The Power of the State in German History

Development and Political Economy

Inaugural-Dissertation
zur Erlangung des Grades
Doctor oeconomiae publicae (Dr. oec. publ.)
an der Ludwig-Maximilians-Universität München

2019

vorgelegt von

Cathrin Mohr

Referent:	Prof. Davide Cantoni, Ph.D.
Koreferent:	Prof. Fabian Waldinger, Ph.D.
Promotionsabschlussberatung:	24. Juli 2019

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Für Matthias
All ways, always

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Introduction

Governments play a central role in shaping the lives of their constituents; they decide to whom to allocate state resources, they determine how taxes are raised to finance government spending, and they shape their population's beliefs.

Despite an extensive literature on these issues, important questions remain unanswered. While economists have analyzed the drivers of and incentives for the allocation of resources in democracies, mechanisms of resource allocation decisions in autocracies are less clear. One important difference between these regimes is that autocratic governments not only allocate resources, but also tools of repression. We do not know whom autocratic regimes target in their allocation decisions, and how the allocation of resources and repression interact. Another important topic that is not yet resolved in the literature concerns one of the ways how governments generate revenues, i.e. taxes. Independent of the type of government, the power to levy and collect taxes is taken for granted by most citizens around the world – and in theoretical models in the economics literature. However, in reality, countries differ in their ability to do so, but we cannot explain why this is the case. Last, there is a growing interest in the way in which governments actively shape their citizen's beliefs and attitudes. The circumstances under which this endeavor is successful are not clear, and warrant further study.

This thesis consists of three chapters that turn to German history as a fruitful ground to empirically examine these questions. There is a "plethora of Germanies" (Emslie, 2015, p. 2), and, as German history has taken many turns, these *Germa-*

nies were governed by different types of governments that faced different incentives and had different objectives. In the three Chapters of this thesis, I turn to three different periods in history, during which Germany was ruled by different regimes. Chapter 1 looks at the German Democratic Republic (GDR) and provides the first empirical evidence on the allocation of resources and tools of repressions, and their interactions, in an autocratic regime. Chapter 2 turns to the Holy Roman Empire in the Middle Ages which was characterized by a large number of small, independent territories competing against each other. It traces the reasons for and effects of the introduction of the earliest form of fiscal administration. Last, Chapter 3 focuses on the American and British zones of occupation in West Germany after World War II to examine the circumstances necessary for externally imposed government programs to alter the beliefs of a population.

In addition to sharing the focus on episodes from German history, all three chapters draw on extensive novel datasets for the empirical analysis. Chapter 1 builds on administrative data that was collected in the GDR and after its demise, as well as on information collected by military historians. Data are at the municipality level, and thus enable an unprecedented fine-grained look at the workings of the GDR. The empirical analysis of Chapter 2 is based on detailed information on the history of cities and territories that was collected by regional historians. Working with geographic information systems, we are able to project these data across space and to generate novel maps of territorial expansion and retraction. Furthermore, city level information allow us to study micro level processes that are linked to changes at the territory level. Chapter 3 employs historical data from various archives and handbooks as well as information from online maps on current day street naming patterns. Using this data, we are able to study the persistence of beliefs at the municipality level, taking into account regional variation in the implementation of the denazification program.

In Chapter 1, I examine how an autocratic regime allocates resources and tools of

repression towards areas that have shown opposition against the ruling regime. I argue that autocratic regimes face a trade-off: allocating resources to opposition areas increases the popularity of the regime among the opposition, but at the same time increases the incentive to engage in behavior that signals opposition, such as protesting. Allocating tools of repression, on the other hand, decreases the popularity of the regime, but also decreases the incentive to signal opposition. To proxy opposition, I look at the occurrence of protests during a country-wide uprising in the GDR in 1953 and examine differential allocation of resources and repression in areas that engaged in protests. Before these protests, construction and military presence did not differ in municipalities that would and municipalities that would not protest. After the uprising, protest-municipalities experience an increase in construction per capita and in the number of military troops. Protests, of course, did not occur randomly, and I address this concern by ruling out alternative explanations. I show that the differences in construction after 1953 are not driven by differences in the need for residential construction, or the share of construction or industry workers in 1950. Differences in the number of military troops after 1953 cannot be explained by distance to West Germany, historical military presence or the presence of Soviet military troops in protest municipalities. Construction increases after the introduction of (additional) military troops in municipalities, which is not driven by construction for military personnel. This suggests that the GDR regime used construction as a tool to alleviate the negative effect on popularity that military troops had.

In addition to expanding our understanding of the political economy of one particular autocracy, this chapter broadens our understanding of autocracies, and the trade-offs they face, more generally. Usually when we think of autocratic regimes, we are more likely to associate them with the use of repression when facing their opposition. However, this Chapter shows that autocratic regimes target their opposition also with resources, and that this could be driven by the desire to alleviate the negative effect repression has on the regime's popularity.

In Chapter 2, Davide Cantoni, Matthias Weigand, and I trace the rise of fiscal capacity in the territories of the Holy Roman Empire. We compare our findings to predictions from the theoretical literature, which argues that threat of war allows governments to introduce fiscal institutions to finance military investments that protect their population. These institutions allow territories to survive and experience more economic growth. We analyze the causes and effects of the introduction of so-called *Chambers* as a first step towards a professionalized fiscal administration. *Chambers* were centralized, permanent institutions that were in charge of collecting and organizing revenues. They were introduced in some territories of the Holy Roman Empire between the late 15th and 18th century. In line with predictions from the theoretical literature we find that territories are more likely to centralize if other centralized territories exist in their vicinity and if they are exposed to a more bellicose environment. Centralized territories are less likely to vanish and are larger than non-centralized territories as a result, even after taking into consideration constant differences in the size of territories that eventually centralize. They invest more in administration and military, however, additional investments into military are not spread equally across centralized territories, but instead are limited to *core* cities of territories. There is no additional construction in *peripheral* cities after they become part of a centralized territory. This conflicts with the key assumption of many models that military investments of states are public goods and that thus all inhabitants of a territory can profit from them and are willing to finance their provision.

The contribution of this paper is fourfold. First, we document the patterns of territorial expansion and retraction within the Holy Roman Empire in great detail, and in doing so add to the understanding of European history during the time of the rise of Europe. Second, we collect a number of outcomes which quantify fiscal centralization and its drivers and consequences. Third, we use these data to empirically examine what causes fiscal centralization, and fourth study its effects.

In doing so, we confirm a large number of predictions from the theoretical literature on the origins and effects of fiscal capacity. However, we also show that one of the core assumptions of many models – that investments into the military are a public good – do not hold in the Holy Roman Empire.

Chapter 3 is joint work with Nico Voigtländer and Hans-Joachim Voth. We examine when government programs are successful in altering the beliefs of their constituents in the context of denazification in Germany after World War II. The denazification program aimed at removing National Socialist ideology and in particular anti-Semitism among Germans. The policies implemented to achieve this goal differed across occupation zones, and we compare the effectiveness of the American and the British approach. The American occupation government pursued a highly ambitious and punitive program which punished many Germans, and which was perceived as being harsher to minor perpetrators than to major ones. British authorities on the other hand followed a more pragmatic approach to denazification and mostly focused on major perpetrators, in many cases neglecting to look at minor ones. We show that there is a persistence of anti-Semitism in the former American zone, but not in the former British zone. We explore three potential channels that potentially explain this pattern by looking at within zone variations: differences in the harshness of punishment of individual Germans, cooperation of the local administration with denazification directives, and emphasis on collective guilt. We only find evidence in line with the first potential channel; harsher punishments during denazification seem to explain why denazification in the US zone was less successful.

The findings of Chapter 3 help us understand why in some cases government interventions aiming at changing the beliefs of citizens fail. Countering anti-Semitism is one example of a belief that governments are actively trying to influence, even today. The number of anti-Semitic incidents is on the rise in Europe, and governments around the EU are currently discussing potential policies to tackle this (The Economist, 2019). The findings of Chapter 3 suggest that the way in which these

policies are designed have a large effect on their effectiveness. Policies that are perceived as being too harsh by the population can have converse effects, and increase anti-Semitism, instead of lowering it.

Each of the following three self-contained chapters is followed by an Appendix which contains supplementary materials. References are presented in a consolidated bibliography at the end of the thesis.

1 | Carrots and Sticks: Targeting the Opposition in an Autocratic Regime

[O]ne ought to be both feared and loved.

Machiavelli (1513)

To stay in power, autocratic regimes must ensure that they are not overthrown by their opposition in the population. They possess two main policy levers to achieve this: carrots, in the form of increases in citizens' welfare, and sticks, in the form of repression. However, these two tools are associated with trade-offs, and it is not clear how autocratic rulers can overcome these: while using carrots to buy off the opposition increases the regime's popularity, this policy also signals that engaging in opposition against the government is beneficial. Targeting the opposition with sticks decreases the opposition's ability to overthrow the regime, but at the same time increases their discontent with the government and thus the willingness to engage in opposition behavior. One potential way to solve this dilemma could be to employ carrots and sticks at the same time, as has already been suggested some 500 years ago by Machiavelli (1513). This way, autocratic leaders could distribute carrots to raise their popularity without increasing incentives for citizens to oppose the regime. So far, we only have a very limited understanding to what degree autocratic regimes target the opposition with carrots and sticks. The existing literature has mostly focused on the use of either of the two policy levers across countries.[1] It

[1] Gandhi and Przeworski (2006) suggest that when the threat of popular opposition increases, dictators are more likely to share rents, i.e. use carrots. Desai et al. (2009) provide evidence

has abstracted from potential inter-dependencies between them and their spatial allocation within countries.

Understanding the political economy of the joint allocation of resources and repression in autocratic regimes is a highly relevant endeavor. Around half of the world's population lived in autocratic regimes in 2017 (The Economist Intelligence Unit, 2017). Historically, the vast majority of people have lived in some form of autocratic regime. If the opposition in autocratic regimes is treated differently than other groups, this has far-reaching effects on the distribution of welfare within autocratic regimes.

In this paper, I empirically analyze how autocrats target their opposition with carrots and sticks in the context of one specific autocratic regime. To do so, I look at housing construction, military establishments, and the surveillance apparatus at the municipality level in the German Democratic Republic (GDR), before and after a wave of protests in 1953. These protests informed the government on where opposition was located. I find that housing construction and military presence increased in protest municipalities after protests occurred. Figure 1.1 shows the number of newly constructed buildings and flats per 1,000 inhabitants from 1946 to 1989.[2] Before protests in 1953, per capita construction developed nearly identical in both groups of municipalities. After some municipalities engaged in protests, they subsequently experience higher levels of construction until 1989. In addition, protest municipalities are more likely to receive military units in particular after the estab-

that carrots and political influence are negatively related, in line with the "authoritarian bargain" theory. Davenport (2007b) analyses how the use of sticks differs across different forms of autocratic regimes instead of the decision on their allocation. Gregory et al. (2011) argue that if a regime does not have precise information on who opposes the regime, they use more sticks. For a broad overview of the literature on repression refer to Davenport (2007a). There are some papers that look at both carrots and sticks. Wintrobe (1990) provides a rational choice model where dictators choose carrots and sticks based on their costs. Another paper that looks at both policy levers is the theoretical framework provided in Gerschewski (2013). He looks at legitimization that can be driven by provision of carrots, repression and co-optation of relevant elites, and how they influence each other. However, both papers do not address the question who is targeted by carrots and sticks within a country.

[2]In this paper, I restrict the analysis to municipalities that had between 2,000 and 10,000 inhabitants in 1950. The graph with all municipalities that I can match is shown in the appendix (Figure A.1).

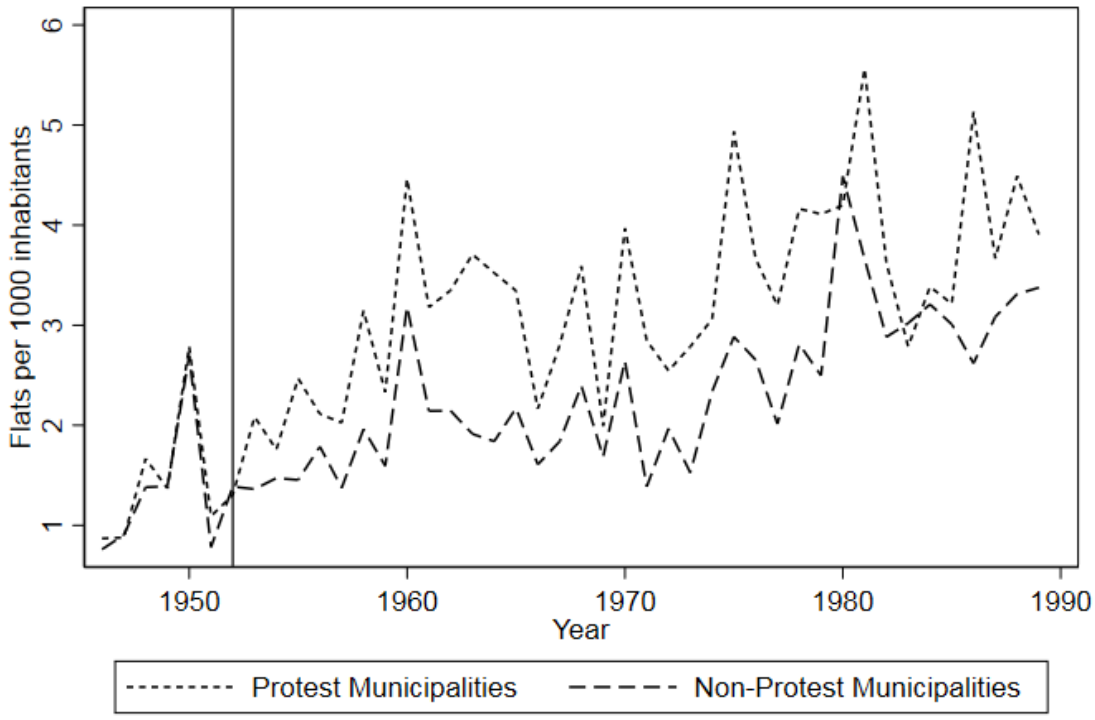


Figure 1.1: Flat Construction per Capita at the Municipality Level

Note The figure shows average construction of flats per 1,000 inhabitants in protest and non-protest municipalities for all municipalities that have between 2,000 and 10,000 inhabitants in 1950. The vertical line indicates the year 1952. Data sources: see text.

lishment of the military in 1956, and are more likely to have any Stasi objects in 1989. Before the official establishment of the military, future protests do not predict where para-military units are located before 1953. After 1953, protest municipalities receive much more military units than would be predicted given their observables until 1989.

I link the timing of the arrival of military troops in a municipality to residential construction to study the interaction of carrots and sticks. I show that all municipalities see an increase in residential construction at the time sticks are introduced, but this does not explain the difference between protest and non-protest municipalities. This increase is not driven by residential construction for military troops themselves. Thus, this paper provides novel evidence that autocratic regimes jointly target carrots and sticks towards the opposition, and that they use carrots as a tool to alleviate the negative effect of sticks.

Construction activity, the military apparatus and surveillance in the GDR offer an ideal setting to study the allocation decisions of non-democratic regimes for three main reasons:

First, new residential housing and the allocation of military units are good measures of the allocation of *carrots* and *sticks* in the context of the GDR. Lack of adequate housing was one of the main complaints of citizens to the government throughout the GDR's existence. Thus, studying the construction of residential buildings focuses on one exemplary allocation that was highly welfare relevant. The National People's Army (NPA), the GDR's military, was regarded by the government as a potential force against the citizens.[3] The military is one of the most extreme measures of repression governments can turn to. Thus, the allocation of military units provides a measure of where the government targeted potentially violent means against their citizens. Complementing this with information on the location of Stasi objects at the end of the GDR also provides insights into a second important stick in the context of the GDR.

Second, the history of the GDR provides us with a wave of protests in 1953 which elicited the location of the opposition to the government (and the econometrician). In the setting of authoritarian states, where people usually hide their opposition to the government in fear of retaliation, protests can be seen as an information signal on local discontent (see for example Lorentzen, 2013, for the case of China). The uprising thus presents a signal about the spatial location of opposition to the government. There have been no other large scale protests until 1989, so that these protests continued to provide an important information signal for a long time.

The third reason relates to data availability and trustworthiness. Non-democratic regimes often do not publish reliable information on themselves. I collected a novel, extensive dataset on the municipality level from numerous sources that overcomes

[3]In contrast to other settings where there would be positive spillover effects on the surrounding economy by the military, this is not a concern here. The GDR's planned, Socialist economy guaranteed full employment throughout the country, and military units did not increase local living standards.

this problem. Data on housing and military units in the GDR were collected retrospectively by statistical offices of the Federal Republic of Germany and military historians after the GDR ceased to exist. I supplement this data with novel information on housing needs, population, and occupation structure at the municipality level, which I have collected from internal archival materials of the Statistical Office of the GDR. During the existence of the GDR, only a selected group of bureaucrats had access to this information.[4] Furthermore, I add information on the location of objects of the Secret Police, the Stasi, at the municipality level in 1989 that has not been used for empirical analysis so far.

