*. Importantly, using data on individual Nazi party members, secret police (*Gestapo*) train transports and on popular anti-Semitism, I show that these results hold conditional on the presence or activity of other actors involved in the deportations. Being Prussian is associated with a 44 percent increase in the effectiveness of deporting Jews. Although the physical transport of German Jews onto trains and out of Germany was enforced by the *Gestapo*, individual bureaucrats were asked to implement policies that aided the deportations. These ranged from recording and updating the whereabouts of local German Jews and the preparation of deportee lists to the organization of within city transports to the train stations from where the Gestapo transports left. I contrast this result with the effect of having been Prussian before repression was government policy. With data measuring anti-Semitic violence, I find that being Prussian is associated with reducing the number of attacks on Jews before 1933 to zero.

To establish a causal interpretation of these results I pursue several strategies. First, I show that becoming Prussian is uncorrelated with development and anti-Semitism before the founding of Prussia. 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 a medieval pogrom.

The rest of this paper studies mechanisms. I test the hypothesis that the effect of Prussia operates mainly through a better functioning bureaucracy, before testing alternatives. I first study 'fiscal capacity' as a standard measure of the effectiveness of the bureaucracy (Besley and Persson, 2011b). Using archival data on public administration, I measure the amount of tax raised per capita, and the amount of tax raised per *Reichsmark* spent on public administration. I find that fiscal capacity is higher in Prussian regions. Crucially, because tax collection objectives did not change with the seizure of power by the Nazis, I find the same result before and after 1933. The effectiveness of local government extends to mundane policy implementation as well: Prussian municipalities collect trash more efficiently. It is of

course possible that the effect of being Prussian operates through more mechanisms than just local state capacity. I study cultural norms for rule-following, education, 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 by and large, *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. Moving away from the study boundary, I estimate heterogeneous effects of Prussian status in a sample encompassing all of Germany. I find that social capital and Protestantism substitute for the capacity of the state, likely through increased popular support for the Nazi regime. On balance, at the study boundary, local state capacity is a central mechanism sustaining the impact of Prussia.

What is it about the Prussian bureaucracy that made it more effective seventy years after Prussia ceased to exist as a polity? The historical literature on the role of local government in the Holocaust emphasizes the role municipalities had in information processing and preparing transports but does not provide guidance on within-Germany variation. I test the hypothesis that Prussian bureaucracy was more functionally (or horizontally) specialized and this aspect of the internal organization of bureaucracies persisted, leading to greater efficiency (Clark, 2006; Garicano and Van Zandt, 2012). To do so, I construct two measures of *within*-municipality specialization of the bureaucracy, holding constant its size: The number of distinct job categories held by local bureaucrats and a Herfindahl index of the concentration of bureaucrats across occupied job categories. I find that Prussian areas have more job categories and are less concentrated - and therefore more specialized - across job categories. I show that this effect is concentrated in white-collar jobs such as overseeing police and tax administration, and not in blue-collar jobs such as park maintenance. I then show that the increased efficiency of white-collar workers results in more timely record keeping. The natural alternative to specialization as an explanation for differences in state capacity is differences in effort or, in the context of this paper, anti-Semitic or Nazi ideology. I measure the penetration of the Nazi party into the local government and the local bureaucracy. In addition, I measure cultural anti-Semitism in the local population. Finally, I measure other aspects of local bureaucratic organization, like hierarchy (Bandiera et al., 2021) and the ideological commitment of bureaucrats the state (Spenkuch et al., 2023). Around the study boundary, I find no differences in these alternative explanations. As before, I then move away from the study boundary, and find that in a sample covering all of Germany, the effect of higher state capacity on deportations are stronger where local ideology was more aligned with the Nazi ideology, and in more hierarchical bureaucracies.

Taken together, these results are consistent with the hypothesis of this paper: Prussian areas inherited a local state with higher capacity. When the objectives of the government included protection of Jews,

greater capacity to implement policy led to lower violence. When the policy objectives changed to persecution and, ultimately, genocide, these objectives were executed more effectively as well. These results hold some interesting implications for the study of state capacity. The Nazis emphasized the importance of legalizing any persecution before executing it. My pre-1933 results suggest that bureaucrats may have refused had policy not be legalized, pointing to the importance of 'legal capacity' not just as a component of state capacity (Besley and Persson, 2011b), but as a determinant as well. Furthermore, the Prussian bureaucracy had not developed capacity in deportations before. Instead, the capacity it had was fungible. My results on information processing and specialization suggest that fungibility is likely due to similarity of *tasks* across objectives. The government's involvement in the deportations was administrative, involving tasks that were similar to, for example, the tasks necessary for tax collection. In the German context, the similarity between the Holocaust bureaucracy and the pre-Nazi bureaucracy was central to Hilberg (1961).

Task-similarity and my findings on specialization relate to a large literature in social science that has asked the question why German bureaucrats willingly aided in the organization of the Holocaust. While many bureaucrats were Nazi party members, those that were less ideological or that weren't members would need to (morally) justify such actions to themselves (Bénabou et al., 2018). I discuss the literature on the idea that a concomitant effect of specialization is diffusion of responsibility (Hamman et al., 2010; Ellman and Pezanis-Christou, 2010; Latane and Darley, 1968). The idea is that morally repugnant directives, when executed in a specialized bureaucracy, are less repugnant to the individual because smaller subtasks are less identifiable with the final output. Ghettoization, for example, was a task shared between desk workers at the municipal housing and taxation offices, as well as the local police. Nevertheless, it was clear that filling in a name on a deportee list, although similar to regular tasks, led to a potentially repugnant outcome. One explanation for this is the link between specialization and a replacement logic: Tasks are more defensible to oneself if otherwise 'someone else would have done it' (Falk et al., 2020). These observations also link my results to Hannah Arendt's notion of 'the banality of evil' (Arendt, 2006) More broadly, being a specialized 'cog in the wheel' was often brought up by Nazis as a defense for their role in organizing the Holocaust.[2]

This paper aims to make several contributions. First, my result contributes to the literature on state capacity, pointing out that the effect that a 'strong' state is conditioned by policy (Besley and Persson,

[2]The term cog in the wheel most closely associated with the trial of Adolf Eichmann, the highest ranking bureaucrat responsible for the Holocaust to survive the war. During his trial in 1967, he repeatedly used the defense that he was but a cog in the wheel, a bureaucrat executing tasks that were so specialized that he could not possibly be held responsible. At one point during the trial, Eichmann stepped out of his glass defendant's booth to show an organizational chart of his ministry. He thought it would be obvious to the prosecutors that the differentiation of the responsibilities within the Nazi bureaucracy would establish his innocence (Breton and Wintrobe, 1986). His bureaucratic mindset and insistence on rules and procedure, together with his unremarkable appearance and demeanor led Hannah Arendt to coin the term 'the banality of evil' based on the trials. See Arendt (2006).

2010, 2011b; Dell et al., 2018; Acemoglu et al., 2015). I also contribute to this literature by pointing to the similarity of tasks across domains to make state capacity fungible across new objectives. My results point to the internal organization of government as an important component of state capacity. Through this result, my paper is part of a growing literature that uses micro-data to study the historical development of bureaucracies as well as to a literature that studies incentives for bureaucrats (Dal Bó et al., 2013; Khan et al., 2016; Finan et al., 2017; Xu, 2018; Khan et al., 2019; Colonnelli et al., 2020). Finally, in the theoretical literature on state capacity, the anticipation of expropriation or repression is a key determinant of underinvestment in state capacity. In economics, the main articulation of this mechanism is from Besley and Persson (2010) and Besley and Persson (2011b). Besley & Persson focus on the anticipation of higher taxation. In political science, authors such as Christensen and Laitin (2019) emphasize future repression. This paper complements this literature by providing empirical evidence for this mechanism. In pre-Nazi Germany future state capacity was used to implement policies that were likely undesirable when capacity was built up. 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). My results make a separate contribution in the forensic debate on the organization of the Holocaust in history. 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 repression.[3]

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 results for violence. Section 6 studies state capacity as a mechanism. Section 7 studies alternative mechanisms.

[3]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.

Section 8 studies bureaucratic specialization as a source of state capacity. Section 9 presents a discussion of the literature on diffusion of responsibility. 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'. Third, I discuss the organization of the deportation of the German Jews by the Nazis between 1933 and 1945.

2.1 Prussian expansion and state capacity

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.[4] 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 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'.

I map the outer borders of Prussia in 1871 in Figure 1. An important aspect of Prussia's growth into its final extent was that expansions did not always occur intentionally, some were the consequence of unexpected deaths of local rulers without male heirs which bequeathed land to Prussia it had not intended to annex, purchase, or conquer. For example, 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

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

as the Duchy of Prussia. When, through a strategic marriage, Brandenburg - the German territory that would be the heartland of the Kingdom of Prussia later on - acquired the Duchy of Prussia, they got Cleve as a new territory as well. 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. Other territories, like Silesia in 1742, were outright conquered. 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. Online Appendix sections 4-5 detail every expansion, and whether I interpret it as accidental, or as intentional.

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. I now briefly review the main events that led to the buildup of Prussian state capacity. I provide a more detailed history in the Appendix.

The consolidation of Prussia's state capacity 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.

I naturally face the question of what happened in terms of state formation in the many larger and smaller states outside Prussia. While the larger states such as Bavaria, Baden, and Wuerttemberg were

modernizing as well, the consensus among historians is that this process had been much more sustained in Prussia (Nolte, 1990). The superiority of the organization of Prussia was manifested in its military victories over the French, Austrian, and the other German states, as well as in the fact that the government of newly unified Germany was essentially the Prussian bureaucracy with unified leadership. After presenting the main results, I will present public finance results that validate the claim that Prussia was better organized than the rest of Germany.

2.2 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 (see e.g. Blau (1950) for a discussion).[5] At the end of the War, there were about 19,000 German Jews left in Germany, and the 1946 census counted about 26,000.[6]

Emigration and deportation were the two primary ways through which this decline 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.