This paper relates to the theoretical and empirical literature on what drives the allocation of resources in autocratic regimes.[5] Empirical studies for autocratic settings are rare due to the data concerns discussed above. Lazarev and Gregory (2003) analyze the allocation of vehicles in Soviet Russia in the 1930s. Examining requests for cars and the decisions of the allocation commission, they find evidence in line with a political gift exchange model. Closest to the analysis in this paper is Thomson (2017), who also looks at the reactions of the GDR government to the Uprising of 1953. Thomson's paper focuses on the power struggle between hard and soft-liners within the ruling elite. He finds that there is no correlation between protest activity and food allocation afterwards at the level of counties (*Kreise*), but protest counties seem to receive more unofficial Stasi informants after 1953. While the two papers look at the Uprising of 1953, they differ in their methodology and focus. My paper studies allocation at a much finer level, municipalities instead of counties, extends from 1946 until 1989 and covers the whole area of the GDR. Housing, in contrast to food, was scarce in the GDR, and citizens regularly complained about the hous-

[4]Most of the statistical material collected in the GDR was never published. Consensus is that data intended for internal use are of high quality, as it formed the basis of policy decisions (Statistisches Bundesamt, 1999, p. 28)

[5]There is a much larger literature on the allocation of resources in democracies. This literature focuses on how governments try to increase their chances of reelection and their election shares. It is not clear from a theoretical viewpoint whether they want to target their supporters or swing voters. Empirically, there is also no unequivocal evidence. Some papers find that swing voters receive larger allocations (Johansson, 2003), others that loyal voters receive larger allocations (Ansolabehere and Snyder, 2006), others both (Case, 2001).

ing situation. Looking at the location of military troops measures a more extreme form of a stick. This stick could be used to stop any potential future protests that threaten the regime. The Uprising of 1953 was stopped by Soviet military troops, and made it apparent to the GDR government that without control over military troops they might be overthrown.[6] In addition, my paper is able to empirically elicit how carrots react to the existence of sticks.

This paper also links to the growing literature on the political economy of autocracies that focuses on the constraints and incentives faced by dictators. Wintrobe (1990, 1998) models the behavior of dictators according to a rational choice model. He argues that dictators can use repression and loyalty to ensure that they stay in power, and choose the optimal mix of these two approaches based on the trade-offs they face.[7] Gershenson and Grossman (2001) examine the case of the Soviet Union, where cooptation into the ruling party was used as a carrot, and how this reacts to external and internal threats. Guriev and Treisman (2018) focus on how autocracies can survive without the use of mass repression. Lorentzen (2013) provides a model in which autocratic regimes allow protests to occur – as long as these protests do not threaten the regime – to get information on grievances held by the the population and on the performance of lower level bureaucrats. In addition, he provides empirical evidence on this channel in China. I add to the understanding in this literature by demonstrating how carrots and sticks can be used simultaneously in an autocratic regime, and who is targeted by these.

Last, this paper is related to the literature looking at the effects of protests on policy outcomes in non-democracies. Aidt and Franck (2015) show that protests also matter when protesters do not have voting rights, as elites fear for their power and are thus willing to make policy concessions. There is also evidence that protests have an effect on the perceived value of firms (Acemoglu et al., 2018) or property

[6]Information on the allocation of food and the location of Stasi informants is not available on the municipality level.

[7]Other influential papers modelling the behavior of autocrats include, but are not limited to, Acemoglu and Robinson (2005), Olson (1993), Bueno de Mesquita et al. (2003).

prices (Collins and Margo, 2007). This paper provides evidence that protests have twofold effects: they lead to an allocation of carrots and sticks.

The remainder of the chapter is structured as follows: Section 1.1 gives a short overview over the historical background of construction activity, the military and protests in the GDR. Section 1.2 introduces the data and their sources employed in the empirical analysis. Section 1.3 explains the empirical framework, and the results first for carrots, i.e. construction, and then sticks, i.e. military and Stasi presence, and their interaction. Section 1.4 discusses potential channels and section 1.5 concludes.

1.1 Historical Background

The German Democratic Republic (GDR) was founded in the Soviet occupation zone in Germany after World War II. It existed until 1989/90, and spanned the eastern part of Germany except for West Berlin. The GDR was an authoritarian, socialist country with a centrally planned economy. The ruling party was the Socialist Unity Party of Germany, called SED. The administration of the GDR was organized hierarchical. The smallest administrative unit were municipalities (*Gemeinden*), which were in turn subordinate to counties (*Kreise*), which were subordinate to regional districts (*Bezirke*).

1.1.1 Uprising of 1953

A wave of protests in 1953 had extensive impacts on the subsequent history of the GDR. The so-called Uprising of 1953 began in Berlin with a strike against an increase in working hours on June 16th 1953. While the rise in working hours was taken back on the same day, people engaged in protests in more than 700 municipalities over the next days. Around 10 percent of the population took part, making this the

largest instance of protest activity in the history of the GDR. The ensuing protests were no longer linked to working hours, instead protesters had a variety of demands, such as reunification, democracy and higher living standards (Kowalczuk, 2003). In some cities, protesters could only be stopped by Soviet troops and tanks, and there were at least 55 casualties.

Ruling SED elites were completely taken by surprise by these events. They were especially shocked that many (industrial) workers – i.e. those people that the government claimed it was representing – had shown their discontent with the government. After the protests, the government began to follow a carrot and stick approach to prevent a second uprising (Diedrich et al., 1998, p. 202). The regime began to focus on raising living standards, for example by increasing residential construction, while at the same timing also starting to build up an extensive security apparatus, for example by founding the National People's Army or increasing the size of the secret police. Until 1989 there were no other large scale, country wide protests in the GDR.

1.1.2 Housing in the GDR

After World War II, around 10 percent of the housing stock in the GDR was destroyed. In the first post-war years there was only little construction, instead the government expropriated home owners to assign new residents to their houses and tried to (provisionally) repair destroyed flats. Authorities could ban migration to municipalities in which housing was too scarce, but even outside of such municipalities, every change of flats within or across municipalities required state approval. Since 1949 the Ministry for Reconstruction (*Ministerium für Aufbau*) was in charge of planning, running and controlling residential construction, which included the construction material industry and construction companies. In 1952, the Ministry also took over leadership over the local construction authorities of the Regional District Councils (*Räte der Bezirke*). After the Uprising of 1953, the Regional District

Councils were in charge of drafting plans on residential housing investment and construction decisions, while the economic leadership in East-Berlin decided which of these plans to implement. All decisions on the allocation of housing were made on the local level, i.e. in counties and municipalities. The government also started to promote individual, private construction via subsidies around this time, but most government support went into state-led construction by cooparatives of workers, employees, and farmers (Melzer and Steinbeck, 1993, p. 16).

Over time, local institutions were equipped with more decision-making power. Local building authorities were established in 1958. The central government set overall target numbers of construction and decided on centers of construction, and local administrative organs then planned and oversaw residential construction programs on a day-to-day basis. Counties decided on how to allocate the construction targets across municipalities within their county (for more information see Melzer and Steinbeck, 1993; Buck, 2004).[8]

Figure 1.2 shows aggregate flat construction from 1946 to 1989 based on the data used in this paper. After the end of World War II, construction of flats increased until 1950, dropped and then remained relatively constant until 1958. In 1951 the first five year plan of the GDR was implemented, which laid out target construction levels until 1956. Aggregate construction could thus not be raised directly after the Uprising of 1953. Construction levels increase in 1958, when the second five year plan was introduced with some delay. After 1961, the year in which the Berlin Wall was erected, flat construction declined until 1970. In 1970 the government launched a comprehensive housing construction program to solve the problem of housing shortages until 1990. Construction of flats increased until 1981, after which a new

[8]This division of responsibilities was enforced in reality. Materials for construction were only given out by districts and counties, not by central authorities. In a meeting of functionaries involved in rural construction the provision of building materials by central institutions instead of local ones is rejected: *"Regional districts and counties decide on the way of allocating construction materials. [...] Therefore we adhere to our principle that the allocation of construction material is not made by the Ministry for Reconstruction, but only by the regional districts and counties. The authority of the regional organs must not be undermined, but has to be raised systematically."* (Ministerium für Aufbau, 1957, p. 17f, own translation).

five year plan began to prioritize exports. Consequently, all domestic investments, including those in housing, were cut.

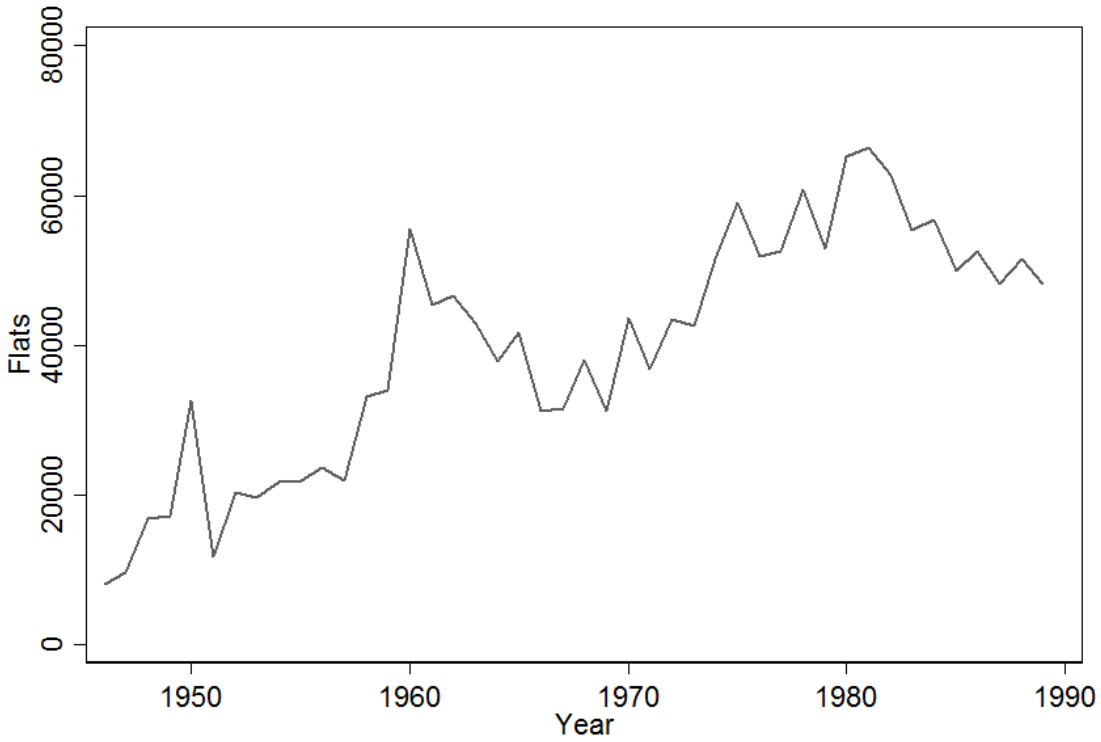


Figure 1.2: Overall Flat Construction

Note Overall new flat construction (all types) from 1946 to 1989. Data is from the building and flat census of 1995 (*Gebäude- und Wohnungszählung*), which was conducted by the Statistical Offices of the German Federal States in former East Germany.

In 1989 before the breakdown of the GDR, officials admitted that the GDR would not be able to reach their ambitious goal of solving the housing crisis until 1990. They also recognized that only two thirds of all planned construction until 1990 had been undertaken so far. There were several reasons for this. Actual construction lagged behind official plans due to a lack of construction materials. State-led construction had priority over the renovation of existing buildings and private construction, which led to a further deterioration of the housing stock and made the scarcity of housing and the emerging low living standards even worse (Melzer and Steinbeck, 1993, p. 11, Bouvier, 2002, p. 158). The lack of construction had large effects on people's satisfaction with the regime: throughout time the largest share of citizens' petitions (around one third) to the government related to the housing

situation (Buck, 2004, p. 258f).[9] The government was acutely aware that the lack of (adequate) housing influenced people's opinion of the government.[10] There is also evidence that the government tried to alleviate housing concerns of opponents of the regime. A report from the administration of the regional district Berlin from 1981 discussed the difficulties of finding a flat in the GDR as a driving factor of petitions of GDR citizens who wanted to resettle to West Germany. Even though people who wanted to relocate to the West were seen as opponents of the GDR, the administration elaborately discussed how to solve the underlying housing issues and organized new flats for petitioners wherever possible.

1.1.3 The National People's Army

After World War II, East Germany was demilitarized and initially banned from establishing an army. Re-militarization first started indirectly in 1949 with the establishment of police units that secretly had a military character, and that were in 1952 transformed into so-called barracked police units (*Kasernierte Volkspolizei*). These were highly armed, barracked police units, that only differed from real army units through their label. When the National People's Army (NPA) was officially founded in 1956, these barracked police units were immediately renamed and incorporated into the NPA.

The experience of the Uprising of 1953 also shaped the development of the military. The military was seen as one part of the security apparatus that could be targeted against the population. At the end of June 1953, the para-military baracked police units were assigned more resources to increase their size and effectiveness. In the next year, the politburo presented plans with measures that armed forces should

[9] These petitions were basically letters of complaints to the government. Unfortunately, the petitions were not collected and thus no longer exist today.

[10] An internal report of the Secret Police, the Stasi, commented that after the discontinuation of a local construction project in 1962 that *"people had lost confidence in the workers' and farmers' state"* and that as a result some people wanted to leave for West Germany as there they *"would be able to build."* (see Ministerium für Staatssicherheit (1962, p. 3), own translation.)

take against the population in case of internal unrest. The pattern of introducing structures for the use of armed forces against GDR citizens continued with the official foundation of the military in 1956.[11] Especially in the early years until the 1960s, the NPA focused much more on internal – as opposed to external – threats. Nevertheless, the military always continued to play an important role in internal security considerations until the end of the GDR. For example, when protest activity in 1989 began to spread across the country, the government discussed the potential involvement of military troops (which it then decided against).

Unlike in other settings, regions in the GDR were not keen on attracting army facilities. During the entire existence of the GDR, not a single municipality or city ever tried to attract military establishments. There were only very few people who benefited from military establishments economically, while for most people they just provided an economic and social burden (Kersten et al., 2011, p. 36). In addition, citizens were very likely aware that the government saw the military as a potential tool against its citizens.

1.2 Data

Data on authoritarian regimes are often unavailable or not trustworthy.[12] This makes it very difficult to empirically study authoritarian regimes. These concerns also apply to the GDR, where official residential construction statistics for example counted every space in a nursing and elderly home as a flat (see Statistisches Bundesamt, 1993, p. 6). To overcome this challenge, I rely on data that were collected retrospectively after the fall of the GDR, and data that were collected for internal purposes only.

[11] For a more detailed dicussion on the Uprising of 1953 and its effects on the military refer to Diedrich et al. (1998).

[12] Hollyer et al. (2011) for example find that non-democracies are less likely to disclose policy-relevant data and Magee and Doces (2015) provide evidence that they overstate their growth rates.

1.2.1 Data Sources

Residential Construction and Housing Demand

Data on residential construction are from the building and flat census of 1995 (*Gebäude- und Wohnungszählung*), which was conducted by the Statistical Offices of the German Federal States in former East Germany. The census includes every residential building and flat existing in 1994.[13] A residential building is defined as a building of which at least half of its area is used as living space. A flat is defined as any number of co-joined rooms used as living space, which have their own entrance (Statistisches Bundesamt, 2016). Data on buildings include information on the construction year, ownership structure in 1990, and the prevalent heating system in the building. Flat data additionally include the number of rooms and information on the size of flats. Using the information on the year of construction, I am able to create a municipality-year panel of construction. To make the data more comparable between urban and rural areas where the number of living units within a building might differ, I focus on the flat data. I classify all flats that were labeled as being privately owned in 1990 as private, and all flats that were labeled as either municipal, public property, belonging to workers' and charitable socialist building cooperative societies, agricultural production cooperatives or were state-owned as state flats.

I normalize construction levels by population size measured in thousands. My dataset includes population data for 1946, 1950, 1964, and 1971. 1946 population data are from Falter (1999) and 1964 population data are from publications of official GDR statistics (Staatliche Zentralverwaltung für Statistik, 1966). Population data from 1950 and 1971 are from archived internal records of the Statistical Office, which I digitized.[14] Based on the available years I interpolate and extrapolate

[13]To the best of my knowledge the demolition of buildings in the former GDR only started with the funding program for city redevelopment East (*Förderprogramm Stadtumbau Ost*) in 2002.

[14]These records are available at the Bundesarchiv Berlin-Lichterfelde. The population census

population years linearly for all other years.[15]

I also collected measures of housing demand at the municipality level around 1953: war destruction in Saxony (one region of the GDR) in 1945[16], and the number of available flats and the number of households looking for flats from the flat demand census in January 1954.[17]

Military units and Stasi presence

Information on military units in the GDR come from Kersten et al. (2011), who provide information on the history of military establishments in East Germany. From this I generate a municipality-year level panel with information on the location and foundation year of National People's Army units, the presence of barracked police establishments and Soviet military troops, as well as the historic presence of Wehrmacht establishments.[18] I supplement this with information from an historical account on the barracked police (Diedrich and Wenzke, 2001), a location database on the National People's Army and the Soviet forces by the Military History Research Institute (*Militärgeschichtliches Forschungsamt*)[19], and the online catalog of the Military Archive in Germany[20].

As a second measure of a stick in the GDR, I turn to the presence of the secret

1950 can be found in the records DE/2/22320-DE/2/33232 and the 1971 population census in the records DE/2/33057-DE/2/33062.

[15]Population data for 1990 would also be available at the municipality level. However, these numbers show population after the introduction of freedom of movement. If people from areas with and without opposition left the area of the former GDR at different rates, this would then bias the interpolated population estimates after 1971. If the extrapolation of population generates a negative population value, I set this to 0.

[16]From the record DH/1/45781 at the Bundesarchiv Berlin-Lichtenfelde.

[17]The survey provides a snapshot of the situation on January 31st, 1954, i.e. around half a year after protests took place. Unfortunately no comparable information is available for the time before June 1953. I assume that there have been no large changes between June 1953 and January 1954. The records are available at the Bundesarchiv Berlin-Lichterfelde DE/2/1-13.

[18]Unfortunately the foundation year is not included for every military unit. I thus exclude these units from my empirical analysis.

[19]The database can be accessed under `http://www.mgfa.de/html/standorte_einleitung.php` (last visited January 23, 2018).

[20]The catalog can be accessed under `https://invenio.bundesarchiv.de/basys2-invenio/login.xhtml` (last visited January 23, 2018).

police of the GDR, known as the Stasi. Detailed information on Stasi activity over time at the municipality level is not available. I use data on the presence of Stasi objects at the municipality level in 1989 as a proxy for overall Stasi presence. These information are from a list of all former Stasi objects that were dissolved in 1990 that was published in a German newspaper in June 1990 (taz, 1990). Objects can be differentiated according to whether they were public, such as office buildings that were known to belong to the Stasi, or disguised, e.g. in the case of flats that were used for secret meetings.