Emigration, 1933-1941. The Nazi government legalized and implemented a systematic marginalization of the German Jews. Although the Party's paramilitary arm, the *Sturmabteilung* (SA) initially harassed German Jews throughout Germany, the Nazis opposed mob violence, and studied new laws removing Jewish rights extensively before implementing them (Friedländer, 2009, p. 162, 203). This effort resulted in the Nuremberg laws of 1935, stripping Jews of German citizenship, and removing them from the routine protection of law enforcement. It also resulted in the decapitation of the SA in the 'Night of the Long Knives' in favor of the more bureaucratic *Schutzstaffel* (SS). Until the start of the War, German Jews from were removed from public office and from public life. Finally, the impoverished Jews were 'relocated' into 'Jewish houses' (*Judenhäuser*), and systematically expropriated. As a result of this repression, German Jews emigrated abroad. Their migrations were heavily taxed, and permits were hard to obtain, but nevertheless 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, 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 *Reichssicherheitshaup-*

[5]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. Blau (1950) provides a discussion.

[6]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 drop in the total number of German Jews can be interpreted as the drop for Germany. For statistics, see Blau (1950) or Arendt (2006).

tamt). Adolf Eichmann led one of the responsible departments for the deportations within the RSHA. At the local level, the secret police of the SS (Gestapo, or *Geheime Staats Polizei*) was responsible for rounding up and transporting Jewish citizens.[7] Now I will discuss that, when a deportation was decided upon, municipal government would not only help with information, but would actively participate in the organization of the transport.

Deportations were prepared by the same actors: the Gestapo, the Nazi party, and the local municipal government, potentially 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.[8] The quota was set to fill up transportation capacity, 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 knew about the local Jewish community. 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.[9] This process was far from perfect. Miscoordination between the municipal

[7]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 implementors of the law (Mommsen, 1966; Matzerath, 1970).

[8]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.

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

government and the Gestapo sometimes led to German Jews' survival: In September 1942, the Gestapo 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 from Leipzig. 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).[10] 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. This observation forms the basis of the hypothesis of this paper that *local* differences in municipal state capacity were important for the effectiveness of repression and deportations.[11] I study this hypothesis using data for the Nazi period. Before introducing the data sources for this paper, I briefly review policy towards German Jews just before the accession to power of the Nazis.

German Jews during the Weimar republic. 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

[10] See Stemmler (2020) for a biography of Friedrich Lehmann.

[11] Note again that 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.

Jews and prosecuted anti-Semitic acts. I study whether violence against Jews is lower in Prussian areas in this period, and hypothesize that, because Jews were protected like other German citizens by law and public order, this effect is driven by a more effective implementation of these laws.[12]

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 government 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. Summary statistics of all variables used in this paper are in Appendix section 1.1 and a complete description of data sources is in the Online Appendix.

The unit of observation in this paper is the *Kreis*, which roughly translates as county. Sometimes counties coincided with other units, such as municipalities (or *Gemeinde*) or cities. Cities and their hinterlands were 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.

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). The Online Appendix describes this process and discusses each individual expansion in detail. Figure 1 shows Prussia, modern Germany, and the discontinuity created by the former Prussian boundaries within Germany. It also displays the municipalities as of 1930.

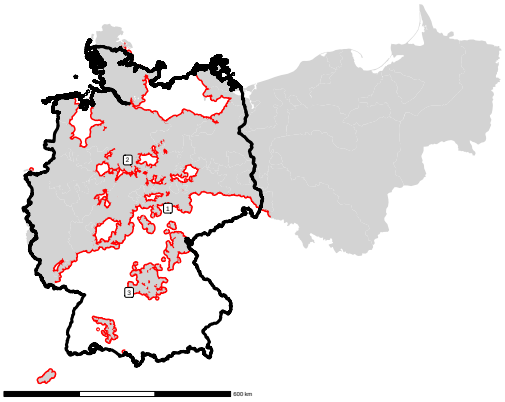
The Nazi period. The main outcome variable for this paper is a measure of the deportations of German Jews by the Nazis between 1941 and 1945. These data come from German Federal Archives (*Bundesarchiv*), which collected background information on 178,898 deported Jews.[13] Importantly, this source

[12]For example, when locals tried to desecrate the Jewish cemetery in Plauen, the leader of the municipality (the *Gemeindevorsteher*) tried "his utmost to publicly prevent these attacks" (translated by the author from the German "Mit aller Kraft versuchte der damalige Gemeindevorsteher Dr. Isidor Goldberg diese Angriffe publizistisch abzuwehren." Source: https://www.jüdische-gemeinden.de/index.php/gemeinden/p-r/1580-plauen-vogtland-sachsen.)

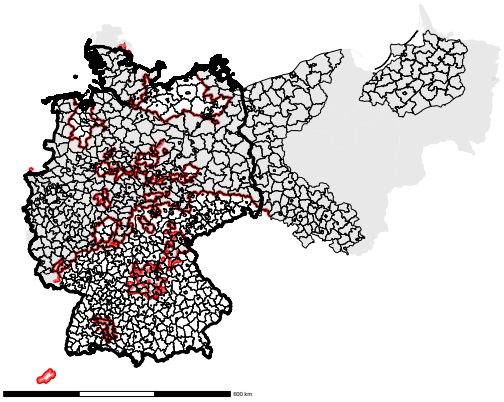
[13]These data can be accessed here: https://www.bundesarchiv.de/gedenkbuch/. Current as of May 2023.

Figure 1: MAPS SHOWING MODERN GERMANY, PRUSSIA, THE STUDY BOUNDARIES, AND SAMPLES

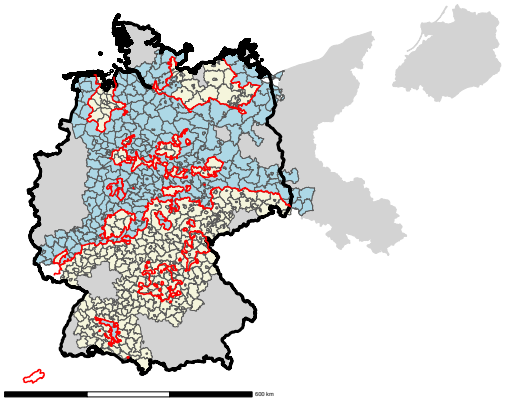
(a) Prussia, Germany and the study boundary



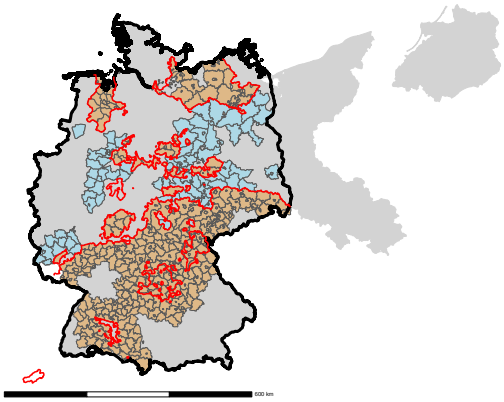
(b) Germany in 1930, and unit of observation



(c) Treatment group (blue) and control



(d) Non-conquest sample



Notes: Figure 1a displays the extent of Prussia in 1871 (in grey), the outline of modern Germany (in black), and the study boundaries (in red). Numbers indicate boundary segments. Segment (1) is the long continuous border that separates Prussia from Southern Germany. Segment (2) is the collection of boundaries between Prussia and the various areas in Northern Germany that were not Prussian. Segment (3) is the collection of boundaries between Prussian territories and non-Prussian areas in Southern Germany. Figure 1b 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 grey). Figure 1c 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 grey, rather than the extent of Prussia in 1871. Figure 1d 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 1c.

includes the location where a deportee lived at the time of the deportation, and the date of deportation. After subsetting to deportees from Germany with the requisite information I obtain a dataset of 126,043

deportees. From this dataset, I compute the total number of German Jews deported per municipality.

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. I record total taxes raised locally, total trash collected, as well as the size and composition of the bureaucratic labor force. From this data, I construct several measures of bureaucratic effectiveness. First, I normalize deportations by the number of local bureaucrats. Second, I normalize total taxes in two ways to understand differences in *effectiveness* of the local state. I first normalize by population to create a measure of total tax raised per capita. I then normalize by expenditure on running the local bureaucracy to arrive at a sharper efficiency measure: The amount of tax raised per German Reichsmark (the relevant currency) expended on raising taxes. In the Appendix, I break taxes down into their constituent parts, such as property and business taxes, but in the main body of the paper I restrict myself to aggregate variables. 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. I take my main outcome variable for this period, violence against Jews before the Nazis won the 1933 election, from Voigtländer and Voth (2012). They code indicator variables for pogroms between 1929 and 1933. I sum these variables to create a count variable of violence against Jews. The modal value is zero, and the modal non-zero value is one. Several pogroms in these data took place after the Nazis took over in January 1933. It is likely that the regime change encouraged individuals with anti-Semitic aims. What matters for this study is that, legally, Jews were still protected by law against violence.[14] In addition, I compute several measures of bureaucratic specialization, using data from Statistical Yearbooks of German Cities that is unusually detailed on the personnel data of the local government.

The postwar (placebo) period. In addition to the Nazi and Weimar periods, I collect data for the postwar period. In the years immediately following the War, the Allies centralized the implementation of public finance away from the municipalities (Diefendorf, 1993). Below I use this period as a placebo period, to understand the role of individual rule following as an alternative to state capacity in interpreting my results. As before, I collect data on the effectiveness of taxation. To measure the impact of the war, I record the total number of dwellings (houses/apartments) that were destroyed in the war, as well as the percentage decrease in the number of men in the local population between 1939 and 1946.

[14]On May 11th, 1933, Hitler was quoted as saying 'I will never agree to the existence of two kinds of law for German nationals' (Friedländer, 2009, p. 68). De facto, Jews were of course heavily discriminated against.

4 Estimation framework

In this section I introduce the estimation framework of this study. I discuss my econometric model, before discussing challenges to identification.