Protest Data

I interpret the incidence of protest activity as a signal that some opposition exists within a municipality.[21] Data on protests in 1953 are from Kowalczuk (2003), who provides a list of 698 places for which either a protest, demonstration, strike or violence against individual persons or institutions between the 16th and 21st of June 1953 is documented. Based on this, I generate a dummy variable that takes the value 1 if some form of protest occurred within a municipality. Kowalzcuk notes that while it is likely that all places with at least 10,000 inhabitants which experienced some form of protest are included, the same cannot be said for places with a lower number of inhabitants. This exclusion of smaller municipalities is likely to be random, and will therefore just have an effect on the precision of the estimates. Figure 1.3 presents the location of all protests that I could match distinctly to a municipality. Overall, I can match protests to 494 municipalities according to 1997 boundaries (see more on this below).[22]

[21] This is similar to Lichter et al. (2016), where differences in riot intensity in the GDR on the county level are used as a proxy for the strength of the opposition, or Lorentzen (2013) where the Chinese government uses local protest activity as an information signal about which social groups oppose the government.

[22] I am currently assembling an additional dataset on the centers of protest activity.

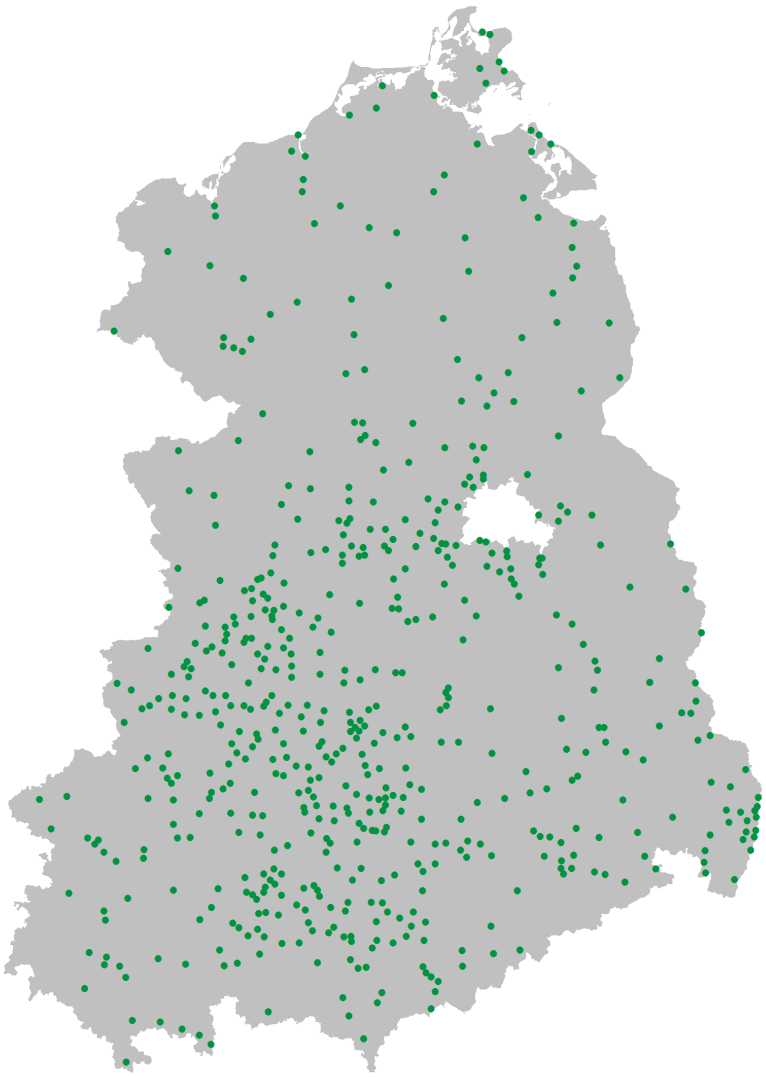


Figure 1.3: Protests during the Uprising of 1953

Note In this map of the German Democratic Republic, each circle denotes a municipality that had a protest event during the Uprising of 1953. This encompasses protests, demonstrations, strikes, or violence against individual persons or institutions between the 16th and 21st of June 1953. Data is from Kowalczuk (2003). The hollow white area is Berlin, which is excluded from the analysis.

Other variables

Additional variables are the voting shares for different parties in 1946, the last free election in the GDR, from Falter (1999). I collected information from job censuses for the years 1950 and 1971 from archival records.[23] In addition, I collected information on the names and years in office of Chairmen of the County Councils by contacting all relevant county archives,[24] and enhancing this with information from historical literature, historical newspapers, and Wikipedia articles.

Municipality Borders

The empirical analysis is conducted at the municipality level according to the municipality borders in 1997.[25] Using information provided by the National Statistical Office (Statistisches Bundesamt, 1995) and the Statistical Offices of the Federal States[26] all municipalities were aggregated according to their 1997 boundaries. I exclude all municipalities which had given up some parts of their area between 1948 and 1997 when I could not precisely identify which areas this included. I also exclude the municipalities that received this land.[27] Overall, I have information on approximately 5,000 municipalities out of 5,792 municipalities that existed in East Germany in 1997.

1.2.2 Summary Statistics

Table 1.1 presents summary statistics. In my empirical analysis, I restrict the sample to municipalities that had between 2,000 and 10,000 inhabitants in 1950. This makes the treatment and control group more comparable, as protests in 1953 were more

[23] Data of the job census 1950 do not span the entire GDR, as some of the archival records were of such bad quality that they could not be accessed. For some municipalities it was also not possible to get information on all sectors for the same reason. It can be assumed that this is random.

[24] I contacted all 69 county archives, that I could identify, of which 57 replied.

[25] 1997 is the first year for which official geocoded maps with municipality borders exist.

[26] Anna Gumpert and Nadja Dwenger kindly shared this information with me.

[27] This affects mostly large municipalities and cities.

likely to occur in larger municipalities. Around 200 municipalities in this restricted dataset experience a protest, slightly more than 750 do not experience a protest.[28] Before 1953, overall, state-led and private construction per 1,000 inhabitants was not statistical significantly different from each other in protest and non-protest municipalities. Each year around 1.4 flats were built in protest municipalities and around 1.3 in non-protest municipalities. After 1953, overall and state-led construction is higher in protest municipalities; overall construction is 3.3 flats per 1,000 inhabitants in protest municipalities, and 2.4 flats in non-protest municipalities, state-led construction accounts to 3.1 flats per 1,000 inhabitants in protest municipalities and 2.4 flats per 1,000 inhabitants in non-protest municipalities. There is no difference in private construction before and after protests occurs.

Before protests occurred, protest and non-protest municipalities were equally likely to host any barracked police units. Protest municipalities are more likely to ever house any military units. This changes after protests occur. 10 percent of all protest municipalities ever house any military units, 5 percent of all non-protest municipalities ever house any military units. The average number of troops that are stationed in a municipality in each year is larger in protest than in non-protest municipalities (0.12 compared to 0.06 troops). These differences are statistical significantly different at the 1 percent level. They are also more likely to have any secret Stasi objects in 1990 (around 40 percent of all protest municipalities have a secret Stasi object, and around 25 percent of non-protest municipalities have a secret Stasi object), but there is no difference in whether they have public Stasi objects.

Protest and non-protest municipalities differ in their observables. Protest municipalities have a larger population in 1946, 1950, 1964 and 1971 – even after restricting the sample to municipalities between 2,000 and 10,000 inhabitants. In 1950, protest municipalities had on average 4,915 inhabitants and non-protest municipalities had 3,943 inhabitants. The working population in 1950 was larger in protest compared

[28] When including places with fewer than 2,000 and more than 10,000 inhabitants, there are around 500 protest municipalities and 4,550 non-protest municipalities.

to non-protest municipalities. The share of votes for the SED in 1946 was larger in non-protest municipalities (0.461 compared to 0.444). Around 2 percent of the population in protest municipalities was looking for a flat in 1954, compared to 1.8 percent in non-protest municipalities. Protest municipalities were also around 30 km closer to Berlin, but 12 km further away from any external border of the GDR. The share of construction workers and industry workers in 1950 is not statistically significantly different from each other. The differences pointed out here will be incorporated in the empirical strategy and the robustness checks.

1.3 Empirical Framework and Results

In this section, I will first discuss where protests occur, before I estimate the effect of protests on construction as well as military and Stasi presence at the municipality level.

1.3.1 Where Do Protests Occur?

Protest activity did not occur randomly within the GDR, and this could potentially bias the results of the following empirical analysis. Protests might be correlated with other factors than opposition to the government that influence construction and military presence after 1953. To test which variables predict protest activity I estimate the simple linear OLS model

$$Protest_m = \beta_1 Population_{1953,m} + \beta_2 Controls_m + \alpha_d + \epsilon_m \tag{1.1}$$

where $Protest_m$ is a dummy indicating protest activity in 1953 in municipality m, $Population_{1953,m}$ is population in the year 1953 in m and $Controls_m$ are different controls accounting for potential differences in location, political preferences, population growth, industry structure, housing demand or military presence between

Table 1.1: Summary Statistics

	Protest Municipalities			**Non-Protest Municipalities**			
	N	Mean	SD	N	Mean	SD	Difference
Panel A: Construction before 1953							
New Flats per Capita per Year	208	1.424	2.522	762	1.329	3.493	0.095
New State Flats per Capita per Year	208	1.360	2.460	762	1.247	3.412	0.113
New Private Flats per Capita per Year	208	0.028	0.156	762	0.044	0.263	-0.016
Panel B: Construction after 1953							
New Flats per Capita per Year	208	3.332	7.649	762	2.360	6.021	0.972*
New State Flats per Capita per Year	208	3.182	7.563	762	2.172	5.574	1.010*
New Private Flats per Capita per Year	208	0.097	0.826	762	0.109	0.864	-0.012
Panel C: Sticks							
Barracked Police Unit in 1952	208	0.019	0.138	762	0.010	0.102	0.009
Ever any military	208	0.101	0.302	762	0.054	0.226	0.047**
Troops per Year	208	0.120	0.624	762	0.057	0.447	0.063***
Public Stasi Object 1990	177	0.198	0.399	639	0.106	0.309	0.091***
Secret Stasi Object 1990	177	0.401	0.492	639	0.257	0.437	0.144***
Panel D: Controls							
Population 1946	197	4966	2288	725	3868	1650	1098***
Population 1950	208	4915	2226	762	3943	1692	971***
Population 1964	207	4683	2745	749	3456	1692	1226***
Population 1971	205	4719	2895	756	3403	1736	1216***
Share Votes SED 1946	187	0.444	0.090	711	0.461	0.100	-0.0167**
Working Population 1950	130	2612	1162	510	2316	1019	295***
Share Construction 1950	118	0.049	0.019	490	0.045	0.032	0.005
Share Industry 1950	104	0.228	0.012	339	0.212	0.07	0.016
Share Buildings Destroyed 1945	46	0.073	0.135	294	0.059	0.132	0.017
Share Population Looking for Flat 1954	160	0.020	0.011	546	0.018	0.012	0.002*
Distance to Berlin	208	138.051	62.949	762	166.840	59.742	-28.788***
Distance to any Border	208	47.918	30.259	762	35.587	28.812	12.331***

Note Mean and standard deviation for municipalities with and without any protest activity in 1953, respectively. Sample is limited to municipalities with 2,000 to 10,000 inhabitants in 1950. The column *difference* reports the difference in means between the two groups. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

protest and non-protest municipalities. α_d are county fixed effects according to county borders in 1953. Standard errors are clustered at the county level.[29]

Table 1.2 presents the results of this linear probability model. All regressions control for population in 1953 – which is correlated with protests at the 1 percent level in all specifications – and county fixed effects. In column 1, I examine the location of protests. Municipalities that are closer to Berlin are more likely to have protests in 1953 (significant at the 10 percent level). As protest activity started in Berlin and spread from there, this is not surprising. County capitals are also more likely to have protests in 1953 (significant at 1 percent level), this might be explained by the fact that county capitals were the center of state activity. They thus provided protesters with the opportunity to protest visibly for representatives of the regime they were protesting against. If protesters from surrounding municipalities traveled to county capitals to protest there, we would expect that distance to county cities positively predicts the existence of protests, because most likely people from closer municipalities would be more likely to travel to county capitals instead of protesting in their home municipality. This does not seem to be the case empirically, there is no statistically significant relationship between distance to county capitals and protests at the municipality level. Distance to the West border excluding Berlin or the county capital do not explain where protests take place. In column 2, I look at the role of political preferences of the local population. In 1946, the only democratic election of the GDR took place. Municipalities with higher share of votes for the SED were more likely (1 percent significance level) and places with a higher turnout are less likely (5 percent significance level) to protest. This suggests that protests in 1953 provided the government with new information on the existence of the opposition, which they could not infer from the election in 1946. Next, I look at population growth (column 3). If some municipalities grew faster after World War II, for example because they received more refugees, this might have led

[29] I run this analysis with all available data, and do not limit the dataset to municipalities that had between 2,000 and 10,000 inhabitants in 1950. By doing so I want to ensure that I do not exclude any potential driver for protests due to a lack of precision of the estimates.

Table 1.2: Where Do People Protest?

	Location	Polit. Preferences	Pop. Growth	Industry	Housing Demand		Milit. Presence	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance Berlin	-0.084*							0.112
	(0.048)							(0.210)
Distance West Border	0.016							0.334*
	(0.048)							(0.188)
Distance County Capital	-0.075							0.200
	(0.076)							(0.245)
County Capital	0.260***							0.339***
	(0.054)							(0.089)
Share Votes SED		0.088***						-0.082
		(0.032)						(0.085)
Turnout 1946		-0.239**						-0.405
		(0.101)						(0.282)
Population Growth, 1946 to 1953			0.040					0.066
			(0.028)					(0.124)
Share in Industry				0.381***				0.518***
				(0.087)				(0.107)
Share in Construction 1950				0.492				0.563
				(0.316)				(0.394)
Share Households Searching Flat					0.337**			0.761**
					(0.131)			(0.331)
War Destruction						0.148**		
						(0.072)		
Barracked Police Units 1952							0.046	-0.213*
							(0.092)	(0.117)
Population 1953	✓	✓	✓	✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓	✓	✓
Mean Dep. Variable	0.099	0.096	0.099	0.123	0.116	0.153	0.100	0.151
Observations	5,170	4,814	4,915	1,914	3,326	742	5,170	1,174
Adj. R^2	0.319	0.310	0.311	0.290	0.299	0.388	0.309	0.312

Note Linear OLS regressions using all municipalities, independent of population. The dependent variable is a dummy variable indicating whether any protest activity occurred within a municipality in 1953. Distance to Berlin, Distance to the West Border (except Berlin) and Distance County Capital measure distance in 100km. County Capital is a dummy for county capitals. Share Votes SED and turnout 1946 are from the state legislature election in 1946. Share in Industry, Agriculture, and Construction 1950 is share of the working force working in the respective sector according to the 1950 job census. Share Households Searching Flats is from 1954. War destruction is destruction in percent in Saxony. Barracked Police Units 1952 is a dummy variable for the existence of barracked police units within a municipality. For data sources refer to Section 1.2.1. Standard errors clustered at the county level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

to discontent among the population. While population in 1953 predicts protests, protest municipalities did not have a different population growth path from 1946 to 1953. The role of different economic sectors is examined in column 4. Protest municipalities have a higher share of industrial workers (significant at the 1 percent level), but do not differ with respect to the share of people working in construction.

An important concern is whether municipalities with a higher demand for housing were more likely to protest. In columns 5 and 6, I examine to what extent housing demands are correlated with protests. I first use information on the share of people searching for a flat in 1954. A larger share of the population searching for a flat is correlated with more protests in 1953 at the 1 percent significance level (column 5). As a second measure of housing demand, I look at the share of war destruction in municipalities. This measure is only available for one region within the GDR, Saxony, and thus the number of observations drops in column 6. There is a positive relationship that is significant at the 1 percent level between war destruction and protests (column 6). Columns 5 and 6 thus provide evidence that demand for residential construction drove protest activity. However, the explanatory power of these coefficients as measured by the R^2 is comparable to that of the other potential explanatory variables.

Last, I look at the effect of having had some unit or establishment associated with the barracked police in 1952 in column 7. This would be problematic if the existence of the barracked police makes protests less likely, and after 1953 places that did not have any military yet received military troops after the introduction of the NPA. There is no statistical significant relationship between them. In column 8, I look at all potential correlates simultaneously (excluding war destruction in Saxony). Political preferences and distance to Berlin are no longer statistically significantly correlated with protests in 1953, whereas distance to the West border excluding Berlin and the existence of barracked police units negatively predicts protests.

The results of this section inform us on important control variables for the main

analysis of this paper. I will examine in turn whether county capitals,[30] the share of workers in industry, demand for housing, and the existence of barracked police units in 1952 drive the the effect of protests on housing and military presence.

1.3.2 Carrots: Residential Construction

To estimate the effect of protests on construction activity, I employ a difference-in-differences approach. This approach compares protest to non-protest municipalities, before and after protests, ceteris paribus. First, I estimate the following simple difference-in-differences model

$$Construction_{mt} = \beta_1 Protest_m + \beta_2 Post1952_t + \beta_3 Protest_m \times Post1952_t + \epsilon_{mt} \tag{1.2}$$

where $Construction_{mt}$ measures the number of new flats or buildings per 1,000 inhabitants in municipality m and year t. $Protest_m$ is a dummy variable that takes the value 1 if a protest occurred in municipality m in 1953. $Post1952_t$ is a dummy for all years after 1952. ϵ_{mt} is the error term. In additional specifications I also include year and municipality fixed effects.

Table 1.3 presents the results of the simple difference in difference estimator outlined in equation 1.2.[31] Over the entire period under consideration, an additional 0.833 flats per 1,000 inhabitants are built per year in a protest compared to a non-protest municipality (column 1). This does not seem to be explained by differences between protest and non-protest municipalities that existed prior to 1953: there is no difference in construction levels before 1953 (column 2). Protest municipalities have 0.878 (significant at the 1 percent level) additional flats per 1,000 inhabitants after 1953 (column 2). To calculate the accumulated difference until 1989, I use

[30] Especially during the early years of the GDR there were many county reforms, so that the effect of being a county capital is not captured by municipality fixed effects.

[31] Results when I include municipalities with fewer than 2,000 or more than 10,000 inhabitants in 1950 are presented in Table A.1 in the appendix.

Table 1.3: Carrots: Difference-in-Differences Estimators

	Flats per 1,000 inhabitants					
	(1)	(2)	(3)	(4)	(5)	(6)
Protest 1953	0.833***	0.095	0.662	0.095		
	(0.162)	(0.120)	(3.967)	(0.120)		
Post 1952		1.030***	78.003***	0.528***	1.030***	
		(0.078)	(2.621)	(0.079)	(0.079)	
Protest 1953		0.878***	35.313***	0.671***	0.878***	0.878***
× Post 1952		(0.223)	(5.611)	(0.170)	(0.225)	(0.223)
Year FEs						✓
Municip. FEs					✓	✓
Observations	41,932	41,932	1,906	1,906	41,932	41,932
Adj. R^2	0.003	0.009	0.432	0.064	0.062	0.076

Note Estimation results for equation (1.2) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variable is the number of newly constructed flats per 1,000 inhabitants per year and municipality. Protest 1953 is an indicator variable whether any protest activity occured in the municipality in 1953. Post 1952 is an indicator variable for all years after 1952. Columns (3) and (4) use the methodology suggested by Bertrand et al. (2004) and aggregate all pre and post treatment observations for each municipality. Standard errors are reported in parentheses. Standard errors are clustered at the municipality level in columns (1), (2), (5), and (6). Robust standard errors in columns (3) and (4). *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

the methodology suggested by Bertrand et al. (2004). When I aggregate all pre and post treatment observations for each municipality, protest municipalities have around 35 additional flats per 1,000 inhabitants after 1952 (column 3) or 0.671 flats per 1,000 inhabitants per year (column 4). To test whether these results are driven by municipality or time specific effects, I include municipality fixed effects (column 5), and municipality and time fixed effects (column 6) to the panel analysis. This does not affect results.[32]

[32]We expect coefficients in column 2, 5, and 6 to be identical, because the analysis is based on a full panel in which treatment occurs at the same point in time for all treated observations. In this case the coefficient of $Protest1953$ captures differences in construction of protest and non-protest municipalities before 1953, which is identical to what the average of municipality fixed effects across these groups captures. The coefficient of $Post1952$ includes the difference in construction after 1953 for municipalities that did not have a protest and the interaction $Protest1953 \times Post1952$ the difference in construction of protest municipalities after 1952 compared to before. The mean of all time fixed effects after 1952 is thus equivalent to the coefficient of $Post1952$.

1.3.3 Carrots: Robustness and Heterogenous Effects

The GDR was a socialist country; thus, the government was in control of most of residential construction. Nevertheless, some private construction existed. If the increase in construction in protest municipalities is an attempt of the government to buy off the opposition, we would expect that the effect only occurs for state-led construction. Therefore, I explore the difference between state and private construction, controlling for year and municipality fixed effects. State construction increases by 0.627 flats per 1,000 inhabitants (significant at 1 percent level) in protest municipalities after protests (Table 1.4, column 2). There is no statistically significant effect on private construction (column 3).

Regression model 1.2 does not take into account that there might be differences in protest and non-protest municipalities that have a different effect on construction over time. We have seen that the share of households looking for a flat is positively correlated with protests. Demand for additional housing in the 1950s might increase construction initially, but this effect might fade out over time. To account for this, I extend the model to a more generalized approach of the following form

$$\begin{aligned} Construction_{mt} = \beta Protest_m \times Post1952_t \quad & + \sum_{t=1946}^{1989} \gamma_t Controls_m \times Year_t \\ + \alpha_m + \alpha_t + \epsilon_{mt}, & \end{aligned} \tag{1.3}$$

where $Construction_{mt}$, $Protest_m$, $Post1952_t$ and ϵ_{mt} are defined as before. α_t and α_m represent time and municipality fixed effects respectively. If construction increases for all municipalities after 1952 or protest municipalities have higher construction levels over the whole period under consideration, this will be captured by the time and municipality fixed effects respectively. $Controls_m$ are a number of control variables, which I interact with dummies for all years. This allows these controls to have a different effect over time. The demand for housing in 1953, for

example, could have an impact on construction in the first few years after 1953, but not in later periods. Standard errors are clustered at the municipality level to control for heteroskedasticity and within-municipality correlation over time.

This approach will lead to causal estimates if the common trends assumption holds, i.e. if protest and non-protest municipalities would have developed in a parallel fashion in the absence of treatment. Figure 1.1 shows that before 1953 both protest and non-protest municipalities with 2,000 to 10,000 inhabitants had as good as identical construction levels per 1,000 inhabitants, and strengthens the credibility of this assumption.

Protests in 1953 were more likely in municipalities that had a larger need for additional housing (Table 1.2). If after 1953, the government began to address housing shortages, and provided housing in the municipalities that had the largest need for housing, protests would be correlated with an increase in housing, but not because the government targeted opposition municipalities. Including municipality fixed effects does not account for this, if the role of the need for housing changes over time. To test whether demand for housing explains the difference between protest and non-protest municipalities, I use two different measures for the local demand for housing. First, I control for the share of households at the municipality level looking for a flat shortly after protests took place, and second I control for war destruction on the municipality level.

Column 4 in Table 1.4 controls for the share of households in a municipality looking for a flat in January 1954 interacted with dummies for all years from 1947 to 1989. This does not affect the magnitude or significance of the coefficient of protest activity (0.691, 1 percent significance level). Next, I control for a second measure for the demand for housing: the share of buildings destroyed in a municipality in 1945. This data is only available for one area of the GDR, thus the number of observations drops. Nevertheless, protests are still positively associated with per capita construction.[33]

[33] When I estimate the baseline regression just for the municipalities for which I have information

Table 1.4: Carrots: Protests and Construction with Controls

	Flats per 1,000 inhabitants						
	All	State owned	Privately owned	All			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Protest 1953 × Post 1952	0.878***	0.627***	0.004	0.691***	0.748**	0.749**	0.864***
	(0.223)	(0.183)	(0.016)	(0.233)	(0.342)	(0.303)	(0.314)
People looking for flat 1954				✓			
War destruction					✓		
Construction Workers 1950						✓	
Industry Workers 1950							✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓
Observations	41,932	41,932	41,932	30,316	14,652	26,224	19,052
Adj. R^2	0.076	0.076	0.063	0.070	0.055	0.071	0.082

Note Estimation results for equation (1.2) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variable in columns (1) and (4) to (7) is the number of newly constructed flats per 1,000 inhabitants per year and municipality. The dependent variable in column (2) is the number of newly constructed flats owned by the state and the dependent variable in column (3) is the number of newly constructed flats that are privately owned. Protest 1953 is an indicator variable whether any protest activity occured in the municipality in 1953. Post 1952 is an indicator variable for all years after 1952. The data on war destruction used as a control variable in column (5) is only available for the state of Saxony. Standard errors clustered at the municipality level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

This shows that even though protests were more likely to occur in municipalities with more housing shortages, the need for additional housing does not drive the observed differences in construction after protests occurred. In fact, protest municipalities see a larger increase in construction than would be predicted based on their need for additional housing.

Another potential concern is that the self-proclaimed *workers' and farmers' state* targeted industrial workers for ideological reasons. Since industrial workers were more likely to protest, this could explain the differences in construction between protest and non-protest municipalities. We might similarly be worried about construction workers, although Table 1.2 does not show a significant relationship between the share of construction workers and protests. If this was the case, we would expect the effect of protests on construction to disappear once we control for the share of construction or industry workers in 1950 (the closest census year to 1953). Column 6 shows the effect of protests when controlling for the share of construction workers, and column 7 when controlling for the share of industry workers. The coefficient of interest remains nearly unchanged (0.749 and 0.864) and is significant at the 5 percent and 1 percent level respectively. This means, that there is no evidence suggesting that the increase in construction in protest municipalities was simply the result of targeting construction or industry workers.

Table 1.5 looks at heterogenous effects for different types of municipalities. In columns 1 and 2, I distinguish between county captials and all other municipalities. County capitals were likely to have a higher share of government employees or other people supporting the regime living in them. If the GDR government actually targeted their supporters who lived in hostile locations, we would expect to see a larger increase in construction in county capitals.[34] However, the results suggest

on war destruction, the coefficient $\hat{\beta}$ is 0.748, and significant at the 1 percent level. Controlling for the share of destroyed buildings thus has no effect on the estimated coefficient.

[34]This is a necessary but not sufficient condition. Construction in county capitals could also be larger for other reasons, for example if the government assigns more importance to the opposition in capital cities, or because it is easier to provide construction in capital cities.

Table 1.5: Carrots: Heterogeneous Effects

	Flats per 1,000 inhabitants					
	County Capital		Dist. West Germany		Border Municipality	
	yes	no	close	not close	yes	no
	(1)	(2)	(3)	(4)	(5)	(6)
Protest 1953	0.515	0.446**	1.075***	0.793***	1.205	0.846***
× Post 1952	(1.328)	(0.212)	(0.307)	(0.268)	(0.741)	(0.233)
Observations	1,348	40,584	28,600	27,500	4,620	37,312
Adj. R^2	0.115	0.063	0.079	0.070	0.055	0.078

Note Estimation results for equation (1.2) using only municipalities with 2,000 to 10,000 inhabitants in 1950 for different sample splits. The dependent variable is the number of newly constructed flats per 1,000 inhabitants per year and municipality. Protest 1953 is an indicator variable whether any protest activity occurred in the municipality in 1953. Post 1952 is an indicator variable for all years after 1952. In columns (1) and (2), the sample is split in county capitals and non-county capitals, respectively. In columns (3) and (4), the sample is split by the distance to West Germany where municipalities fewer than 50km away from West Germany are considered close and all others are considered not close. In columns (5) and (6), municipalities at the border of the GDR and those in the interior of the country are studied separately. Standard errors clustered at the municipality level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

that construction only increases in non-county capitals (columns 1 and 2). Thus the government increased construction in opposition areas that had lower levels of government supporters. Both protest municipalities that were close to West Germany or further away (less or more than 50 km away from West Germany) see an increase in construction levels after protests occurred. The increase is higher for municipalities close to West Germany (1.075, significant at 1 percent level) than those further away (0.793, significant at 1 percent level), but the difference between the coefficients is not statistically significant. Geographic location across the border plays an important role for construction in protest municipalities: border municipalities did not see an increase in construction if a protest occurred (column 5), whereas non-border municipalities did (column 6).[35]

[35]This pattern does not change before or after the construction of the Berlin Wall.

Table 1.6: Carrots: Quality of Flats

	Share of Flats with							
	Modern Heating	Kitchen	Toilet	Bathroom	>3 rooms	<40m^2	40 to 100m^2	>100m^2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Protest 1953 × Post 1952	0.003	0.001	-0.037***	-0.015*	-0.011	-0.001	0.056***	-0.055***
	(0.014)	(0.002)	(0.009)	(0.008)	(0.010)	(0.004)	(0.014)	(0.015)
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	34,599	34,599	34,599	34,599	34,599	34,599	34,599	34,599
Adj. R^2	0.153	0.010	0.203	0.150	0.057	0.019	0.161	0.167

Note Estimation results for equation (1.2) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variables is the share of newly constructed flats per year and municipality with a given quality attribute as indicated by the column header. Standard errors clustered at the municipality level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Quality of Housing

There are several ways to target a population with flats. Increasing the quantity of available flats is one way. Another way is to increase the quality of available housing. I use the information on the features of flats from the flat and building census to examine how protests affected the quality of housing. New flats in protest municipalities after 1952 did not have more amenities than in non-protest municipalities (table 1.6). There is no effect of protest activity on the share of flats with modern heating (column 1) or a kitchen (column 2). The share of flats with a toilet or a bathroom is lower than in non-protest municipalities (columns 3 and 4). In addition, the share of flats that were of medium size and suited for families increased (column 7), at the cost of the share of very large appartments (column 8). Lowering the quality of each individual unit allowed the government to provide more housing and target a larger number of people.

1.3.4 Sticks: Army Units

One way for the ruling elite to secure its power and to prevent further protests is to provide the opposition with higher living standards, but this policy also demonstrates to citizens that it can be beneficial to show opposition against the government. Another option is to use (or threaten the use of) state-led violence against the opposition. In this section, I examine to what extent the GDR government adopted this second option. I focus on the military, as one component of the security apparatus of the GDR. Throughout the existence of the GDR, the military was seen as a potential tool to stop internal turmoil. This is a very extreme form of a stick, which should be kept in mind when interpreting the results.

I use a difference-in-differences estimator to estimate the effect of protests on the establishment of military units. Even though the military was officially only established in 1956, there were para-military police units before that. I treat these police

units as military units in my empirical analysis. Thus, I am able to look at the existence of military units in municipalities from 1949 to 1989.

The model has the following form

$$\begin{aligned} MilitaryUnits_{mt} =& \beta_1 Protest_m + \beta_2 Protest_m \times Post1952_t \\ &+ \gamma ProtestsWithin10km_m \\ &+ ProtestsWithin10km_m \times Post1952_t \\ &+ \zeta NumberNPAtroopsWithin10km_{mt} \\ &+ \delta Controls_m + \alpha_d + \alpha_t + \epsilon_{mt} \end{aligned} \tag{1.4}$$

where $MilitaryUnits_{mt}$ measures the number of military units within municipality m in year t. $Protest_m$ is a dummy that takes the value 1 if any protest activity occurred in municipality m in 1953. Military units are mobile and can be moved, and I account for the spatial pattern of opposition behavior and military troops. $ProtestsWithin10km_m$ counts the number of protest municipalities within a 10 km distance of m and $NumberNPAtroopsWithin10km_{mt}$ the number of municipalities with NPA troops within a 10 km distance of m in year t. $Controls_{mt}$ are controls at the municipality level. I control for the natural logarithm of population in 1950, the share of workers in industry in 1946, share of votes for the SED in 1946 (and its interaction with a post 1952 dummy), distance to any Western border, a dummy for border municipalities, a dummy for Nazi military presence, distance to the next municipality with Nazi military, a dummy for the presence of Soviet military, distance to the next municipality with Soviet military and a dummy for county capitals. α_d are county fixed effects and α_t are time fixed effects. I also consider how results change when I control for municipality fixed effects instead of county fixed effects. In this case I drop all controls that do not vary over time. Standard errors are clustered at the municipality level in all regressions.

The coefficient of interest in regression 1.4, β_2, indicates how many more military

units exist in a protest municipality in a given year compared to a non-protest municipality conditional on all controls. γ captures to what extent the decision whether to place military in a given municipality is driven by the existence of opposition in close-by municipalities. ζ captures to what extent the number of military units is driven by the existence of military units in close-by municipalities.

Results are presented in Table 1.7.[36] If the location of protests was not anticipated by the GDR government, protests should not predict military location before 1953 conditional on municipality characteristics. Column (1) shows protests in 1953 and the number of protests in a 10 km radius are not correlated with the number of military troops before 1953 at any conventional significance level. Next, we turn to the location of military troops after protests occurred. If a municipality was involved in protests in 1953, it has 0.066 additional troops in each year (significant at 10 percent level). If there are more protests within a 10 km radius, this decreases the number of troops in municipality m after 1952, ceteris paribus. The effect of the number of troops within 10 km, on the other hand, does not have a statistically significant effect on the number of troops in m. Protest municipalities might have unobserved characteristics that explain why the number of military troops they host is larger, and to control for this, results in column 3 are conditional on municipality fixed effects. Protests are associated with 0.078 more military troops after 1952 in this specification (significant at 5 percent level). Again, the amount of protests within a 10km radius decreases the number of troops in municipality m, holding constant the number of military troops that exist in municipalities within a 10 km radius.

To ensure that these effects really capture a reaction to protest activity, we exploit that the rationale for establishing military troops differed over time in the GDR. Until 1961, the military was created as a potential weapon against internal opposition in case of turmoil. After the construction of the Berlin Wall in 1961, there

[36] Table A.2 presents results if I also include municipalities with fewer than 2,000 or more than 10,000 inhabitants in 1950.

Table 1.7: Sticks: Military Units

	Number of Military Units			
	before 1953	1949 – 1989	1949 – 1989	only after 1961
	(1)	(2)	(3)	(4)
Protest 1953	0.018	0.013		-0.001
	(0.013)	(0.021)		(0.019)
Protest 1953 ×		0.066*	0.078**	
Post 1952		(0.040)	(0.036)	
Number Protests	-0.002	0.009		-0.004
< 10km	(0.005)	(0.008)		(0.004)
Number Protests <		-0.009**	-0.011**	
10km × Post 1952		(0.004)	(0.004)	
Number NPA	-0.023*	-0.010	0.036*	-0.026**
< 10km	(0.012)	(0.026)	(0.021)	(0.012)
County FEs	✓	✓		✓
Municipality FEs			✓	
Year FEs	✓		✓	✓
Observations	3,546	36,490	36,818	24,108
Adj. R^2	0.240	0.249	0.740	0.390

Note Estimation results for equation (1.4) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variables are the number of military units per municipality and year that were established in the years indicated in the column title. Column (1) restricts attention to military establishments until 1952. Columns (2) and (3) incorporate all years from 1949 to 1989. Column (4) looks at the number of military units after 1961 that were established in municipalities that did not have any military presence prior to 1961. Controls in column 1, 2 and 4 are log of population 1950, the share of workers in industry in 1946, distance to any Western border, a dummy for border municipalities, a dummy for Nazi military presence, distance to the next municipality with Nazi military and a dummy for county capitals, whether there is Soviet military in a municipality, distance to the next municipality with Soviet military, share votes for the SED in 1946, and share votes for the SED interacted with post 1952 (column 2 and 3). In column 3 and 4 I control for population and the number of military units within a 10km radius. Column 1, 2 and 5 have county fixed effects, column 3 and 4 municipality fixed effects. All regressions control for year fixed effects. Standard errors clustered at the municipality level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

was a shift: the military began to focus on potential external threats. If military units were really targeted to protest municipalities, most units should have arrived between 1953 and 1961. To exclude any path dependency (municipalities that have troops before 1961, are likely to also have troops after 1961), column 4 regards only municipalities that did not have any military units until 1961. If these municipalities see the establishment of military units, this is very unlikely to be the case because of internal considerations. There is no longer a relationship between protest activity in 1953 and the number of military units (column 4). Thus, protest municipalities were only targeted by military troops in the time frame during which considerations about internal warfare dominated military location decisions, but not once considerations about external warfare play a role. This does not mean that after 1963 military troops became irrelevant as a tool to secure internal stability, as the stock of military troops that targeted to protest municipalities before 1963 continues to exist. Overall, the evidence in this section suggests that the GDR targeted protest municipalities after 1953 with military units to ensure stability of the regime.