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 exposed to Prussian bureaucracy before German unification and some had not, and I capture the hypothesized treatment effect of this difference. I estimate the following cross-sectional model:

$$Y_{mp} = \alpha + \beta Prussian_m + f(location_m) + \sum_{i=1}^{n} segment_m^i + \gamma_d distberlin_m + \gamma_x X_m + \epsilon_{mp} \quad (1)$$

Y_{mp} is an outcome of interest for municipality m estimated for period p, $p \in \{Weimar, Nazi, postwar\}$. α is a constant, and $Prussia_m$ is an indicator variable equal to one if municipality m was part of Prussia before German unification in 1871. β is the coefficient of interest, the measured effect of being historically Prussian on outcome Y in period p. $f(location_m)$ is a function of location, controlling smoothly for the position of municipality m relative to the study boundary. Following Dell et al. (2018) I include a linear polynomial in latitude and longitude in all regressions. I vary this specification in the Appendix. $\sum_{i=1}^{n} segment_m^i$ is a vector of boundary segment fixed effects. In all specifications, I include three fixed effects here, one for each boundary indicated in Figure 1. These fixed effects ensure that I am comparing Prussian municipalities to their nearest neighbors across the relevant Prussia boundary. $distberlin_m$ is the distance to Berlin measured in kilometers, accounting for the proximity of a municipality to the capital. I refer to these covariates as my 'baseline' covariates. X_m is a vector of additional covariates, which I will introduce with the relevant regression. ϵ_{mp} is a heteroskedasticity robust error term. In the main sample, I include all municipalities within 50 kilometers from the study boundary. I map these in Figure 1 as well. Given the two-dimensional running variable, rather than computing an optimal bandwidth, I show robustness to varying the bandwidth between 15 and 100 kilometers in steps of five kilometers for my main results.[15] I estimate equation 1 using OLS. This model estimates the effect of being Prussian across the two-dimensional discontinuity created by the external border of the historical Prussian state. In order for me to interpret the estimates of β in equation 1 as causal, several assumptions need to be met. The

[15] There is no consensus on optimal bandwidth for two-dimensional running variables. Contributions have focus 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.

Table 1: BALANCE CHECKS: PRUSSIA DOES NOT LOOK DIFFERENT BEFORE ITS ESTABLISHMENT

Dependent Variable:	Population		Jewish Presence and Pogroms		Geographic Isolation		Economic & Political Openness	
	Population 1600 (1)	Growth rate 1500-1600 (2)	Jewish comm. 1349 (3)	N. pogroms pre-Black Death (4)	High ruggedness (5)	River nearby (6)	In Hanseatic League (7)	Free Imperial City (8)
				Full sample				
Prussian	-2.82	-0.07	0.07	-0.07	-0.02	0.09	0.02	-0.01
	(2.30)	(0.18)	(0.07)	(0.11)	(0.05)	(0.07)	(0.05)	(0.06)
Mean dep. var.	7.14	0.34	0.59	0.36	0.61	0.47	0.12	0.10
Observations	103	81	367	367	367	367	264	367
(Pseudo) R^2	0.07	0.12	0.09	0.12	0.41	0.02	0.12	0.07
				Non-conquest Prussia				
Prussian	-3.23	-0.23	0.18**	0.22	-0.04	0.00	0.03	-0.08
	(2.43)	(0.17)	(0.09)	(0.21)	(0.08)	(0.10)	(0.08)	(0.07)
Mean dep. var.	7.23	0.32	0.65	0.48	0.50	0.41	0.16	0.11
Observations	74	58	252	252	252	252	89	252
(Pseudo) R^2	0.11	0.15	0.05	0.14	0.19	0.02	0.14	0.11
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. 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 for if a Jewish community existed pre-Black Death. N. pogroms pre Black-Death is the number of pogroms that occurred pre-Black Death. High ruggedness is equal to 1 if the municipality is above the median in ruggedness for the respective sample; details on construction can be found in Nunn and Puga (2010). River nearby is equal to 1 if the municipality was on or near a river during the medieval times. In Hanseatic League is equal to 1 if the municipality was part of the Hanseatic League. Free Imperial City is equal to 1 if the municipality was a Free Imperial City. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

most important is that all relevant variables besides Prussian status vary smoothly at the boundary. To study this assumption, I report results from a balance test.

Balance. Table 1 studies balance. Since Prussia started centralizing after the Thirty Years War, I study balance using variables from the late medieval period and early modern periods. Panel I present estimates using the full sample, and panel II presents estimates subsetting municipalities within Prussia to those that became Prussian by accident. I refer to this sample as 'non-conquest Prussia'. To assess balance on development, I rely on city size. I run an OLS regression that uses city size information 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, $Prussian_m$. The idea of this analysis is that 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 1, 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 estimate is small and insignificant. This suggests urbanization was not concentrated in future Prussian areas.

In columns (3) and (4) I follow Voigtländer and Voth (2012) and study medieval pogroms. The authors show that medieval pogroms are correlated with early twentieth century anti-Semitism as well as persecution. 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 the number of pogroms a municipality experienced before the Black Death of 1349. 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. I take both measures from Voigtländer and Voth (2012). 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.

When I restrict to the non-conquest sample in panel II, I find similarly small and insignificant results.[16] 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.

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 the Weimar population data I use have been migrants at any point. In Table A5 in the Appendix 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. Columns (1) to (3) use nominal numbers and (4) to (6) normalize by population. Across all regressions, there is no effect of being Prussian. In the Appendix, I also show that my main

[16]The one exception is the presence of a Jewish community indicator. I control for the presence of German Jews in all subsequent regressions. I also provide a robustness check that limits the sample to municipalities that never experienced a pogrom.

Table 2: DESCRIPTIVE STATISTICS

		Non-Prussian Germany				Prussian Germany				Prussian - Non-Prussian	
	N	mean	sd	min	max	mean	sd	min	max	difference	t-stat difference
						Nazi Period					
Log deportations 1941-1945	1009	1.1	1.6	0.0	8.7	1.7	1.8	0.0	10.9	0.54	5.08***
Tax per Mark spent on (tax) administration	94	4.9	1.2	3.4	8.3	6.0	1.9	3.1	15.4	1.11	2.97***
						Weimar Republic					
Violent acts against jews before 1933	1009	0.1	0.3	0.0	2.0	0.1	0.3	0.0	2.0	-0.04	-2.44**
Tax per Mark spent on (tax) administration	94	4.9	1.2	3.4	8.3	6.0	1.9	3.1	15.4	1.11	2.97***

Notes: This table summarizes the main variables for the Weimar Republic and the Nazi Period. The unit of observation is a German municipality. A municipality is considered Non-Prussian if it was not part of Prussia during the establishment of unified Germany in 1871. Log deportations 1941-1945 is the log number of Jewish citizens deported between 1941 and 1945. Tax per Mark spent on (tax) administration is the ratio of total local taxes collected divided by total local government expenditures on administration. Violent acts against jews before 1933 is the count of violent acts against jews before 1933. Data are in 1928 prices. The column "difference" is the difference in means between Prussian and Non-Prussian regions. The column "t-stat difference" is the t-statistic for the difference in means. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

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.[17]

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. Before reporting my main results, I discuss summary statistics and provide graphical intuition for the relationship between Prussian status and deportations.

4.2 Summary statistics

Table 2 reports summary statistics for the main variables for the Nazi period, and for the Weimar as a comparison. Columns report summary statistics. I report two sets of columns, one for municipalities in Prussia, and one for municipalities outside Prussia. The sample includes all of Germany. The last columns provide a t-test for differences in means between the two groups.

The t-tests reveal some interesting significant correlations. When the Nazis prioritize deportations, deportations are higher in those parts of Germany that used to be Prussian. Before the Nazis came to power, violence against Jews is lower in Prussian regions. Both before and after the Nazis came to power, state capacity, measured by tax raised per Mark spent on administration, is higher in Prussian regions.

[17] 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).

These patterns are consistent with the hypothesis of this paper but cannot be interpreted as causal. The rest of this paper is dedicated to estimating the causal effect of being Prussian.

5 Results

In this section I present the main results of this paper. I find that deportations of German Jews are more effectively implemented in Prussian municipalities. In contrast, there is less anti-Semitic violence in Prussian municipalities before the Nazis come to power.

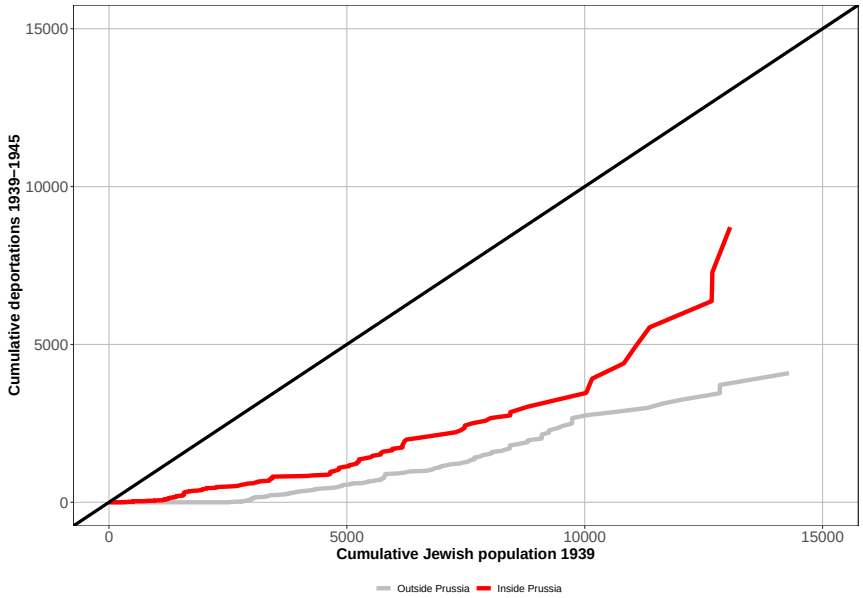
Graphical intuition. Before presenting the main regression results, I build intuition using the raw data. The summary statistics in Table 2 suggested that when considering simple means across Prussian status in all of Germany deportations were higher in Prussia. I now zoom in to the study area, a bandwidth of 50 kilometers around the former Prussian borders. For both sides of the discontinuity, I sort the municipalities by pre-War German Jewish population, and plot cumulative Jewish population on the horizontal axis, and cumulative deportations on the vertical axis. The resulting plots are in Figure 2. Note that both plots are away from the 45-degree line. The vertical difference 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 are higher for every level of 1939 Jewish population. This is in line with the simple averages in Table 2. I now study this finding by providing estimates of equation 1.

Main results. Table 3 reports estimates of equation 1 using two main dependent variables. In column (1) and (2), I use the natural log 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. In addition, I include several covariates over and above the baseline covariates.

In the background section, I noted that there were several actors that have been hypothesized to have been involved in deportations: The local government, the local Nazi party, the population and most importantly, the Gestapo. I hypothesize that the Prussian boundary captures the capacity of the local government. I use several covariates to capture the presence of the other actors.

First, 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 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 the number of Nazi

Figure 2: CUMULATIVE DEPORTATIONS 1939-1945



Notes: This figure 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 red and gray lines measure German Jews population and deportations inside and outside Prussia. Both lines are away from the 45-degree line due to emigrations between 1939 and 1941. Large scale deportations started in 1941. The curve for Prussia is everywhere above the curve for non-Prussia, suggesting that a larger share of German Jews was deported inside Prussia than outside Prussia.

party members in local politics and local government in 1939, before the start of the deportations. Second, to capture popular anti-Semitism, I control for the vote share for Nazi party in 1928, the first election in which they participated.[18] Finally, to capture the presence of the Gestapo and the intensity of the rail transports, I collected archival data on the number of Gestapo offices in a municipality as well as an indicator equal to one if a transport directly visited a municipality.[19] Finally, 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). As one could view each of these covariates as outcomes of Prussian government in the past, I present regressions that only include baseline covariates in the Appendix.

Row 1 of Table 3 contains the estimated effects of being formerly Prussian. As before, Table 3 has

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

[19] The data source for the transports is the Federal Archives. The data source for the offices is the Gestapo records held in the Arolsen archives. See the Online Appendix for full data documentation.

Table 3: THE EFFECT OF PRUSSIA ON DEPORTATIONS AND VIOLENCE

Dependent variable:	Log deportations		Dep. per bur. (×10)		Comparison
	1941-1945		1941-1945		Pre-1933
	(1)	(2)	(3)	(4)	(5)
			Full sample		
Prussian	0.44***	0.43***	0.06***	0.06***	-0.11***
	(0.08)	(0.08)	(0.02)	(0.02)	(0.03)
Mean dep. var.	1.28	1.28	0.14	0.15	0.10
Observations	593	591	557	552	620
R^2	0.59	0.61	0.27	0.33	0.17
			Non-conquest Prussia		
Prussian	0.25**	0.26**	0.07**	0.05*	-0.20***
	(0.13)	(0.13)	(0.03)	(0.03)	(0.04)
Mean dep. var.	1.17	1.16	0.14	0.13	0.10
Observations	430	428	412	410	447
R^2	0.57	0.57	0.24	0.25	0.20
Gestapo office	No	Yes	No	Yes	No
Gestapo transports	No	Yes	No	Yes	No
NSDAP in bur. 1939 (count)	Yes	Yes	Yes	Yes	No
NSDAP in gov. 1939 (count)	Yes	Yes	Yes	Yes	No
Nazi votes 1928 (%)	Yes	Yes	Yes	Yes	Yes
Total population 1925	No	No	No	No	Yes
Jewish population 1925	No	No	No	No	Yes
Jewish population 1939	Yes	Yes	Yes	Yes	Yes
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes
Bandwidth	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. Log deportations 1941-1945 is the log number of Jewish citizens deported between 1941 and 1945. Dep. per bur. (×10) 1941-1945 is the total deportations between 1941-1945 divided by the size of the bureaucracy measured in the 1933 census (×10). Comparison is the count of violent acts against Jews before 1933. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Gestapo office is an indicator for whether a Gestapo office was located in that municipality. Gestapo transports is an indicator for whether the municipality was ever visited by the Gestapo between 1941-1945. NSDAP in bur. 1939 (count) is the number of Nazi party members in the bureaucracy in 1939. NSDAP in gov. 1939 (count) is the number of Nazi party members in the government in 1939. Nazi votes 1928 (%) is the fraction of people who voted for the NSDAP in the 1928 elections. Total population 1925 is the population of the unit of observation in 1925. Jewish population 1925 is the number of Jewish citizens in 1925. Jewish population 1939 is the number of Jewish citizens in 1939. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

two panels. In the top panel, I include the full sample, whereas in the bottom panel I restrict to the non-conquest sample.

Columns (1) and (3) present the first results of this paper. 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, 0.44 (s.e. 0.08). Being formerly Prussian is, at a local level, with a 44% 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 44% difference corresponds to a predicted difference in levels of 18 deportees (34 outside Prussia, 52 inside). 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 43%.[20] 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 when the Nazis came to power in 1933. I find that deportations per bureaucrat (x10) are 0.06 higher, relative to a mean of 0.14. The result, too, is significant and essentially the same with controlling for the presence of the Gestapo. 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. Until the Nuremberg laws of 1935, German Jews had citizenship status and were protected. There was anti-Semitic violence perpetrated by the local population, which the local governments were tasked with preventing. In column (5), I use the number of anti-Semitic violent events in 1932 and early 1933 as the dependent variable. Conditional on votes for the Nazi party as a measure of the anti-Semitism of the local population, I find a negative significant effect of being Prussian on violence against Jews. Consider the point estimate in column (1), row 1, -0.11 (s.e. 0.03). This effect is about equal to the size of the mean of the variable, indicating that in Prussia the number of incidents is reduced, on average, to zero.

In Panel II I study the part of the former Prussian boundary established unintentionally. This strategy has two advantages. First, even when covariates vary smoothly over the boundary, the location of the boundary itself may be endogenous. Second, the literature on support for the Nazi party has focused on the deep determinants of anti-Semitism. There may be a correlation between such factors and the expansion of Prussia. If there is, I expect this to be less of an issue in Panel II. 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 lower in the Weimar period.[21] As a corollary, in the Appendix I re-estimate Panel I subsetting to municipalities that never experienced medieval anti-Semitism. Results are virtually identical.

[20] 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'.

[21] The point estimates are smaller in columns (1) and (2), and larger in columns (3)-(5). In levels, the point estimates in columns (1) and (2) imply 13 additional deported German Jews.

Comparing the results in columns (1) and (3) to column (5) gives the main result of this paper. In Prussian places, when the government policy shifts to persecution, are more effective at deportations. Violence against Jews is lower when it is official government policy to protect their safety. The hypothesis of this paper explains these patterns through higher Prussian state capacity. More effective bureaucracies empower politicians to implement policy, whether this is a pro-development policy or not. Before discussing challenges, I now discuss the extent to which the Nazi government achieved its objectives.

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 A8, 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 A17 I instead code an indicator equal to one of all Jews were deported, and an indicator equal to one if more than 90% of pre-War 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. It may be that Prussian areas are simply more rule-following. When the rules protect German Jews, I therefore observe less anti-Semitic violence. Similarly, I 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 a placebo exercise to test for this possibility in the next section. In the Appendix, I implement two more exercises to study this phenomenon. 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). 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 result. Together with the placebo check below these result suggest that rule compliance is not driving my results. Below I directly for state capacity as the primary mechanism.

Local policy differences. A second 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 Prus-

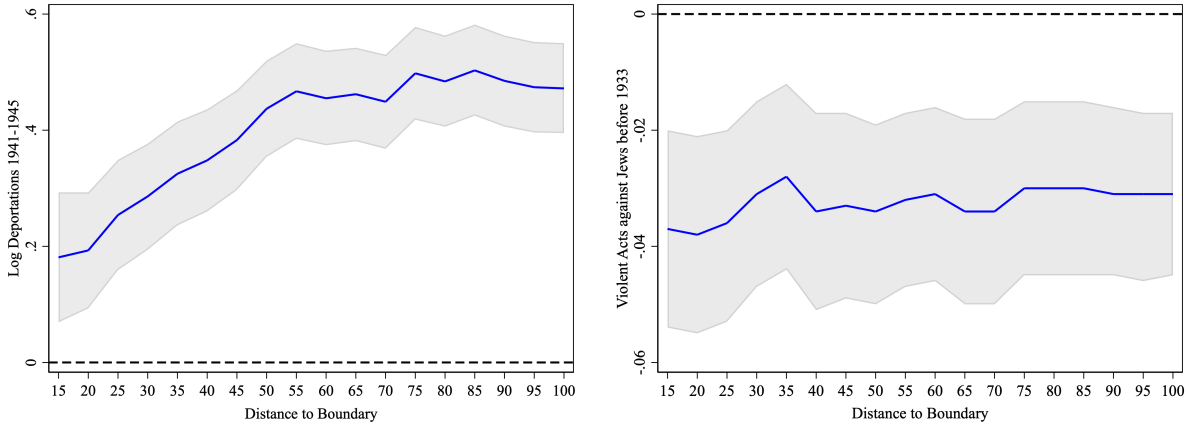
sia may have implemented policies that varied at the discontinuity. I may therefore, instead of identifying the effect of differences in former Prussian status, pick up differences in policies that were implemented by the Prussian provincial government. In 1932, however, Prussia as a local administrative 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. In the Appendix I rerun all analyses with post-1932 data only. The results are virtually identical. This result shows that policy differences are not driving my main results.