1.3.5 Sticks: Stasi Presence

Next, I turn to a second measure of a stick: the secret police in the GDR, known as the Stasi. Unfortunately, no information on Stasi presence at the municipality level over time is available. However, I am able to examine the location of Stasi objects in protest and non-protest municipalities in 1989 with a novel dataset. Using this information, I estimate the following model

$$StasiPresence_m = \beta Protest_m + \gamma Controls_m + \alpha_d + \epsilon_m, \tag{1.5}$$

where $StasiPresence_m$ is a dummy indicating whether there has been any, any public or any secret Stasi presence in municipality m in 1989. $Protests_m$ is a dummy indicating whether any protest activity occured in m in 1953. $Controls$ are popu-

Table 1.8: Sticks: Stasi Presence in 1990

	Indicator for		
	Public Stasi presence	Secret Stasi presence	Stasi Flats
	(1)	(2)	(3)
Protest 1953	-0.005	0.082*	0.023
	(0.034)	(0.046)	(0.020)
Controls	✓	✓	✓
County FEs	✓	✓	✓
Mean Dep. Variable	0.126	0.288	0.048
Observations	811	811	811
Adj. R^2	0.524	0.455	0.472

Note Estimation results for equation (1.5) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variable in column 1 is a dummy that takes the value 1 if there are any public, official Stasi establishments in 1990. In column 2 it is a dummy indicating whether there were any disguised Stasi offices, and in column 3 a dummy indicating whether any flats for Stasi personnel in 1990. Protest 1953 is a dummy that takes value 1 if there has been any protest activity in 1953 in a municipality. Control variables in all regressions are population in 1989, distance to any border of the GDR, a dummy for county capitals, distance to the county capital, share of workers employed in industry in 1971 and share of workers employed in state administration in 1971, in addition to county fixed effects according to 1989 county borders. Standard errors clustered at the county level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

lation in 1989, distance to any border of the GDR, a dummy for county capitals, distance to the county capital, and shares of workers employed in industry as well as state administration in 1971. α_d are county fixed effects. Standard errors ϵ_m are clustered at the county level.

Results are presented in Table 1.8.[37] I interpret the regression as a linear probability model. The probability that any publicly visible Stasi (for example official office buildings that were known to belong to the Stasi) object exist in a protest municipality in 1989 does not differ between protest and non-protest municipalities (column 1). The probability to have disguised objects, such as safe houses or flats used for secret meetings, is around 8 percentage points (significant at 10 percent level) higher in protest municipalities (column 2). This suggests that the GDR government not only targeted opposition areas with military units that could potentially be used violently against the population, but also increased their control of the population

[37] Table A.3 presents results if I extend the sample to include all municipalities that I can match.

in other, less visible ways.

In column 3, I look at the number of flats for Stasi employees in a municipality. This serves as a further robustness check whether the increase in construction in protest municipalities happened to provide housing for state supporters, in this case Stasi employees. The probability that Stasi housing existed is not higher in protest municipalities in 1953.

1.3.6 Carrots and Sticks: Reactions in Housing to New Military Troops

The GDR targeted opposition municipalities with both carrots and sticks. In this section, I explore the relationship between construction and military presence by looking at the reaction of carrots to an increase in sticks. Troops decrease the probability of successful protests in municipalities, but at the same time they are very likely to decrease the popularity of the government. If the government used housing to alleviate the negative effect of stationing troops, construction would increase once new troops enter a municipality.

To examine whether introducing sticks in an area leads to an increase in carrots, I estimate the following regression

$$\begin{aligned} Construction_{mt} = \beta NewMilitaryUnits_{mt} + \gamma NewMilitaryUnits_{mt} \times Protest_m \\ + \delta Controls_{mt} + \alpha_m + \alpha_t + \epsilon_{mt} \end{aligned} \tag{1.6}$$

where $Construction_{mt}$ is the number of flats per 1,000 inhabitants in municipality m and year t, $NewMilitaryUnits$ is a dummy measuring whether additional military units are assigned to an area, $Protest$ is a dummy that indicates protest activity in 1953 and α_m and α_t are municipality and time fixed effects, respectively. In some additional regressions, I include whether there have been any additional military

units in the 5 years prior or after year t. Standard errors are clustered at the municipality level.

Results are presented in Table 1.9.[38] Column 1 establishes that municipalities that have any military units in a given year have higher levels of residential construction (1.583 additional flats per year, significant at 1 percent level).[39] This effect does not differ between protest and non-protest municipalities. Next, I examine the spatial pattern of this. Construction increases by 3.364 (significant at 10 percent level) if new military units join the municipality in that year, and by about 1.512 (significant at 5 percent level) additional flats per 1,000 inhabitants when any additional military units joined in the previous five years (column 2). This pattern does not differ between protest and non-protest municipalities (column 3). There is also an anticipation effect: if military units will be assigned in the next five years, construction already increases. This effect is smaller for protest municipalities (column 4). Column 5 provides evidence that this increase in construction is not simply construction of housing for military personnel. In the GDR, "normal" soldiers had to live on base, only higher ranked soldiers were allowed to live in flats outside of base (Kersten et al., 2011, p. 34). Thus, construction for soldiers would mostly consist of military barracks. I only consider flats that have a kitchen here, because military housing for troops would not have a kitchen in every flat. The coefficients remain nearly unchanged compared to column 3, indicating that this result is not driven by the construction of military barracks.

The existence of military troops will likely have impacted the popularity of the regime in protest and non-protest municipalities. The results of this section suggest that the government did in fact use carrots to alleviate the negative effect of sticks on their popularity. In doing so, they did not differentiate between protest and

[38] Table A.3 in the appendix presents results when including municipalities with fewer than 2,000 or more than 10,000 inhabitants in 1950.

[39] The existence of military troops does not explain the entire difference in construction between protest and non-protest municipalities. When comparing only municipalities that never had any military troops, protest municipalities still see higher construction levels after 1953.

Table 1.9: Reaction of Carrots to Sticks

	Flats per 1,000 Inhabitants				
	All				w/ kitchen
	(1)	(2)	(3)	(4)	(5)
Any military units	1.583***				
	(0.575)				
Any military units	1.438				
× Protest 1953	(1.172)				
New military units in t		3.364*	3.361*	3.307*	3.365*
		(1.799)	(1.814)	(1.771)	(1.814)
New military units in t			0.008	-0.069	0.007
× Protest 1953			(0.634)	(0.636)	(0.634)
New military units 5		1.512**	1.557**		1.560**
years before		(0.644)	(0.637)		(0.637)
New milit. units 5 years			-0.150		-0.151
before × Protest 1953			(0.245)		(0.245)
New military units				1.238*	
5 years after				(0.752)	
New milit. units 5 years				-0.764***	
after × Protest 1953				(0.247)	
Controls	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓
Municipality FEs	✓	✓	✓	✓	✓
Observations	41,932	41,932	41,932	41,932	41,932
Adj. R^2	0.078	0.077	0.077	0.077	0.077

Note Estimation results for equation (1.6) using only municipalities with 2,000 to 10,000 inhabitants in 1950. The dependent variable in columns (1) to (4) is the number of newly constructed flats per 1,000 inhabitants per municipality and year. In column (5), the dependent variable is the number of newly constructed flats with a kitchen per 1,000 inhabitants per municipality and year. All regressions control for a dummy for county capitals, distance to county capital, and population. Standard errors reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

non-protest municipalities.

1.4 Mechanisms: Carrots and Sticks or Bureaucrat Quality?

One explanation for the observed increase in residential construction and the presence of military troops in protest areas is that the GDR regime targeted the opposition with carrots and sticks. But, the same empirical pattern could also be explained by the behavior of local bureaucrats in areas with opposition (similar to the argument made in Lorentzen, 2013). It could either be the case that after 1953 there were increased incentives for local bureaucrats to perform well in areas with more protests. This would be the case if the government penalized bureaucrats that could not stop the opposition after 1953. Career concerns could then have led to a buying off of the opposition. Or it could be the case that places with more opposition had higher quality bureaucrats after 1953. If opposition areas were assigned bureaucrats of a higher quality, who were better able to deal with opposition and were more effective at providing public goods and station military troops, construction and military troops would increase in protest municipalities. To examine to what extent these two channels can explain the empirical findings, I look at term duration and quality measures of County Council Chairmen. I look at County Council Chairmen, because County Councils decided on where within a county to build.[40] These Councils were headed by Chairmen, who held most decision power (Bittorf, 2014, p. 102). To proxy the degree of opposition at the county level, I calculate the share of municipalities within a county that had protests in 1953. Results are presented in Table 1.10.

I first investigate the role of career concerns. There is archival evidence supporting the notion that the occurrence of protests could lead to a dismissal of local

[40]In addition, information on municipality level bureaucrats is not available.

bureaucrats. The chairman of the county Löbau, for example, was replaced after the Uprising of 1953, during which protests took place in his county. The following extract can be found in his personal file

"Colleague Hutschenreither was withdrawn as Chairman of the County Council due to his behavior in the days around the 17th of June 1953. Colleague H. was on a holiday [...] from beginning of June until beginning of July. As the leading figure of his county, he did not deem it necessary to obtain information about the situation in the county Löbau after the fascist provocation became known, not even via telephone."

If the government punished bureaucrats that could not prevent protests, we would expect the probability of replacement after the Uprising of 1953 in counties with more protest activity to be higher. First, I examine the "survival" of County Chairmen who were in power during protests in 1953 as a function of the share of protests municipalities within their county. The share of protest municipalities in 1953 is not associated with a statistically significant difference in survival rates of County Chairmen (column 1). Next, I estimate the probability that a Chairman is replaced in a given year, controlling for counties and year fixed effects. The outcome variable takes on the value 1 if the Chairman is replaced non-temporarily.[41] While the probability of replacement increased after 1953 in all counties, neither the share of protest municipalities nor the interaction of this share with a post 1953 dummy is significant (column 2). County Chairmen in counties with more protests were thus over the entire period of analysis not more likely to be replaced than their counterparts in counties with less protests. This would be very unlikely if the regime punished local officials for protests.

Another alternative explanation of how local bureaucrats affected construction and military presence is that more able bureaucrats were assigned to counties with more

[41] Temporary replacements took place if the Chairmen could not serve for a limited period of time, for example due to medical issues or because he earned additional degrees.

opposition. I proxy quality in different ways: first, I look at Chairmen who received the Patriotic Order of Merit (*Vaterländischer Verdienstorden*) for their work as Chairmen. This order was given to individuals and institutions in the GDR that had distinguished themselves with their actions, which included local bureaucrats that did exceptionally well in their position. Column 3 examines whether Chairmen in counties with a higher share of protest municipalities were more likely to ever receive an order of merit for their work as County Council. There is no statistically significant relationship between protests and orders of merit, suggesting that Chairmen of Counties with many protests were not of a higher quality. It is also not the case that people with better qualifications were chosen as Chairmen for the more difficult counties: Chairmen in counties with higher share of protests were not more likely to have had received an Order of Merit for their work as chairmen before their appointment before or after 1953 (column 4). There is also no evidence that they had more experience as chairmen by having been appointed in another counties before they were assigned to counties with a larger share of protest municipalities (column 5). We could also imagine that these Chairmen differed from each other in how good their understanding of the local situation was. I proxy this by the distance between place of birth to the county capital for those chairmen for which I could find information on their birthplace. There is no statistically significant effect of the share of protest municipalities on the distance in general or after 1953 (column 6).[42] Overall, the evidence presented in Table 1.10 does not support the argument that differences in construction were driven by differences in the quality of or incentives for local bureaucrats.

[42] All results are robust to excluding all city counties, which are cities that form their own county (see Table A.4 in the appendix).

Table 1.10: Channels: Chairmen of County Councils

	Duration of term		Characteristics of Chairmen			
	Survival Analysis	Replacement	Order of Merit Ever	Order of Merit Before	Appointment Before	Distance Place of Birth to County Capital
	(1)	(2)	(3)	(4)	(5)	(6)
Share Protests	-0.137	0.198	0.062	0.154	0.673	64.142
	(0.387)	(0.997)	(0.544)	(0.176)	(0.608)	(56.057)
Post 1953		0.286***	-0.114*	-0.007	-0.103	-0.680
		(0.050)	(0.060)	(0.006)	(0.079)	(2.090)
Share Protest × Post1953		0.016	0.029	0.015	0.131	-1.059
		(0.050)	(0.079)	(0.012)	(0.094)	(1.665)
County FEs		✓	✓	✓	✓	✓
Year FEs		✓				
Appointment Year FEs			✓	✓	✓	✓
Mean Dep. Var		0.131	0.061	0.003	0.074	179.053
Observations	380	4,233	677	677	677	131
Clusters	105	109	109	109	109	38
Adj. R^2		0.065	0.240	0.312	0.296	0.410

Note Column (1) presents the estimates of a Cox Hazard model that estimates the survival of County Chairmen that were in power during protests in 1953. *ShareProtests* is the share of municipalities within a county that had a protest in 1953. Column (2) shows the results of the regression $Replacement_{it} = \beta_0 + \beta_1 ShareProtests_i + \beta_2 Post1953_{it} + \beta_3 ShareProtests_i \times Post1953_{it} + \alpha_i + \alpha_t + \epsilon_{it}$ where $Replacement_{it}$ is a dummy that takes on value 1 if in a given year t the Chairmen of county i is replaced non-temporarily, $ShareProtests_i$ is the share of protest municipalities in 1953 in county i, $Post1953_{it}$ is a dummy that takes on 1 after 1953 and $alpha_i$ and $alpha_t$ are county and time fixed effects. For columns (3) to (6) the underlying model is $ChairmenCharacteristic_{ij} = \beta_0 + \beta_1 ShareProtests_i + \beta_2 Post1953_{ij} + \beta_3 ShareProtests_i \times Post1953_{ij} + \alpha_i + \alpha_{AppointmentYear} + \epsilon_{ij}$, where $Post1953_{ij}$ is a dummy indicating whether Chairmen j was appointed to county i after 1953. $\alpha_{AppointmentYear}$ are appointment fixed effects. The remaining variables are defined as before. In column (3) the dependent variable is a dummy measuring whether a Chairmen ever receives an Order of Merit for his work as a Chairmen in this or another county, in column (4) a dummy whether he received an Order of Merit for his work as Chairmen before he was appointed at the current county, in column (5) a dummy that takes on value 1 if the chairmen has served as Chairmen in another county before and in column (6) the geodetic distance in 100 km between the place of birth of the chairmen and the county capital. Standard errors clustered at the county level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

1.5 Conclusion

This paper studies how an autocratic regime targets its opposition with carrots and sticks using the example of the German Democratic Republic. In 1953, some municipalities signaled their opposition to the regime by engaging in protests. As a result, construction of flats per capita increased in these municipalities in comparison to municipalities without protests, and this increase is driven by state-led construction. This difference cannot be explained by differences in the demand for additional housing, the share of construction workers, or the share of industry workers. At the same time, protest municipalities were more likely to receive military units and hidden Stasi objects. Military units in the GDR were seen as measures that can be directed against opposing citizens and municipalities had no economic incentives to try to attract the National People's Army. When we look at the timing of construction and introducing military troops within municipalities, we see that after municipalities receive military troops, construction increases. This indicates that the GDR did take into account that increasing military presence has a negative effect on the popularity of the regime, and tried to counteract this.

This paper help us to better understand the trade-offs of autocratic regimes when using resource allocation and repression. I show that the government especially targeted the living standards of those areas that opposed them to prevent further protests, while at the same time building up a security apparatus that could have prevented or crushed future protests. Even though this paper only empirically considers one specific autocratic regime, other regimes that have a similarly hierarchical organization and aim to be popular, as is for example the case for China, might behave comparably.