Migration into Prussia before 1933. A final challenge comes from (differential) Jewish migration. Column (5) shows that anti-Semitic violence was lower in Prussian municipalities before 1933. It may therefore be the case that Jews moved away from non-Prussian municipalities, where violence was relatively higher, 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 Jewish population in 1939, after such migrations would have happened. Second, rather than controlling, I normalize deportations by the Jewish population in 1939 in the Appendix, with similar results. I then repeat my analyses for deportations directly controlling for the number of violent events before 1933. Doing this does not affect the results.[22]

Further robustness. In Figure 3 I verify that the results in this section hold when restricting to municipalities closer to the boundary. Starting with municipalities within 15 kilometers from the boundary - a bandwidth of one municipality on either side - I increase the bandwidth in steps of five kilometers to 100 kilometers. For deportations, the effect stabilizes around 50 kilometers from the boundary. It is significant for all bandwidths. For violence, the estimated effects are stable for any bandwidth and the effect of being Prussian is negative and significant throughout. In the Appendix I show robustness of my main results to 1) varying the $f(location)$ function in Equation 1 and using the estimators proposed by Calonico et al. (2020), 2) excluding covariates, and 3) using deportations over the full period of Nazi rule, 1933-1945. I then control for emigrations between 1939 and 1941, religious differences, 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. It may be important for the estimated effect that the territories of *Ansbach* and *Bayreuth* became part of Prussia in 1791, but left Prussia and became part of Bavaria in 1815. I verify that dropping them from the sample does not affect the results.

[22] 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. I test for this in two ways. First, I include an indicator for receiving a transport as a covariate. Second, I code an indicator equal to one if a municipality deported more German Jews than the number of German Jews recorded in the 1939 census. Using this as the dependent variable I find no correlation with having been Prussian.

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 15 and 100 kilometers in 5 km increments (x-axis). Gray outlines show 95% confidence intervals. I have connected both point estimates and confidence intervals horizontally for visual convenience. The left sub-figure uses the natural log of deportations between 1941-1945 as the dependent variable. The regression specification corresponds to column (1) of Table 3. The left sub-figure uses the number of violent incidents against Jews before 1933 as the dependent variable. The regression specification corresponds to column (5) of Table 3.

In this section I related former Prussian status to deportations, both in levels and *per bureaucrat*. The hypothesis of this paper rationalizes these findings through a better functioning bureaucracy. In the remainder of this paper, I study mechanisms to substantiate this interpretation.

6 Mechanisms: State capacity

There may be many mechanisms connecting former Prussian status to deportations and violence. The hypothesis of this paper focuses on the capacity of the state and predicts that Prussian places will be better able to implement policy, whether this is protection, persecution, or otherwise. Before studying competing mechanisms, this section provides direct evidence in favor of the state capacity channel. I measure tax collection and the implementation of public policy as conventional measures of state capacity. I find a strong, positive, and significant effect of being Prussian on local state capacity. Importantly, I find positive effect both during the Weimar and the Nazi period. Formerly Prussian municipalities were more effective before and after the Nazis came to power.

In addition, for tax collection, I study the immediate postwar period as a placebo exercise. I focus on the period before 1949, when Germany was occupied and administered by the Allies. The Allies centralized public finance and the municipalities were no longer responsible for raising taxes, but I still observe tax raised within a municipality. This fact gives me a way to study rule-following as an alternative

interpretation of my results in the previous section as well as this section. If the effect of being Prussian works through the internal organization of the bureaucracy, I would not expect to see an effect of being Prussian on tax raised in the immediate postwar period. If, instead, the effect of being Prussian works through a different mechanism that is unrelated to the tasks and efforts of the local bureaucracy - such as rule-following - I would expect to see a lingering effect of being Prussian when tax collection is taken away. Naturally, the war may have affected Prussian municipalities differently, and I control for the impact of the war throughout these exercises.

6.1 Results

In Table 4 I study conventional measures of the capacity of the state. The most used measure of the capacity of the local state to implement policies is 'fiscal capacity', the ability of a government to raise taxes (Besley and Persson, 2011b). Groups of columns study different periods. In columns (1)-(3) I study the Weimar period. In column (1) I use total tax collected per capita as the dependent variable. This variable is available for a large number of municipalities, so I restrict the sample to be within 50 kilometers from the study boundary. In column (2) I use total local tax raised normalized by total local expenditure on public administration, which includes tax administration. Data on expenditure on public administration is available for a smaller number of municipalities, and I include all municipalities with data in the sample here. In column (3), I use the total amount of trash collected in cubic meters per administrator responsible for trash collection as a measure of the effectiveness of mundane public policy implementation. I use the same outcome variables for the Nazi period in columns (4)-(6). In columns (7) and (8) I use tax collected per capita and tax efficiency for the post-war period. The relevant currency units are the *Reichsmark* before the war and the *Deutschmark* after the war. I express all relevant quantities in 1928 prices.

Table 4 presents results. For the Weimar period, I find that Prussian municipalities raise more taxes per capita. They raise more taxes per mark spent on public administration and collect trash more efficiently. Importantly, I find very similar effects for the Nazi period. Former Prussian areas raise taxes more efficiently and collect trash more efficiently after the Nazis came to power. For both periods, the estimated effect sizes are economically meaningful. Take the estimated effect in column (1), 18.1 (s.e. 4.5). This effect is equal to about forty percent of the mean tax collected per capita. The results in this table are consistent with my hypothesis: Prussian status proxies for higher local state capacity. This capacity is present under different governments or regimes but can be directed towards different ends. After the war, the Allies centralized public finance, and I use this period as a placebo. To account for the differential impact of the war across municipalities, I control for the share of the pre-war housing stock that was destroyed, and the

Table 4: THE EFFECT OF PRUSSIA ON LOCAL STATE CAPACITY

Dependent variable:	Tax raised per capita and per Reichsmark spent on administration, and trash collected (m^3) per Reichsmark spent on collection							
Period:	Weimar			Nazi			Postwar	
	Capita (1)	Adm. (2)	Trash (3)	Capita (4)	Adm. (5)	Trash (6)	Capita (7)	Adm. (8)
				Full sample				
Prussian	18.1***	1.1***	0.1**	13.0***	1.2***	0.1*	5.9	-0.5
	(4.5)	(0.2)	(0.0)	(2.7)	(0.4)	(0.0)	(5.1)	(3.0)
Mean dep. var.	50.2	2.1	0.2	59.1	5.6	0.2	79.6	28.5
Observations	112	80	72	238	94	71	90	88
R^2	0.25	0.18	0.15	0.12	0.15	0.11	0.52	0.26
				Non-conquest Prussia				
Prussian	16.1***	1.5**	0.1**	13.0***	1.3***	0.1	-3.7	3.4
	(5.7)	(0.5)	(0.0)	(3.9)	(0.4)	(0.1)	(9.9)	(3.9)
Mean dep. var.	47.6	2.0	0.2	57.1	5.6	0.2	95.5	28.0
Observations	81	58	56	173	71	53	71	85
R^2	0.28	0.16	0.36	0.11	0.13	0.18	0.35	0.24
Total subsidy 1950	No	No	No	No	No	No	Yes	Yes
War destruction 1939-1946	No	No	No	No	No	No	Yes	Yes
Change in the male population 1939-1946	No	No	No	No	No	No	Yes	Yes
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	50KM	All	All	50KM	All	All	50KM	All

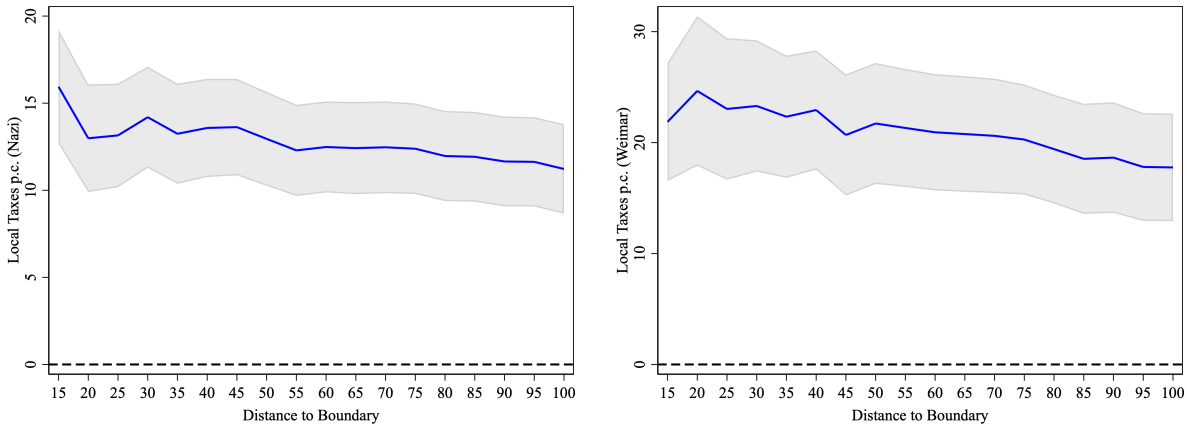
Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. Data are in 1928 prices. Tax data for the Postwar period were collected for the year 1953 in Deutsch Mark (DM), expressed in terms of Reichsmark (RM) in 1928. Expenditure data for the Postwar period were collected for the year 1950 in Deutsch Mark (DM), expressed in terms of Reichsmark (RM) in 1928. Capita is total local taxes divided by total population. Adm. is total local taxes collected divided by local gov't expenditures. Trash is total trash collected divided by total expenditure on trash collection. Unemployment 1933 p.c. is the number of unemployed persons in 1933 divided by total population in 1933. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Total subsidy 1950 is the sum of subsidies received from the central government for construction, infrastructure, housing, housing administration and general public needs such as education, welfare, and arts. War destruction 1939-1946 is the change in housing units between 1946 and 1939 normalized by 1939 housing units. Change in male pop. 1939-1946 is the change in composition of males in the population between 1939 and 1946. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

percentage change in male population at a local level. I find precise zero effects of being Prussian for this period. All results are similar, albeit slightly noisier, in the non-conquest sample.