A Appendix to Chapter 1

A.1 Figures

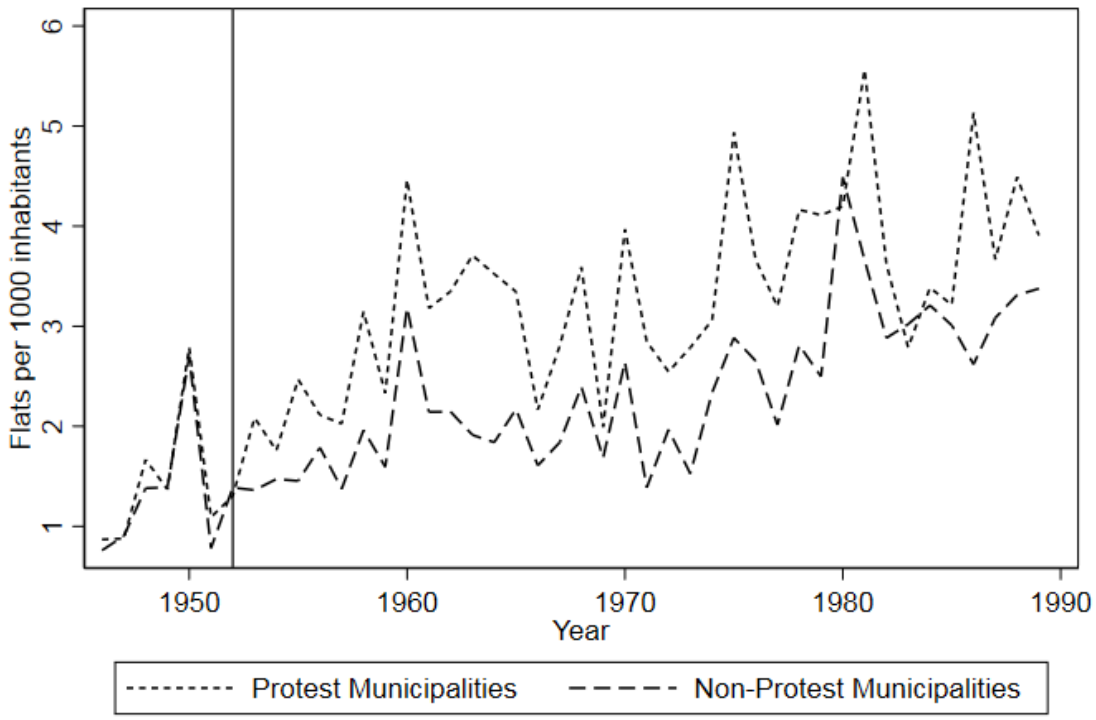


Figure A.1: Flat Construction per Capita at the Municipality Level - Extended Sample

Note: The figure shows average construction of flats per 1,000 inhabitants in protest and non-protest municipalities for all municipalities in 1950. The vertical line indicates the year 1952. Data sources: see text.

A.2 Tables

Table A.1: Carrots: Difference-in-Differences Estimators, Extended Sample

	Flats per 1000 Inhabitants					
	(1)	(2)	(3)	(4)	(5)	(6)
Protest	1.190***	-0.235*	-1.879	-0.235**		
1953	(0.120)	(0.128)	(2.362)	(0.091)		
Post 1952		1.741***	56.113***	1.414***	1.741***	1.741***
	(0.180)	(3.340)	(0.129)	(0.182)	(0.182)	
Protest '53	2.115***	2.036***	16.287***	2.036***	2.013***	0.892***
× Post '52	(0.022)	(0.033)	(0.740)	(0.029)	(0.033)	(0.034)
Year FEs						✓
Muni. FEs					✓	✓
Observations	220,264	220,264	10,012	10,012	220,264	220,264
Adj. R^2	0.003	0.0040.330	0.020	0.044	0.054	

Note Estimation results for equation (1.2) using all municipalities. The dependent variable is the number of newly constructed flats per 1,000 inhabitants per year and municipality. Protest 1953 is an indicator variable whether any protest activity occured in the municipality in 1953. Post 1952 is an indicator variable for all years after 1952. Columns (3) and (4) use the methodology suggested by Bertrand et al. (2004) and aggregate all pre and post treatment observations for each municipality. Standard errors are reported in parentheses. Standard errors are clustered at the municipality level in columns (1), (2), (5), and (6). Robust standard errors in columns (3) and (4). *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Table A.2: Sticks: Military Units, Extended Sample

	Number of Military Units			
	before 1953	1949 – 1989	1949 – 1989	only after 1961
	(1)	(2)	(3)	(4)
Protest 1953	0.019*	-0.073**		0.015
	(0.011)	(0.034)		(0.013)
Protest 1953		0.190***	0.225***	
× Post 1952		(0.048)	(0.052)	
Number Protests	-0.000	0.006		-0.000
< 10km	(0.002)	(0.005)		(0.002)
Number Protests <		-0.008	-0.009*	
10km × Post 1952		(0.005)	(0.005)	
Number NPA	-0.024***	-0.004	0.017**	-0.007
< 10km	(0.008)	(0.009)	(0.007)	(0.005)
Observations	17,936	184,031	192,044	123,032
Adj. R^2	0.188	0.440	0.809	0.058

Note Estimation results for equation (1.4) using all municipalities. The dependent variables are the number of military units per municipality and year that were established in the years indicated in the column title. Column (1) restricts attention to military establishments until 1952. Columns (2) and (3) incorporate all years from 1949 to 1989. Column (4) looks at the number of military units after 1961 that were established in municipalities that did not have any military presence prior to 1961. Controls in column 1, 2 and 4 are log of population 1950, the share of workers in industry in 1946, distance to any Western border, a dummy for border municipalities, a dummy for Nazi military presence, distance to the next municipality with Nazi military and a dummy for county capitals, whether there is Soviet military in a municipality, distance to the next municipality with Soviet military, share votes for the SED in 1946, and share votes for the SED interacted with post 1952 (column 2 and 3). In column 3 and 4 I control for population and the number of military units within a 10km radius. Column 1, 2 and 5 have county fixed effects, column 3 and 4 municipality fixed effects. All regressions control for year fixed effects. Standard errors clustered at the municipality level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Table A.3: Sticks: Stasi Presence in 1990, Extended Sample

	Indicator for		
	Public Stasi presence	Secret Stasi presence	Stasi Flats
	(1)	(2)	(3)
Protest 1953	0.028	0.091***	0.018
	(0.017)	(0.027)	(0.012)
Observations	4,212	4,212	4,212
Adj. R^2	0.376	0.276	0.170

Note Estimation results for equation (1.5) using all municipalities. The dependent variable in column 1 is a dummy that takes the value 1 if there are any public, official Stasi establishments in 1990. In column 2 it is a dummy indicating whether there were any disguised Stasi offices, and in column 3 a dummy indicating whether any flats for Stasi personnel in 1990. Protest 1953 is a dummy that takes value 1 if there has been any protest activity in 1953 in a municipality. Control variables in all regressions are population in 1989, distance to any border of the GDR, a dummy for county capitals, distance to the county capital, share of workers employed in industry in 1971 and share of workers employed in state administration in 1971, in addition to county fixed effects according to 1989 county borders. Standard errors clustered at the county level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Table A.4: Channels: Chairmen of County Councils, Excluding City Counties

	Duration of term		Characteristics of Chairmen			
	Survival Analysis	Replacement	Order of Merit Ever	Order of Merit Before	Appointment Before	Distance Place of Birth to County Capital
	(1)	(2)	(3)	(4)	(5)	(6)
Share Protests	-0.900	0.154	-0.122	0.140	0.933	8,022.888
	(1.344)	(1.070)	(0.539)	(0.155)	(0.568)	(10,253.060)
Post 1953		0.246***	-0.047	-0.004	-0.009	-237.680
		(0.047)	(0.052)	(0.006)	(0.047)	(230.954)
Share Protest × Post 1953		0.144	0.139	0.036	0.061	-989.586
		(0.196)	(0.202)	(0.043)	(0.152)	(2,354.441)
County FEs		✓	✓	✓	✓	✓
Year FEs		✓				
Appointment Year FEs			✓	✓	✓	✓
Mean Dep. Var		0.133	0.052	0.002	0.062	140.869
Observations	314	3,467	563	563	563	54
Clusters	87	91	91	91	91	21
Adj. R^2		0.065	0.264	0.344	0.301	0.866

Note This table estimates the same regressions as Table 1.10, but excludes all city counties from the analysis. Column (1) presents the estimates of a Cox Hazard model, that estimates the survival of county Chairmen that were in power during protests in 1953. *ShareProtests* is the share of municipalities within a county that had a protest in 1953. Column (2) shows the results of the regression $Replacement_{it} = \beta_0 + \beta_1 ShareProtests_i + \beta_2 Post1953_{it} + \beta_3 ShareProtests_i \times Post1953_{it} + \alpha_i + \alpha_t + \epsilon_{it}$ where $Replacement_{it}$ is a dummy that takes on value 1 if in a given year t the Chairmen of county i is replaced non-temporarily, $ShareProtests_i$ is the share of protest municipalities in 1953 in county i, $Post1953_{it}$ is a dummy that takes on 1 after 1953 and $alpha_i$ and $alpha_t$ are county and time fixed effects. For columns (3) to (6) the underlying model is $ChairmenCharacteristic_{ij} = \beta_0 + \beta_1 ShareProtests_i + \beta_2 Post1953_{ij} + \beta_3 ShareProtests_i \times Post1953_{ij} + \alpha_i + \alpha_{AppointmentYear} + \epsilon_{ij}$, where $Post1953_{ij}$ is a dummy indicating whether Chairmen j was apppointed to county i after 1953. $\alpha_{AppointmentYear}$ are appointment fixed effects. The remaining variables are defined as before. In column (3) the dependent variable is a dummy measuring whether a Chairmen ever receives an Order of Merit for his work as a Chairmen in this or another county, in column (4) a dummy whether he received an Order of Merit for his work as Chairmen before he was appointed at the current county, in column (5) a dummy that takes on value 1 if the Chairmen has served as Chairmen in another county before and in column (6) the geodetic distance between the place of birth of the Chairmen and the county capital. Standard errors clustered at the county level reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

2 | The Rise of Fiscal Capacity

The fiscal history of a people is above all an essential part of its general history.

Schumpeter (1918)

2.1 Introduction

The idea that "nothing is certain but death and taxes" would have surprised a person living in the Middle Ages. While death was of course certain, taxes were either non-existent or irregular. Today, on the other hand, taxation by sovereign states is taken for granted in most parts of the world. However, there are large differences in the ability of governments to levy and collect taxes as it for example becomes apparent when looking at the size of the shadow economy relative to overall GDP in different countries: in Switzerland the shadow economy is estimated to make up 7 percent of GDP, whereas in Georgia around 65 percent (Medina and Schneider, 2018). These differences have far-reaching consequences for the ability of states to provide government spending, social services, or growth-enhancing investments. It is thus not surprising that most accounts of the rise of the modern state, from Schumpeter (1918) and Weber (1919) to Tilly (1975) and Olson (1993), identify the emergence of taxation as the key hallmark and defining feature of the state.

Even though the introduction of permanent and stable fiscal administrations is one

This Chapter is joint work with Davide Cantoni and Matthias Weigand.

of the most striking changes in the relationship between states and their citizens, and there is no scarcity of theories explaining this crucial transition to modernity, we often lack empirical evidence on the circumstances and consequences of the establishment of fiscal capacity. Historical data on tax revenues or tax-collecting institutions are scarce, and in most cases do not extend back to the date of their introduction. In fact, the very emergence of modern statistics is clearly a consequence, not a precondition for the emergence of fiscal capacity (Woolf, 1989).[1]

In this paper, we use the rich history of the Holy Roman Empire to study a range of economic and institutional developments arguably linked to the introduction of modern fiscal administrations. Between the 16th and the 18th century, several component territories of the Holy Roman Empire introduced permanent offices, staffed by professionally trained individuals, in charge of raising and organizing revenues, and replacing personalized, local, or ad-hoc systems of taxation. These offices, mostly called "Chambers" (*Hofkammer* or *Rentkammer*), substantially increased the ability of sovereigns to raise taxes and thus to increase military power or provide public goods.[2] The outcomes we study concern both the periods *before* the introduction of fiscal institutions – thus allowing us to test theories relating to the emergence of fiscal capacity – and *after* their instruction – thus shedding light on the economic consequences of this momentous transition.

Our contribution is fourfold. First, we study and date the process of fiscal centralization for 24 territories of the Holy Roman Empire in the period between the 16th and the 18th century. The Holy Roman Empire, a loose confederation of hundreds of largely sovereign states of varying size, in an ideal setting in which to study the genesis and consequences of this institutional innovation. The rich available historiography provides evidence on where, and under which circumstances, states invested

[1] An alternative approach to investigate the origins of state formation and taxation is to study contemporary weakly institutionalized environments and their development in reaction to external shocks, as in Sánchez de la Sierra (2019).

[2] For most if not all territories of that time, raising sovereign debt was not a feasible path to increase state revenue, due to unsurmountable commitment problems (North and Weingast, 1989; Drelichman and Voth, 2014). Arguably, access to credit was easier for city states (Stasavage, 2011).

in their fiscal capacity. The detailed nature of our historical data allow us to observe all major territories – kingdoms, prince-bishoprics, dukedoms, margraviates, and principalities of all kinds – on a yearly level. We can thereby overcome selection (suvivorship) bias. In contrast to existing literature that focuses on few, ex-post successful territories such as Prussia or England, we consider all territories that existed and not just those that survived and eventually became large.[3] To understand the mechanisms that explain fiscal capacity and its effects, we need to understand which territories do not survive, and why.

Second, we map the cities in our dataset to a rich set of state formation and growth-related outcomes for the Holy Roman Empire. Our data are based on the rich city histories contained in the *Deutsches Städtebuch* (Keyser et al., 1939-2003), a detailed encyclopedia of all 2,394 places within Germany according to its borders in 1937 that were granted, at some point, city rights.[4] In particular, we can first measure a variety of outcomes related to historical territories, as our unit of observation: we observe dynastic mergers, break-ups, territorial expansions or losses as a consequence of wars or treaties. More generally, we can trace the existence, size, and shape of a territory through the number of cities a territory controls. Our second set of outcomes are all major construction events listed, for any given city in the *Deutsches Städtebuch*: these can be classified into different types of buildings (military, administrative, public infrastructures...) and are dated precisely at the city-year level.[5] Construction data give us an insight into administrative and military investments, and are ultimately a proxy for economic (urban) growth. We

[3]Tilly (1975) points out this fundamental selection problem: *"Most of the European efforts to build states failed. The enormous majority of the political units which were around to bid for autonomy and strength in 1500 disappeared in the next few centuries, smashed or absorbed by other states-in-the-making [...] [O]f the handful which survived or emerged into the nineteenth century as autonomous states, only a few operated effectively–regardless of what criterion of effectiveness we employ. The disproportionate distribution of success and failure puts us in the unpleasant situation of dealing with an experience in which most of the cases are negative, while only the positive cases are well-documented"* (p. 38-39).

[4]We use the anachronism "Germany" throughout the paper to refer to the German-speaking lands of the Holy Roman Empire. The cities included in the *Deutsches Städtebuch* encompass Germany in the borders of 1937.

[5]These data have been used previously in Cantoni et al. (2018).

can also measure a territory's exposure to war through the prism of the recorded history – occupations, sieges, destruction – of the cities that belong to it, again as reported in the *Deutsches Städtebuch*.

Third, we investigate the circumstances under which territories have adopted the institutions of fiscal centralization. We are able to confirm a number of hypotheses that have been put forward in the theoretical literature on the origins of fiscal capacity: territories are more likely to centralize when neighboring territories are centralized, and when they are exposed to a more bellicose environment.

Fourth, we look at the consequences of fiscal centralization. Observing territory-related outcomes, we find that centralized territories are more likely to survive, and grow more (expand the number of cities that they control) than non-centralized territories. Observing city-related outcomes, we find that cities belonging to centralized territories invest more in administrative and military construction. This is predicted by models of fiscal centralization. But we also find evidence that is contrary to one of the core assumptions in the formal literature on fiscal capacity. In theoretical frameworks citizens accept fiscal institutions and taxes because they expect revenues to be used for military investments, which will protect them. These investments are always modeled in the form of non-excludable public goods. Despite this, we find that increases in military investments only occur in the *core* of territories, and not in the *periphery*. Factually, some citizens were thus excluded from these investments, and they can thus not be thought of as public goods.

The rest of this Chapter is organized as follows. In section 2.2, we survey the existing literature on fiscal capacity, and derive testable hypotheses that we will empirically test. Section 2.3 discusses the historical background of fiscal capacity in the Holy Roman Empire, and section 2.4 explains our dataset. We first look at potential determinants of fiscal capacity in section 2.5, before turning to the effects of fiscal capacity in section 2.6. Section 2.7 concludes.

2.2 Literature and Hypotheses

There is a rich literature in the field of history trying to explain how taxation by a sovereign state went from a contested concept to an accepted fact of life in Europe. In this section, we discuss this literature, and derive hypotheses that we will empirically test.

2.2.1 Definitions

At the center of the literature discussed here are the concepts of state capacity and fiscal capacity. State capacity can broadly be defined as a "state's ability to implement a range of policies" (Besley and Persson, 2010, p.1), or "the *ability* of a state to collect taxes, enforce law and order, and provide public goods" (Johnson and Koyama, 2017, p. 2). Often, states are described according to their ability to enforce policies as either "weak" or "strong". In some cases the term state capacity does not refer to this broad definition, but to a concept mostly termed fiscal capacity in the economics literature.[6] Fiscal capacity "captures how much tax a government could potentially raise given the structure of the tax system and its available power of enforcement" (Besley and Persson, 2013, p. 52). Sufficient levels of fiscal capacity allow states to collect enough taxes to finance their policies (Johnson and Koyama, 2017, p. 2). Empirical work has used several observable measures or proxies for fiscal capacity. Dincecco (2009, p. 52) uses a binary variable measuring whether a country is fiscally centralized, where fiscal centralization "was completed the year that the national government began to secure revenues using a tax system with uniform tax rates throughout the country". Besley and Persson (2011) turn to per capita tax returns as a measure of the degree of fiscal capacity.

[6]The usage of the term state capacity to refer to what is mostly termed fiscal capacity in the economics literature is in line with how Tilly (1975) originally used the term. In this Chapter state capacity refers to the broad definition listed above, and we use the term fiscal capacity to talk about the capacity to tax in particular.

The literature on fiscal and state capacity is linked to the large literature on the importance of institutions for economic growth. Much influential work has defined institutions along the lines of what can be called "legal capacity". Legal capacity is the ability to support markets (Besley and Persson, 2010, p.1) and the ability to enforce rules across the entirety of the territory a government claims to rule (Johnson and Koyama, 2017, p. 2). North (1981), for example, measures the strength of states in early modern Europe by their ability to enforce property rights. Acemoglu et al. (2001) look at the risk of expropriation and repudiation of government contracts. Some work on fiscal capacity also studies interactions with legal capacity (e.g. in Besley and Persson, 2009).