Robustness. Figure 4 shows that my results for tax collected per capita are robust to bandwidth choice. The effect of being Prussian is essentially stable and significant for any bandwidth between 15 and 100 kilometers from the study boundary. In the Appendix I report several additional analyses. I first report results for difference 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 than for the level of taxes collected. 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 4 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. For the postwar period, I find zero effects throughout.[23]

Figure 4: ROBUSTNESS TO BANDWIDTH CHOICE: STATE CAPACITY



Notes: Both sub-figures plot the point estimates of β from equation (1) on the y-axis for different bandwidth values between 15 and 100 kilometers in 5 km increments (x-axis). Gray outlines show 95% confidence intervals. I have connected both point estimates and confidence intervals horizontally for visual convenience. The left sub-figure uses total tax raised per capita during the Nazi period as the dependent variable. The regression specification corresponds to column (1) of Table 4. The left sub-figure uses total tax raised per capita during the Weimar period as the dependent variable. The regression specification corresponds to column (4) of Table 4.

This section has shown that one mechanism through which being formerly Prussian exerts an effect during the Weimar and Nazi period is through the effectiveness of the local bureaucracy. Such a mechanism is plausible given the main results for violence: As policy changes from protection to persecution, more effective local governments are better at implementing either policy. There may, of course, be other mechanisms. The next section therefore studies the most plausible alternative mechanisms.

[23] In the Appendix, 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 repeat the analyses in the previous section using expenditure per capita. Prussian places expend more per capita, in both the Weimar and Nazi periods. In the postwar period, the local budget was centrally funded, and I find no differences between Prussian and non-Prussian areas. 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 large and significant increase in local expenditure on police. In the postwar period, I find no significant differences.

7 Alternative Mechanisms

The literature - and especially the literature studying Nazi party membership and the persecution of Jews - suggests several alternative mechanisms. For example, the Nazis came to power in the 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 5 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 (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). 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 slightly imbalanced balanced across the study boundary (columns (7) and (8)). I show robustness to including measures of religion in the main specification in the Appendix, and I find that it does not affect the main result. These results by and large remain unchanged in the non-conquest sample.

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

Table 5: 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. (x1000) (5)	In 1933 p.c. (x1000) (6)	Catholic share 1925 (7)	Protestant share 1925 (8)
				Full sample				
Prussian	-0.03	-0.01	-0.08	0.02	-0.00	-0.00	0.04*	-0.04*
	(0.05)	(0.00)	(0.11)	(0.05)	(0.01)	(0.01)	(0.02)	(0.02)
Mean dep. var.	-0.45	0.07	0.58	0.27	0.22	0.27	0.29	0.69
Observations	558	563	137	137	542	544	620	620
R^2	0.09	0.22	0.16	0.10	0.14	0.14	0.38	0.38
				Non-conquest Prussia				
Prussian	-0.03	-0.01	-0.04	0.06	0.00	-0.01	0.15***	-0.14***
	(0.02)	(0.00)	(0.14)	(0.07)	(0.01)	(0.01)	(0.04)	(0.04)
Mean dep. var.	-0.47	0.07	0.64	0.28	0.22	0.27	0.33	0.65
Observations	418	419	100	100	382	384	447	447
R^2	0.10	0.26	0.17	0.15	0.13	0.14	0.40	0.39
Signal strength	No	No	No	No	Yes	Yes	No	No
Population 1933	Yes	No	No	No	No	No	No	No
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. Employment growth is the change in the employment between 1925 and 1933. Unemployment 1933 p.c. is the number of unemployed persons in 1933 divided by total population in 1933. Civic clubs p.c. ($\times$1000) is the ratio of non-militaristic clubs per 1000 inhabitants in 1925. Military clubs p.c. ($\times$1000) is the ratio of military clubs per 1000 inhabitants in 1925. Radio listeners per capita (x10k) in 1932 is radio subscriptions per 10000 people in 1932. Radio listeners per capita (x10k) in 1933 is radio subscriptions per 10000 people in 1933. Share of Catholics 1925 is the number of Catholics divided by total population in 1925. Share of Protestants 1925 is the number of Protestants divided by total population in 1925. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Signal strength is radio signal strength for that respective year Adena et al. (2015). Total population 1925 is the population of the unit of observation in 1925. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

results raise two questions. First, the fact that I find balance locally around the former Prussian borders does not mean that, for example, social capital did not matter for the implementation of the Holocaust. In the next section, I move away from the study boundary and investigate the factors I considered in this section complement state capacity across all of Germany. Second, Prussia ceased to exist as a country in 1871 yet I find effects fifty to seventy years later. In the last section of this paper, I study what transmits differences in state capacity over time, and I find that the internal organization of the local bureaucracies is a source of persistence.

7.1 Complements and substitutes to effective local government

In this section I 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 from the previous 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 posi-

tive 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. Table 6 reports results by in. Each set of columns reports results on a category of mechanisms and individual columns contain regressions that include interactions. Table 6 shows several interesting patterns. For example, social capital acts as a substitute for the capacity of the state, as does Protestantism. Radio ownership and the Great Depression do not interact with having been Prussian.[24]

Table 6: COMPLEMENTS AND SUBSTITUTES OF PRUSSIAN STATE CAPACITY

Dependent variable:	Log deportations 1933-1945 (%)							
	Great Depression:		Social Clubs:		Radio Listeners:		Religion:	
Complement/substitute:	Employment growth 1925-1933	Unemployment 1933 p.c.	Civic clubs p.c. (x1000)	Military clubs p.c. (x1000)	In 1932 p.c. (x1000)	In 1933 p.c. (x1000)	Catholic share 1925	Protestant share 1925
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prussian	0.64**	0.48***	0.49**	0.55**	0.22**	0.20**	0.16*	0.57***
	(0.32)	(0.17)	(0.23)	(0.21)	(0.10)	(0.10)	(0.09)	(0.15)
Interaction	0.51	-1.46	-0.20	-1.54**	0.01	0.10	0.78***	-0.57***
	(0.65)	(2.23)	(0.33)	(0.61)	(0.13)	(0.13)	(0.20)	(0.17)
Mean dep. var.	1.66	1.62	2.85	2.80	1.57	1.58	1.58	1.58
Observations	856	841	190	193	919	924	909	892
R^2	0.60	0.63	0.72	0.72	0.65	0.67	0.62	0.65
Main effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jewish population 1933	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NSDAP in bur. 1933 (count)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NSDAP in gov. 1933 (count)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nazi votes 1928 (%)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	All	All	All	All	All	All	All	All

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. Employment growth is the change in the employment between 1925 and 1933. Unemployment 1933 p.c. is the number of unemployed persons in 1933 divided by total population in 1933. Civic clubs p.c. ($\times$1000) is the ratio of non-militaristic clubs per 1000 inhabitants in 1925. Military clubs p.c. ($\times$1000) is the ratio of military clubs per 1000 inhabitants in 1925. Radio listeners per capita (x10k) in 1932 is radio subscriptions per 10000 people in 1932. Radio listeners per capita (x10k) in 1933 is radio subscriptions per 10000 people in 1933. Share of Catholics 1925 is the number of Catholics divided by total population in 1925. Share of Protestants 1925 is the number of Protestants divided by total population in 1925. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Signal strength is radio signal strength for that respective year Adena et al. (2015). Total population 1925 is the population of the unit of observation in 1925. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

I now turn to explaining what it is about the Prussian bureaucracy that made it more efficient.

8 The sources of Prussian state capacity

Prussia ceased to exist as a country in 1871, but I find a treatment effect of having been Prussian on state capacity and deportations fifty to seventy years later. What is it about the Prussian bureaucracy that made it more persistently efficient? In this section, I study aspects of the local German bureaucracy to understand what drives the more efficient local bureaucracy in formerly Prussian places. I find that Prus-

[24] In the Appendix, I repeat this exercise for the non-conquest sample, with similar results.

sian bureaucracies are more specialized, leading to efficiency improvements, including in information processing, one aspect of the local government I highlighted in section 2. I then study the composition and ideology of local government. The most important candidate aspect of ideology is the penetration of the Nazi party into local politics and the bureaucracy. I also study the anti-Semitism of the population. For example, Mallmann and Paul (1994) suggest that a more anti-Semitic population helped the local bureaucrats, and Prendergast (2007) points out that bureaucrats respond to both their superiors as well as the population since both act as their principals. Naturally, more anti-Semitic bureaucrats may simply exert more *effort*. A variant of this argument concerns the legal mindedness of Prussian bureaucrats. Culturally, Prussian bureaucrats were often characterized as the prototypical Weberian impartial implementors of the law (Mommsen, 1966). I test for these competing mechanisms and find that they cannot explain the persistent effect of Prussia. I close this paper by discussing a literature in economics and sociology which studies organizational structure is an important driver of the implementation of (potentially) repugnant directives. This literature connects my findings on specialization to the question why bureaucrats that may not have been fanatical Nazis would implement policies that clearly contributed to the deportations of the German Jews.

8.1 Bureaucratic specialization

Considering that bureaucrats employed before 1871 will not have been in their jobs anymore by 1933, a natural starting point for studying organizational performance is its structure. Historians of the Prussian bureaucracy argue that specialization is one of the dimensions that set the Prussian bureaucracy apart. 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).[25] For further evidence on specialization within the Prussian administration, see Dorn (1931, 1932a,b), Kiser and Schneider (1994) and Clark (2006). Of course, a drive towards specialization may simply come from

[25]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).

a natural desire to improve the efficiency of government (see e.g. Rosen (1983)).[26]

Many of my outcomes so far, such as deportations per bureaucrat, the reaching of the policy goal of deporting all Jews, or efficient tax collection are consistent with increased specialization. In this section, I directly measure specialization of bureaucrats *within* the bureaucracies of individual municipalities. I measure specialization in several different ways. 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. Naturally, absence of a certain type of, say, managers may simply be a size effect. I therefore first use the number of civil servants per capita as an outcome, and subsequently I control for the number of civil servants throughout. I then construct two measures of specialization. First, I construct a Herfindahl index measuring the concentration of bureaucrats across occupied job categories. I compute this index separately for: All public sector employees, white-collar workers, and for blue-collar workers.[27] I list all job titles in the Online Appendix. Examples of blue-collar workers are trash collectors and park maintenance personnel. Examples of white-collar workers are those bureaucrats managing the police or tax collection.[28] Second, I count the number of categories that have at least one employee. The idea here is that many cities do not have employees in all categories and that employment in more categories is indicative of specialization in the bureaucracy. I compute these measures for the Weimar period only because the Nazi period records are far less detailed on the composition of the municipal workforce. A further limitation of this data is that it is available for a smaller number of cities.[29]

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 also expect Prussian municipalities to have more distinct job categories, conditional on the total number of employees. Since white-collar workers were responsible for tasks that are more intuitively related to dif-

[26]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).

[27]The German equivalents are *Beamte* and *Angestellte* for white-collar workers, and *Arbeiter* for blue-collar workers.

[28]To be precise, I compute:

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

H_i is the 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 Online Appendix lists all job categories.

[29]Below, I report results within the bandwidth of 50KM around the discontinuity. In the Appendix, I report results using all municipalities, outside the bandwidth as well as inside, which results in a larger sample at the cost of including municipalities further away from the boundary. Results are similar.

ferences in bureaucratic effectiveness, I expect the concentration results to be stronger for white-collar workers. Table 7 presents results. I start by measuring the total number of bureaucrats per capita to check if bureaucracies are systematically larger in Prussia. I find that Prussian bureaucracies are larger, but the result is only marginally significant, and is insignificant in the non-conquest sample. Columns (2), (3) and (4) use the Herfindahl indices for all employees, white-collar workers and blue-collar workers as outcome variables. Focusing on column (2), the Herfindahl index is 3.4 percentage points lower, relative to a mean of 0.11. This effect is driven by lower concentration in white-collar jobs.[30] When concentration is lower, bureaucrats are more spread out across job categories. Therefore, by this measure, former Prussian municipalities are more internally specialized. Columns (5) through (7) use the number of employment categories as outcomes. Column (5) shows that, on average, Prussian municipalities have one job category more occupied, again conditioning on overall employment (relative to a mean of eight categories occupied). Columns (6) and (7) show that this effect is driven by managers. Results for concentration and the number of jobs hold in the non-conquest sample. In the Appendix I show that, as a corrolary, Prussian bureaucrats are compensated for their higher productivity with higher wages. Taken together, these results are consistent with the idea that Prussian municipalities are more functionally, or horizontally, specialized.

Specialization and information processing. 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. To measure the quality of information processing I use the fact that when a municipality did not deliver the most up to date information to the organization that published the Statistical Yearbooks this was noted, for a small subset of variables. I code an indicator equal to one if a municipality delivered out of date information on its surface area and land use as a proxy for their information processing capacity.[31] In columns (8) and (9), I use this indicator as the outcome variable. I find that late reporting is, on average, reduced to zero in formerly Prussian municipalities, relative to mean of 3%. This result is consistent with Prussian municipalities being better able to process information. While not conclusive, this evidence is supportive of the hypothesis of this

[30] The point estimate for workers is similar, but noisy. Therefore, a Chow test cannot reject the hypothesis that the two coefficients for managers and workers are equal.

[31] Every year, municipalities delivered data on surface area and land use. For the data reported in 1939, there is variation in whether municipalities delivered up to date data for March 31st, 1938, or out of date data for December 31st, 1937.

Table 7: THE STRUCTURE AND EFFECTIVENESS OF THE BUREAUCRACY

Dependent variable:	Size of bureaucracy	Professional specialization (Herfindahl index):			Number of occupation categories:			Late reporting	
	per capita	All	White	Blue	All	White	Blue		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Full sample					
Prussian	0.002*	-0.034***	-0.018***	-0.018	0.904***	0.612***	0.173	-0.041**	-0.167**
	(0.001)	(0.007)	(0.005)	(0.027)	(0.243)	(0.208)	(0.166)	(0.018)	(0.078)
Chow test of coeffecient equality (p-value)			0.37	0.37		0.12	0.12		
Mean dep. var.	0.010	0.111	0.184	0.242	7.915	4.124	2.750	0.025	0.128
Observations	94	70	73	96	94	97	96	630	94
R^2	0.04	0.44	0.24	0.17	0.27	0.20	0.09	0.05	0.12
				Non-conquest Prussia					
Prussian	0.006	-0.048***	-0.028***	-0.025	1.273***	0.775**	0.301	-0.063**	-0.202*
	(0.004)	(0.009)	(0.008)	(0.036)	(0.346)	(0.347)	(0.266)	(0.030)	(0.116)
Chow test of coeffecient equality (p-value)			0.93	0.93		0.3	0.3		
Mean dep. var.	0.010	0.118	0.187	0.247	7.781	4.045	2.708	0.029	0.141
Observations	64	46	48	65	64	66	65	454	64
R^2	0.09	0.49	0.30	0.22	0.30	0.23	0.16	0.08	0.21
Overall employment 1928	No	Yes	No	No	Yes	No	No	No	Yes
Total management 1928	No	No	Yes	No	No	Yes	No	No	No
Total workers 1928	No	No	No	Yes	No	No	Yes	No	No
Jewish population 1939	No	No	No	No	No	No	No	Yes	Yes
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	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. Size of the bureaucracy per capita is the sum of arbeiter, angestellte and beamte that assist in local government in 1928 divided population in 1925. Professional specialization (Herfindahl Index) is a measure of employment concentration constructed using the number of employees across employment categories (all employees, white collar, and blue collar) within the local government. Number of occupation categories is the number of occupation categories that employ at least one bureaucrat. Late Reporting is an indicator for whether the municipality was late in reporting administrative statistics in 1939. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Overall employment 1928 is the sum of total white and blue collar workers. Total management 1928 is the total number of people in management positions. Total workers 1928 is the total number of people not in management positions. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

paper.

8.2 Alternative sources of state capacity

In this section I explore several other sources of state capacity. The first most important alternative is simply effort. It may be that bureaucrats in formerly Prussian municipalities were ideologically motivated by anti-Semitism or Nazism. It may also be that their principals were more motivated, either as Nazis or as *beamte*. Tenured civil servants, or *beamte*, are thought to have been more ideologically committed to functional government (Mommsen, 1966). Such civil servants are in part responsible for the stereotype of the impartial and motivated Prussian bureaucrat. In their various forms, these alternative potential

sources of capacity also speak to the principal-agent structure of the local governments. Both principals, the white-collar bureaucrats or the population may each have had a different composition in Prussian places or may simply have been more motivated. In addtion, it may be that former Prussian places are more hierarchical, leading to efficiency differences (Bandiera et al., 2021). In this section I test these ideas. I find that these factors do not explain the effect of Prussia, but that Nazi ideology is a complement of state capacity in Germany as a whole.

Table 8 reports results. In column (1) I study hierarchy by measuring the ratio of white-collar 'managers' to blue-collar 'workers'. In column (2) I instead focus on the idea that *beamten* were different by measuring the ratio of beamten to total white collar workers. In columns (3)-(5) I study Nazi ideology. In column (3) I use the *change* in number of Nazi Party members in the local government, to capture potential change in both the fanaticism of the local politicians, and the ideology of one of the principals of the local bureaucracy. In column (4) I use the same measure, but for the local bureaucracy rather than local politics, to measure the ideology of the bureaucrats themselves.[32] Finally, in column (5) I use the number of letters (per capita) expressing anti-Semitic sentiments that were sent to *Der Stürmer*, a radical anti-Semitic propaganda newspaper, using data from Voigtländer and Voth (2012). I use this to measure popular anti-Semitic sentiment that may either lead to greater cooperation by the local government or more effort by the local bureaucracy to respond to local preferences.

Row 1 contains results. I find no evidence that the Nazi party penetrated differentially in Prussia, or that the local population was more ideological *around the study boundary*. Hierarchy is not different, and I find a small negative coefficient for the ratio of beamte to white-collar workers. This suggests that an ideological commitment to civil government does not explain Prussian state capacity. In the non-conquest sample, this result is insignificant. All other results are similar in the non-conquest sample. As before, the fact that these variables do not vary systematically at the study boundary does not mean they are not important for the implementation of the Holocaust more broadly. In Table 9 implement a complements and substitutes exercise like above for the variables used in Table 8. I find that Nazi ideology and hierarchy, in a sample comprising all of Germany, are complements to state capacity.

Taken together, the results in this section suggest that municipal bureaucracies in former Prussian areas are more specialized and process information more efficiently. This aspect of the organization of the Prussian bureaucracy is a source of persistence for the treatment effect of having been Prussian. A natural question that remains is how bureaucrats that may not have been ardent Nazis dealt with directives that they knew led to repression. In the next section, I discuss how specialization may make it easier for

[32]Note that I use these variables *in levels* as covariates in my main analyses above. Here I use them to understand whether the Nazi governed stacked bureaucracies differently in Prussia, or whether Nazi party members entered government or politics differentially.

Table 8: ALTERNATIVE SOURCES OF A MORE EFFECTIVE BUREAUCRACY

	Hierarchy and Dutifulness		Nazi principals: Government and popular		
Dependent variable:	White collar/ Blue collar 1933 (1)	Beamte/ White collar 1933 (2)	Δ NSDAP in gov. 1933-1939 (3)	Δ NSDAP in bur. 1933-1939 (4)	Stürmer letters p.c. (× 10k) (5)
			Full Sample		
Prussian	-0.01	-0.02**	-0.03	-0.18	-0.50
	(0.03)	(0.01)	(0.04)	(0.18)	(0.44)
Mean dep. var.	0.38	0.37	0.14	1.23	1.25
Observations	563	563	630	630	630
R^2	0.08	0.11	0.42	0.84	0.16
			Non-conquest Prussia		
Prussian	-0.01	-0.02	0.05	0.10	0.36
	(0.04)	(0.01)	(0.06)	(0.19)	(0.52)
Mean dep. var.	0.37	0.37	0.17	1.41	1.40
Observations	419	419	454	454	454
R^2	0.08	0.12	0.45	0.84	0.18
Size of bureaucracy 1933	Yes	Yes	No	No	No
Jewish population 1933	No	No	Yes	Yes	Yes
NSDAP in bur. 1933 (count)	No	No	No	Yes	No
NSDAP in gov. 1933 (count)	No	No	Yes	No	No
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes
Bandwidth	50KM	50KM	50KM	50KM	50KM

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. Non-conquest Prussia is the sample that consists of all non-Prussian municipalities and the parts of Prussia that became Prussian for reasons that were unrelated to the territorial ambitions of the Prussian rulers. The Appendix discusses each expansion. White/blue collar 1933 is the ratio of white collar workers to blue collar workers in 1933. Beamte/White collar 1933 is the ratio of beamte to white collar workers in 1933. Δ NSDAP in gov. 1933-1939 is the difference between the stock of Nazi party members in government in 1933 and 1939. Δ NSDAP in bur. 1933-1939 is the difference between the stock of Nazi party members in the bureaucracy in 1933 and 1939. Stürmer letters per capita (x10k) is the number of letters sent to the anti-Semitic propaganda newspaper "Stürmer" per 10,000 inhabitants during the Nazi period. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Size of bureaucracy 1933 is the sum of *arbeiter*, *angestellte* and *beamte* from the 1933 census. Jewish population 1933 is the number of Jewish citizens in 1933. NSDAP in bur. 1933 (count) is the number of Nazi party members in the bureaucracy in 1933. NSDAP in gov. 1933 (count) is the number of Nazi party members in the government in 1933. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

individual bureaucrats to justify implementing potentially morally repugnant directives.