2.2.2 Determinants of Fiscal Capacity

Attempts to explain the emergence of state capacity as a driver of state formation go back at least as far as Weber (1919). Weber defined the state in terms of its monopoly on legitimate violence over a defined geographical area. Many authors have tried to explain how states managed to establish this monopoly (see for example Brewer, 1990; Levi, 1989). One of the most influential arguments on state formation was made by Charles Tilly (1975), and can be summarized as "[w]ar made the state and the state made war" (Tilly, 1975, p. 42). Fighting war was costly, and thus rulers had to extract money from their population to finance these wars. As losing war was costly to everyone (e.g. if cities were looted), the population supported higher military investments and was willing to contribute to finance these. To organize the collection of taxes, fiscal bureaucracies were established. Once some states had built up such institutions, their neighboring states had to build up these fiscal capacity themselves to be able to compete with them or they would vanish (see for example Tilly, 1975; Bean, 1973).[7]

[7]For a more detailed summary of the argument that war led to fiscal capacity and this led to state foundation in Europe refer to Herbst (1990, p. 117-122).

This leads to our first hypothesis

Hypothesis 1: *Fiscal Centralization of neighboring territories increases the probability that a territory will fiscally centralize*

Tilly's argument that war drives fiscal centralization is at the core of many theoretical models on fiscal and state capacity in the economics literature.[8] Military spending as a reaction to a *threat of war* enters models as a public good. While economists argue that there could also be other public goods that drive the build-up of fiscal capacity, they usually discuss military investments as the only example of a common-interest public good (see for example Besley and Persson, 2009). Besley et al. (2013) extend these considerations to a dynamic framework looking at the evolution of fiscal capacity over time, again focussing on the demand for public goods, i.e. military investments, as a potential driver of fiscal centralization.[9] A higher demand for the public good translates into more investments into fiscal capacity.

The link between war and fiscal centralization has been refined in several ways. Some authors distinguish between external and internal wars (Besley and Persson, 2008, 2010). While the threat of external war generates a common demand for military investments across the entire population, threat of internal war generates conflicting interests among the population. These models predict that as a result, the threat of external war leads to higher levels and threat of civil war leads to lower levels of fiscal capacity. Ko et al. (2018) examine how the number and geographic origins of external threats affect state-building. In their model military strength decreases with distance to the capital city. They argue that, as European territories faced external threats from different directions, it was optimal to have several (smaller)

[8]Other potential determinants of fiscal capacity that are discussed in the literature are political stability, protection of minorities, dependence on natural resources as well as the distribution of economic and political power (Besley and Persson, 2009). Other authors have argued that distance mattered for building up institutions, and that thus smaller polities had an advantage when travel was still slow and costly (Stasavage, 2010).

[9]They also regard cohesiveness of institutions and stability of institutions as additional drivers.

states instead of a centralized European empire. This allowed each state to tax its population and use tax incomes to provide military investments in a capital that was as close as possible to the external threat it was facing. Queralt (2018) distinguishes how war was financed in the modern period (from 1817 to 1913): while tax-financed wars have a positive impact on fiscal capacity, loan-financed wars do not always lead to an increase in fiscal capacity. Karaman and Pamuk (2013) look at twelve European territories from the 16th to the 18th century to test the role of warfare, economic structure, and political regime for the development of fiscal and state capacity. They find that war and modern urbanized economic structures increase tax revenues. Political regimes interact with these factors as representative regimes were more successful at building state capacity in urbanized economies, whereas authoritarian regimes fared better in less urbanized settings.

Gennaioli and Voth (2015) examine the changing role of money for military success over time. Available financial resources only begin to matter for war success after the so-called Military Revolution (after Roberts, 1956), which introduced new (costly) military technologies.[10] The authors derive a model in which threat of war only leads to increases in state capacity when financial resources matter for the probability of winning war.[11]

[10] While some key innovations such as gunpowder and the star-shaped fortification spread earlier (in the 14th and 15th centuries, respectively), Gennaioli and Voth (2015) determine the year 1650 as the turning point after which differences in military expenditures start becoming crucial in determining the outcomes of a battle.

[11] Another result of the model in the paper is that only cohesive countries will invest in state capacity, as building up state capacity is costly for divided countries. Thus cohesive countries survive, while divided countries disappear.

In our second hypothesis, we test the general argument on war and fiscal capacity, and the refined argument made by Gennaioli and Voth (2015)

Hypothesis 2a: *Threat of war increases the probability that a territory will fiscally centralize*

Hypothesis 2b: *Threat of war only increases the probability that a territory will fiscally centralize* ***after the Military Revolution*** *(i.e. after 1650)*

2.2.3 Effects of Fiscal Capacity

In the models discussed here, more investments into fiscal capacacity generate higher levels of fiscal capacity. These investments translate into higher revenues for the state, which are used to finance war, or – more generally – to finance investments into public goods (for example in Besley and Persson, 2009). In addition, states that are able to extract some part of their citizens' incomes have incentives to foster economic growth by investing into growth enhancing policies (in the spirit of Olson's (1993) *stationary bandit*). This mechanism is captured both in the theoretical (Besley and Persson, 2008) and in the empirical (Dincecco, 2015; Dincecco and Katz, 2016; Dincecco and Prado, 2012) literature on fiscal capacity. To study the effect of fiscal capacity on economic growth empirically, these papers use the incidence of war or war casualties in the past as an instrument for fiscal capacity. Conflict in the past is associated with higher economic growth/more wealth today. This motivates our third set of Hypotheses

Hypothesis 3a: *Fiscally centralized territories invest more in military and administration*

Hypothesis 3b: *Fiscally centralized territories have higher economic growth*

In theoretical frameworks higher spending on war increases the probability of win-

ning war, which means that rulers are able to remain in power (Besley and Persson, 2010); winning wars is thus predicted to be associated with the survival of territories in our setting. Gennaioli and Voth (2015) predict that fiscal capacity leads to a higher probability to win wars after the Military Revolution. They show empirically that large European states with more tax revenues were more likely to win wars after the Military Revolution. To circumvent the problem that war is endogenous, they instrument the threat of war for countries by wars in neighboring countries. In Alesina and Spolaore (2005), there is a positive relationship between wars and the size of states, because a more bellicose environment is associated with higher incentives to form larger states. We will study the following hypotheses

Hypothesis 4a: *Fiscally centralized states are less likely to vanish*
Hypothesis 4b: *Fiscally centralized states grow more in size*
Hypothesis 4c: *Fiscally centralized states are less likely to vanish and grow more, because they are more successful at war*

Other effects of war that are discussed in the literature include the build-up of national identity (Alesina et al., 2017; Mazumder, 2018), urbanization (Dincecco and Onorato, 2016), or the welfare state in general (Dincecco, 2015).

2.2.4 Beyond Explaining European History

The argument that historic warfare led to investments in state capacity has also been studied outside the context of nation building in Europe. Herbst (1990, 2014) aims to explain why African states are so weak by comparing the history of European state formation to (the absence) of African state formation before colonialism. In Europe, high population densities and scarce land meant that rulers competed over land, and thus had to finance conflicts over territories. In Africa, on the other hand, land was abundant and people scarce. The absence of external threats implied that there

were no incentives for rulers to build up fiscal capacity to finance wars. According to Herbst, these historical developments explain weak state power of African states today. Depetris-Chauvin (2016) also suggests that there is a link between historical experiences and current state effectiveness in Africa, as he argues that regions in Africa with a longer history of statehood are better able to preserve order and experience less conflict today. Dincecco et al. (2018) study the link between conflict and fiscal capacity in the African context and find that conflict in Africa is associated with higher fiscal capacity, but at the same time more civil war. They suggest that a lack of social trust might explain why more historical warfare is not associated with more favorable current outcomes in Africa in contrast to the European case.

Ko et al. (2018) explain the political centralization of China as a reaction to having an external threat only from one direction. In this case, having a government with a large tax base that can provide military investments at the affected border is the rational strategy. The authors present some evidence that during times in which there was a larger external threat, China was more likely to be politically unified.

State capacity and fiscal capacity are also studied outside of the literature explaining the rise of effective states. State and in particular fiscal capacity are key concepts in development economics as states with low state and fiscal capacity lack economic growth. Missing fiscal capacity is said to prohibit developing nations to raise revenues that they could use to govern effectively (Migdal, 1988), and missing tax revenues in turn explain low overall economic development (Bird et al., 2008).

2.3 Historical Background

In this paper, we study the rise of fiscal capacity in the Holy Roman Empire. The Holy Roman Empire (HRE) was an assembly of many different territories and existed from the 9th until the 19th century in Central Europe. It was headed by an elected Emperor. This Emperor was never able to fully unite the different territories and

to create a centralized government, instead territories were ruled by local princes, who decided on the administrative and fiscal organization of their lands.

In the beginning of the Middle Ages, there was only very little fiscal capacity in the territories of the HRE. Local offices (so-called *Ämter*) were in charge of revenue collection and spending of princes. Revenues did not stem from taxes yet, instead they came from sources that were accessible even without sophisticated levels of fiscal capacity: from estates, demesnes and forests, income from court fees, as well as mine, salt and coin monopolies, and tariffs. The introduction of taxes began in the 13th century (Klein, 1974, p. 12-14). Initially, dues and taxes were mostly paid in-kind, because the medieval economy was not monetarized yet. If the prince, who at this point did not have a steady court location, came to a region, in-kind payments were used to provide for the prince and his court. This local collection of non-monetary revenues continued until approximately the middle of the 15th century. After that central cash offices (*Kassen*) were introduced.

Over time, princes needed more revenues, for example to finance an increasing number of feuds between territories or to pay for the increasing costs of holding court. Naturally, this raised their incentives to introduce some form of fiscal administration. As a first step in many territories, fiscal matters were assigned to one person, the so-called *Landrentmeister*.[12] The *Landrentmeister* was in charge of collecting and organizing revenues, but there was no formal institution which he presided over. Initially, most of these *Landrentmeister* were of noble origin, however, over time there was a shift towards commoners with a professional and university-trained background. In larger territories he was supported by one or two writers. One of the first jobs of this basic, un-institutionalized financial administration was the inspection and auditing of local offices.

The continuing centralization of territories' administrations in the 15th and 16th

[12]In the lower Rhine areas the *Landrentmeister* existed already in the 14th century, but in general this position only arose from the mid-15th century onward.

century also affected the organization of fiscal administrations. During this time, Court Councils (*Hofräte*), that is informal groups of confidants, who helped rulers to make decisions, first emerged. Initially, these councils were concerned with all administrative areas, but quickly certain fields were handed to newly established, specialized councils. This was the beginning of the establishment of the first Finance Councils, mostly called Chambers (*Hofkammern* or *Rentkammern*) (Klein, 1974, p. 16), which marked the introduction of centralized fiscal institutions, that continued to evolve over the next centuries. The Chamber was in charge of all domains and their revenues, as well as revenues from dues, tariffs, and taxes, and used these revenues to make payments in the name of the prince. Eventually, the Chamber took on the role of an economic institution which tried to secure old and promote new revenue sources. This institutional and geographical centralization of fiscal administration was only possible because taxes were now paid in cash and princes had a constant residence. In general, smaller territories had smaller chambers (Wakefield, 2009, p. 16-17); larger territories had several Financial Councilors, smaller territories often just had one.

The exact form and time of introduction of Chambers varied across territories. The first territory in our dataset to centralize was Württemberg in 1521 while it was controlled by Habsburg. The concept of centralizing the fiscal administration in a Chamber was already introduced in some areas of the Habsburg Empire at that time. After occupation ended, Württemberg continued to have an independent Chamber, which was the central cash office, and in charge of the prince's domains and local offices. In Bavaria, a Chamber was introduced in 1550. Before this, an assigned official administered all revenues. There was no special office in charge of finances yet. After 1550 all spending and all financial matters had to go through the Chamber. In Hesse, the financial administration was also already executed by assigned officials before the introduction of a Chamber. Some of these officials were also responsible for other matters. The actual establishment of a Chamber was in

1558, when the first Chamber order (*Kammerordnung*) was adopted. It specified that the Chamber was in charge of managing the treasure, debts, spending of the court on wages and food, as well as all princely properties including the forests. In 1567, Hesse split up into two lineages, Hesse-Kassel and Hesse-Darmstadt. Hesse-Kassel continued the Chamber, whereas in Hesse-Darmstadt there is only evidence for a Chamber from the 1590s onwards.

Until the first half of the 18th century, a collegial organization of Chambers became the norm, even though the exact organizational set-up differed across territories. Different departments run by different Councilors within the Chamber were established. The first areas where this happened were trade and crafts. The aim was to establish departments in charge of individual aspects of governing. Habsburg and Prussia faced even more challenges here, because they also had to combine the (fiscal) administration of their different territories while establishing territorial unity. Over time, these newly established departments suppressed the formerly all-encompassing role of the Chambers (compare to Willoweit, 1982, p. 330-347).

The dissolution of the Holy Roman Empire in 1806 also marks a turning point in the fiscal history of German territories. The number of territories fell rapidly and in 1815 39 German states formed the German Confederation. Each territory established its own finance ministry, which organized the relevant subordinated departments. There were still large differences in the fiscal capacity of different states. For example, the introduction of the universal income tax, a hallmark of sophisticated levels of fiscal capacity, ranged between 1834 in Hanover and 1913 in Mecklenburg-Neustrelitz and Mecklenburg-Schwerin. The Principality of Waldeck never introduced a universal income tax until it ceased to exist in 1918.

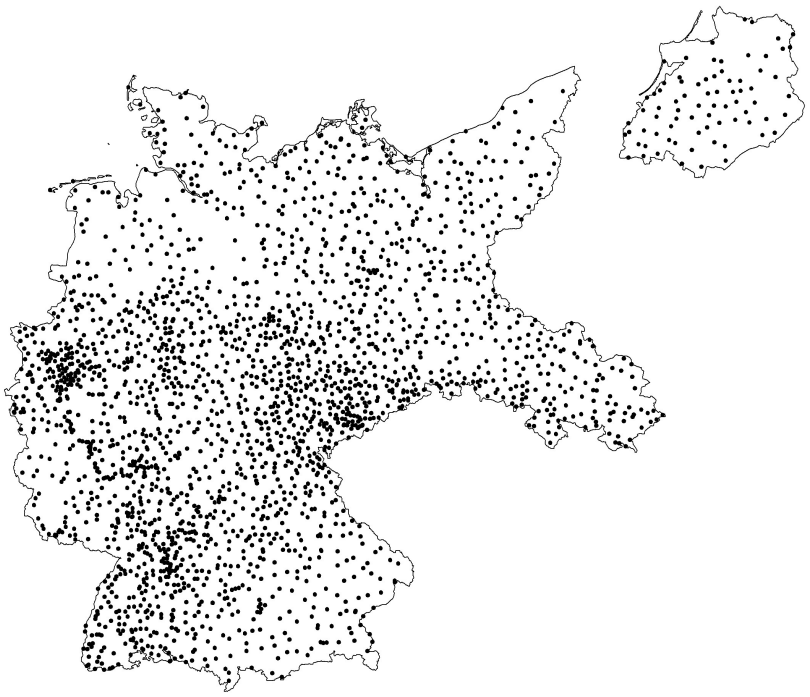


Figure 2.1: Cities in our Dataset within 1937 Borders
Note Each dot represents the location of one city in our dataset within the borders of Germany of 1937. Data sources: see text

2.4 Data

Our analysis is based on a novel panel of 2,394 cities and their corresponding 707 territories from 1400 to 1800.[13] Cities are taken from the *Deutsches Städtebuch* (Keyser et al., 1939-2003), an encyclopedic book containing all cities that existed in Germany in 1937. The location of these cities is mapped in Figure 2.1. We complement this with information on the history of administrative entities based on an encyclopedia on German territories (Köbler, 2007), a website listing the majority of German and European noble families, a large number of historical maps, and

[13]We exclude the territory Bohemia from our analysis. There are two reasons why we do so: first, due to the location of the Bohemian lands, our dataset only captures a fraction of the kingdom, which extended far into the east. Thus, we cannot clearly infer about its size and the changes thereof. Second, its ruling family, the Habsburg Dynasty, aggravates the issue further, as their lands in Austria, Hungary, and Spain are also not covered in the data. No other territorial entity or ruling family is so clearly peripheral in our data. We note that no results change fundamentally with Bohemia included into the analysis (results available on request).

sources on individual families and territories.[14]