9 Repugnant policies and the 'cog in the wheel' argument

It is clear from the historical literature on the involvement of the municipal governments in the deportations that individual bureaucrats knew that the German Jews would be expropriated and sent to Poland.

Table 9: COMPLEMENTS AND SUBSTITUTES OF PRUSSIAN ORGANIZATION

Dependent variable:	Log deportations 1941-1945 (%)				
Complement/Substitute:	White / Blue collar 1933 (1)	Beamte/ White collar 1933 (2)	Δ NSDAP in gov. 1933-1939 (3)	Δ NSDAP in bur. 1933-1939 (4)	Stürmer letters p.c. (× 10k) (5)
Prussian	0.24**	0.42*	0.43***	0.43***	0.39***
	(0.12)	(0.25)	(0.07)	(0.07)	(0.07)
Interaction	0.48*	-0.01	-0.00	0.06**	0.04**
	(0.27)	(0.67)	(0.18)	(0.03)	(0.02)
Mean dep. var.	1.40	1.34	1.36	1.34	1.35
Observations	843	836	927	925	929
R^2	0.64	0.63	0.65	0.64	0.63
Main effect	Yes	Yes	Yes	Yes	Yes
Jewish population 1939	Yes	Yes	Yes	Yes	Yes
NSDAP in bur. 1933 (count)	Yes	Yes	Yes	Yes	Yes
NSDAP in gov. 1933 (count)	Yes	Yes	Yes	Yes	Yes
Nazi votes 1928 (%)	Yes	Yes	Yes	Yes	Yes
Latitude, Longitude	Yes	Yes	Yes	Yes	Yes
Distance to Berlin	Yes	Yes	Yes	Yes	Yes
Segment fixed effects	Yes	Yes	Yes	Yes	Yes
Bandwidth	All	All	All	All	All

Notes: All regressions are estimated using Ordinary Least Squares. The unit of observation is a German municipality. White/blue collar 1933 is the ratio of white collar workers to blue collar workers in 1933. Beamte/White collar 1933 is the ratio of beamte to white collar workers in 1933. Δ NSDAP in gov. 1933-1939 is the difference between the stock of Nazi party members in government in 1933 and 1939. Δ NSDAP in bur. 1933-1939 is the difference between the stock of Nazi party members in the bureaucracy in 1933 and 1939. Stürmer letters per capita (x10k) is the number of letters sent to the anti-Semitic propaganda newspaper "Stürmer" per 10,000 inhabitants during the Nazi period. Prussian is equal to 1 for each municipality that was part of Prussia during the establishment of unified Germany in 1871 and zero otherwise. Main effect is the complement/substitute of the respective column. Jewish population 1939 is the number of Jewish citizens in 1939. NSDAP in bur. 1933 (count) is the number of Nazi party members in the bureaucracy in 1933. NSDAP in gov. 1933 (count) is the number of Nazi party members in the government in 1933. Nazi votes 1928 (%) is the fraction of people who voted for the NSDAP in the 1928 elections. All regressions include latitude and longitude at the centroid of a unit. Distance to Berlin is the straight line distance between the centroid of the municipality and Berlin, in kilometers. All regressions include fixed effects for the closest Prussian boundary segment. Bandwidth indicates whether the regression is conditional on the optimal bandwidth of 50 kilometers (KM) or unconditional (All). Heteroskedasticity robust standard error are in parentheses. * Indicates significance at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level.

In this section I discuss the connection between the specialization, and the incentives less ideologically motivated bureaucrats have to implement directives they know contribute to enormous human suffering. In fact, Prendergast (2007) observes that bureaucrats may have an incentive to prioritize the interests of citizens over the interests of their principal.[33] They may also refuse to implement repugnant orders.

A massive literature has asked the question why thousands of Germans were nevertheless involved in the implementation of the Holocaust. Besides Nazi fervor, the main explanation in economics is focused on competition among government ministries in Berlin (Breton and Wintrobe, 1986), but by far the most influential explanation is associated with Hannah Arendt's 'banality of evil' argument (Arendt, 2006). In the remainder of this paper, I connect her notion of the ordinary bureaucrat who - as a 'cog in the wheel' - implements directives with repugnant ultimate consequences to specialization in a bureaucracy as a potential explanation for why the implementation of the deportation of Jews was so much more effective

[33] A whistleblower also naturally comes to mind.

in formerly Prussian, and more specialized, municipalities of Nazi Germany.

After the War, functional specialization was often brought up as a defense by former Nazis against the accusation of repugnant war crimes. For example, Adolf Eichmann, the highest-ranking official in charge of the Holocaust to survive the war, used the defense that he was merely a bureaucrat and had no part in the decisions to execute the Holocaust (Arendt, 2006). Eichmann was, among other things, involved with the organization of the Wannsee conference of 1942, where the Nazis decided on the 'Final Solution to the Jewish question', and he was also responsible for coordinating the transport of Jews to the concentration camps. When asked about the organization of the Wannsee conference, Eichmann defended himself as follows:

> "What it says here - I am not trying to deny that, but I must protest, Mr. Attorney General, at the implication that I was the competent official-in-charge for the entire Final Solution; that is not true. I was simply assigned the task of writing the invitations, in other words, to carry out the administrative work of inviting these people to the conference in March of the same year, 1942, and this is shown plainly by the organization chart and the documents."

And the prosecutor responds:

> "Leave the documents for a moment. We have already agreed that the extermination of the Jews was not a part of your sacrosanct organization chart..."

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)).

Two days before his hanging, on 29 May 1962, Eichmann wrote a letter to the Israeli president asking for clemency.[34] 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..."

A recent literature has explored principal-agent relationships in the context of morally questionable tasks. Bartling and Fischbacher (2012) find that managers 'shift the blame' for a morally questionable task to subordinates. In turn, subordinates hide behind their leaders, as the Eichmann example shows. More broadly, teamwork creates 'moral wiggle room' for each of the group members when implementing tasks (Dana et al., 2007). Operating in a specialized team diffuses responsibility among its members, and allows

[34]The letter is publicly available, see here: https://www.documentcloud.org/documents/2698866-Handwritten.html (accessed November 2019).

individuals to, as Eichmann describes, carry out simple orders without feeling - or acting as if they do not feel - responsible for the ultimate outcome of one's actions.

Naturally, knowing that responsibility diffuses within an organization leads managers to hire others to do their 'dirty work' (Hamman et al., 2010). Prominent Nazi figures, for example, would display 'willful ignorance' of what went on in the concentration camps during the Nuremberg trials.[35] In the concentration camps, there is evidence that the process of offloading prisoners from trains, stripping them, and sending them to the gas chambers was split up into small tasks so as the reduce the moral burden on camp employees (Waller, 2007). Besides a rational analysis of delegating and hiding behind the limited responsibility of a specialized task can be - it was heavily debated whether Eichmann truly believed his limited role or whether his defense was a ploy - psychologists have long worked on the psychological aspects of acting immorally.

In brief, the psychological basis of behaving immorally in specialized environments is threefold. First, the infamous 'bystander' effect is the psychological finding that in groups, individuals are less likely to act morally. Responsibility 'diffuses' among bystanders (Latane and Darley, 1968). Bénabou et al. (2018) provide a microfoundation for this effect, as well as for Eichmann's defense: By ignoring the spillover effects of one's immoral behavior, agents can build a more convincing narrative to themselves for why their behavior is excusable. Second, Falk et al. (2020) find evidence for a 'replacement logic' in deciding to undertake immoral tasks - 'if I don't do it, someone else will'. This motivation seems particularly pertinent for tasks that were very similar to what municipal employees had been doing before deportations, like cataloging appropriated Jewish assets, finding *Judenhäuser* or making sure trams were available for transportation of German Jews to the train stations. Finally, early studies of authority, most famously the Milgram experiments, document a strong effect of authority figures' instructions (Milgram, 1963).

In light of this literature, specialization and diffusion of responsibility have been interpreted as two sides of the same coin. More specialized local bureaucracies may both be more efficient but reduce individuals to 'cogs in the wheel'. Being a small part of a big organization has the effect that bureaucrats become less morally encumbered when directives are potentially repugnant.

10 Conclusion

This paper studies the effects of an efficient local bureaucracy across periods with radical policy changes. More effective municipalities deport Germany's Jews more effectively when the Nazis pursue this policy, and protect Jews better before. I provide evidence that the changing effects of a well-organized

[35] On feigning ignorance to avoid moral responsibility, see Bartling et al. (2014).

bureaucracy on outcomes are due to its internal organization. In particular, I provide evidence for a 'cog in the wheel' interpretation of the German bureaucracy. Prussian bureaucracy is more specialized than non-Prussian bureaucracy.

Taken together, the results in this paper suggest that state capacity, once established, becomes a tool for 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. 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 be helpful 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 profitable explored further.

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