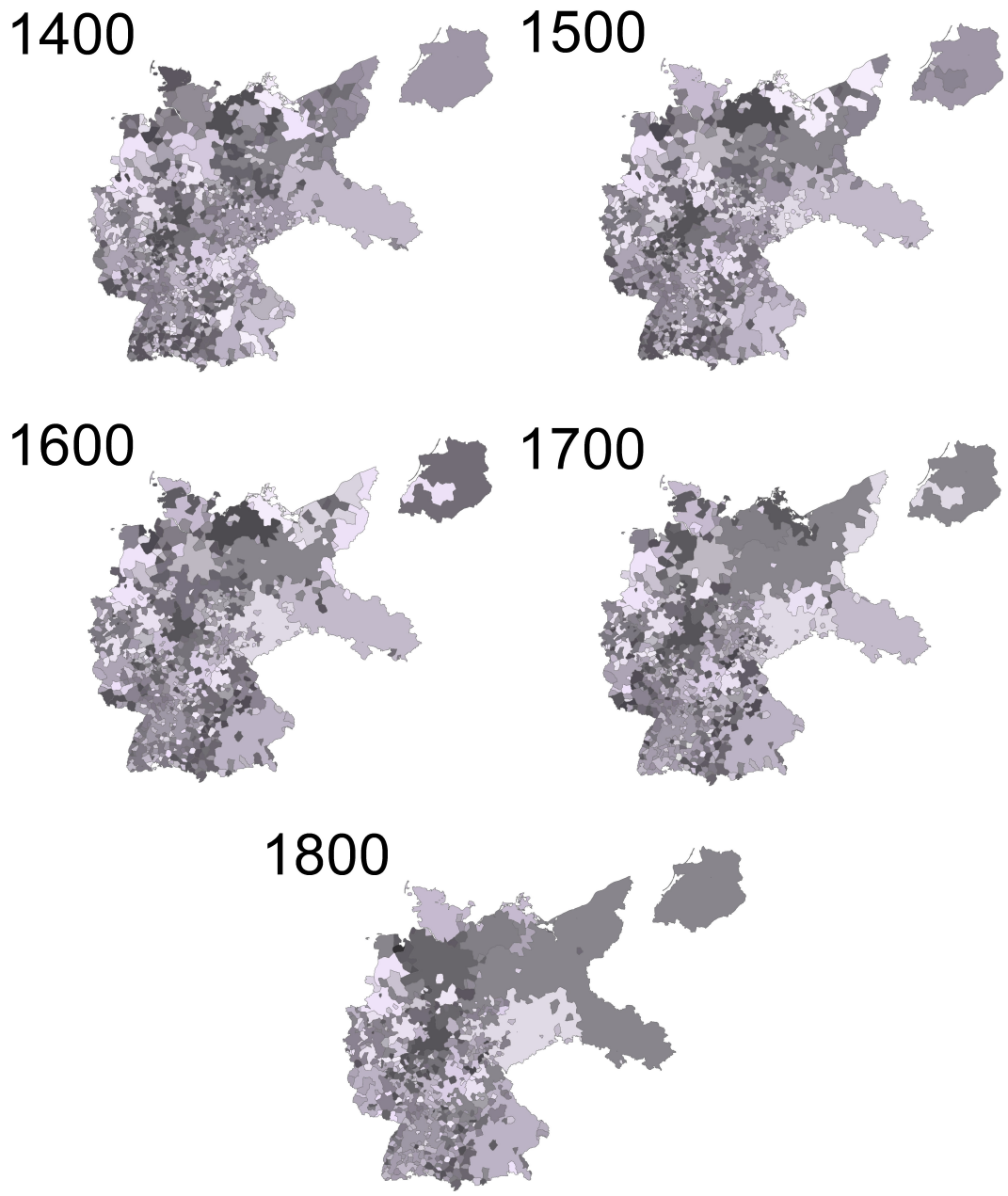


Figure 2.2: Territories in the Holy Roman Empire

Note The figure shows territorial borders for the years 1400, 1500, 1600, 1700, and 1800 within German borders of 1937. To map territories, we first calculate a Thiessen polygon around each city, taking into account terrain ruggedness. Next, we aggregate all cities and "their" surrounding Thiessen polygons that belong to the same territory in a given year. Data sources: see text

Each city is assigned to a territory in each year. The exact borders of territories in the Holy Roman Empire are mostly unclear. To obtain an idea of the geographical

[14]For more information on the coding of the territories refer to Appendix B.1.

extent of territories, we thus estimate each territory's likely area of influence. We proxy borders by drawing polygons around each city, and assigning each point in the Holy Roman Empire to its closest city, taking into account territory ruggedness. The assignment is decided upon by modified Thiessen polygons (Voronoi partitions).

Figure 2.2 shows a snapshot of the size and location of territories created in this way for each century from 1400 to 1800. The number of territories in our dataset declines from 367 to 196 between 1400 and 1800. At the same time the surviving territories grew in size; while the average territory that we observe in 1400 had 85 cities, it had 245 in 1800 (the overall number of cities that we observe in each year is constant).[15] The Herfindahl index, which measures the concentration of cities across territories, increased by a factor of 3 between 1400 and 1800 (see Figure B.1 in the appendix). In other words, half of the territories were unsuccessful and disappeared, while the successfully remaining territories tripled their number of cities. By 1800 small territories still existed, albeit in much lower numbers than 400 years earlier.

For each city, the *Städtebuch* contains information on significant construction events at the city level. We group construction events into different categories: administrative (courthouses, town halls, ...), military (castles, arsenals, fortifications, ...), and economic construction (storages, factories, manufacturies, ...). There are around 16,000 construction events for which we know the year of construction. We supplement this by data on modern, star-shaped fortifications based on Schütte (1984), Klöffer (2004), as well as Wikipedia, and google maps statellite images. These fortifications developed during the Military Revolution and are one example of a costly military technology that territories can adopt.

The *Städtebuch* also contains attacks at the city level. Unfortunately, it is not known from which territories these attacks originated. It could very well be the case, that attacks on cities are part of a broader set of war activities, in which the territory under consideration is attacked and at the same time also attacks other territories.

[15] Median territory size increased from 32 to 90 in the same time span.

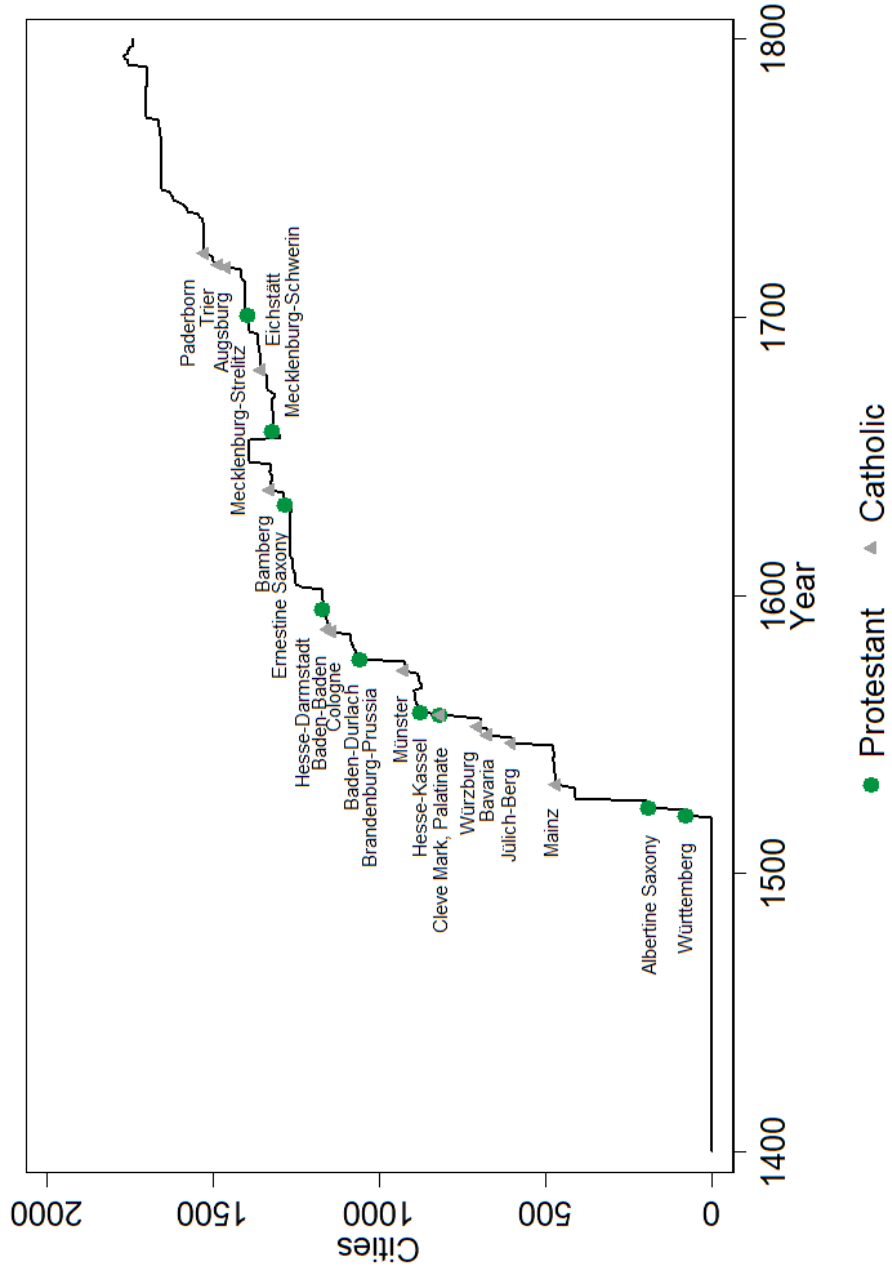


Figure 2.3: Number of Centralized Cities and Dates of Centralization

Note The figure shows the number of cities that belong to centralized territories in each year. Dots represent the year of the introduction of a Chamber in a Protestant territory, triangles the introduction of a Chamber in a Catholic territory. Data sources: see text and Appendix B.1

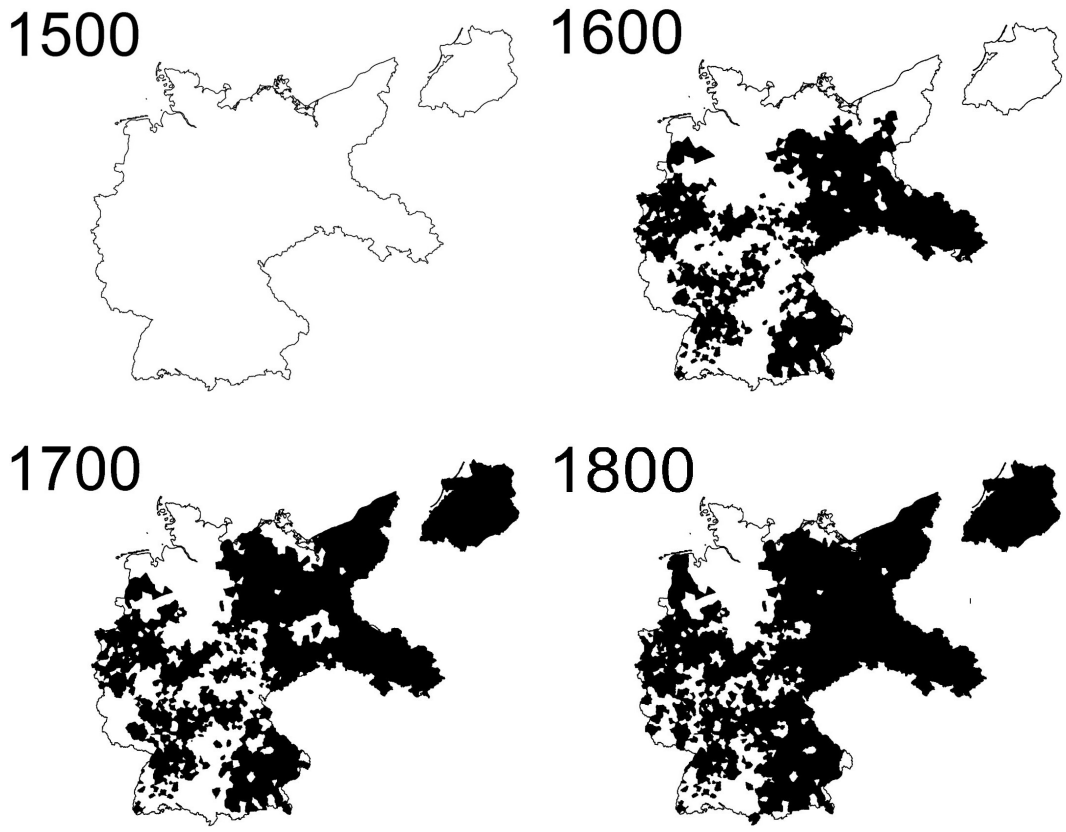


Figure 2.4: Centralized Territories in the Holy Roman Empire

Note The black area represents the area covered by centralized territories in the years 1500, 1600, 1700, and 1800. Data sources: see text

If this is the case, attacks can be used as a proxy for war. In Table B.2 in the Appendix we provide evidence that attacks on cities of territories occur at the same time as new cities enter these territories. We thus interpret attacks as a proxy for overall military conflict in the remainder of the paper.

To measure fiscal centralization, we collected a novel dataset on the timing of fiscal centralization in the territories of the Holy Roman Empire. The dataset builds on a comprehensive handbook on the history of administration in Germany (Jeserich et al., 1983). We supplemented this with information from a large number of publications on fiscal and regional histories. We find evidence for fiscal centralization in 24 territories, which are presented in Figure 2.3.[16] There is considerable variation in the timing of fiscal centralization: Württemberg and Albertine Saxony were the

[16] Appendix B.1 summarizes the dates of fiscal centralization and type of institution that was introduced for each territory, and lists selected sources.

first territories to centralize at the beginning of the 16th century, whereas the Duchy of Mecklenburg-Strelitz, Prince-Bishopric of Augsburg, the Electorate of Trier, and the Prince-Bishopric of Paderborn only centralize at the beginning of the 18th century. The number of cities within centralized territories increases as the number of centralized territories increases. When Württemberg centralized in 1521, 81 cities belonged to a centralized territory, by 1600 261 cities, and in 1723, when the last territory in our dataset, the Prince-Bishopric of Paderborn, introduced a *Chamber*, there were 1,526 cities in centralized territories. Figure 2.4 illustrates how the area covered by centralized territories increases between 1500 and 1800. In 1500 there were no centralized territories yet, but by 1600 already large parts of the area of Germany in 1937 belonged to centralized territories. By 1700 and 1800 this area had increased even further, incorporating even more space especially in the Northern parts.[17]

2.5 Determinants of Fiscal Centralization

According to the literature on fiscal capacity, fiscal centralization is driven by centralization of other territories (hypothesis 1) and an environment of bellicosity (hypotheses 2a and 2b). Centralized neighbors and threat of war increase the need of territories for revenues to finance potential conflict. To meet these demands,

[17]In our main analysis we assume that territories for which we have not found any evidence for the existence of a chamber, were not centralized. It is possible that by doing so we are treating some territories which in fact were centralized as non-centralized. If this wrong assignment is random, our estimates would be smaller than the real difference between centralized and non-centralized territories. It would be more difficult to find statistically significant differences between the two groups. However, it could also be the case that we were more likely to find evidence on centralization for larger territories. In this case our empirical analysis would lead to larger estimates than the real difference that is due to centralization. There are several reasons why we believe that this is not the case here. First, the historical literature clearly states that centralization in the HRE started in Württemberg in 1521. Thus, it is very unlikely that we missed anything before 1521. Second, not all of our treated territories are big (for example Munster or Trier). Third, we have found evidence for some territories that have not been treated – and these are in fact territories that ceased to exist. And last, there is a large regional history on German territories, which also focuses on small territories. In addition, we address many of these issues in our empirical analysis by controlling for territory fixed effects and thus only looking at the variation of outcomes within a territory after centralization was introduced.

territories centralize. To explore this we estimate the following model

$$\begin{aligned} 100 \times Centralization_{jt} = & \gamma_1 CentralizedNeighbors_{jt} \\ & + \gamma_2 CentralizedNeighbors_{jt} \times Post1650_t \\ & + \beta_1 ThreatWar_{jt} + \beta_2 ThreatWar_{jt} \times Post1650_t \\ & + \zeta_1 Controls_{jt-1} + \zeta_1 Controls_{jt-1} \times Post1650_t \\ & + \alpha_t + \epsilon_{it} \end{aligned} \tag{2.1}$$

where $Centralization_{jt}$ is a dummy that takes value 1 if territory j centralizes in year t, and zero otherwise. For readability we multiply this dummy with 100. Once a territory centralizes, it does not lose this status. Since we are interested in territories centralizing instead of staying centralized, we drop territories from the sample after they centralize. $CentralizedNeighbors_{jt}$ is the natural logarithm of one plus the number of cities from centralized territories within a 50 km radius of j in t. Positive values of γ_1 indicate that centralization of neighbors is related to the centralization of a territory, as claimed in hypothesis 1. To capture the threat of war, $ThreatWar$ is measured in two ways: first, we use a dummy measuring whether any cities in territory j were attacked in the current period or one or two decades before, which proxies actual war activities. Second, we also turn to the stock of military construction by other territories within a 50 km radius of territory j as a measure for the risk of a potential war, capturing how militarily exposed j is. The coefficient β_1 tests hypothesis 2a, whether threat of war is associated with an increased probability to centralize. $CentralizedNeighbors$ and $ThreatWar$ are both interacted with *Post1650*, a dummy for all decades after 1650. We follow Gennaioli and Voth (2015) who also use a post 1650 dummy to capture the onset of the Military Revolution, after which financial resources arguably became more important to win wars. If hypothesis 2b is true, and threat of war after the Military Revolution increases the probability that a territory centralizes, β_2 will be larger than zero. γ_2 indicates whether centralization of neighbors is associated with a

different probability of centralization of a territory after the Military Revolution.

$Controls_{jt-1}$ comprise, at the current state, only the lagged natural logarithm of the number of cities in territory j. We also interact this with $Post1650$ to allow the number of cities to have a different effect before and after the onset of the Military Revolution. α_t are decade fixed effects. By including time fixed effects we are controlling for any time specific factors that are constant over time. This is important in our setting if these factors are correlated with threat of war or centralization of neighbors and have an effect on centralization themselves. Standard errors are clustered at the territory level.

We use a linear probability model here to calculate the probability of centralization, following the approach in Drago et al. (2016). Using a linear probability model in this setting, as opposed to a probit, logit or Cox duration model, allows us to include a wider range of fix effects.

2.5.1 Results

Results of Regression 2.1 are presented in Table 2.1. Before the onset of the Military Revolution, each additional city from a centralized territory within a 50 km radius is associated with a 1.4 percent increase in the probability of centralizing. After the Military Revolution, there is no longer a statistically significant difference in the probability of centralizing between territories with and without a centralized neighbor. This supports Hypothesis 1 with limitations; having a centralized neighbor increases the probability of centralization, but only before 1650; after the Military Revolution, having a centralized neighbor no longer predicts centralization.

There are two potential explanations for this pattern. It could be that territories with a centralized neighbor are more likely to disappear, that is, they are more likely to be conquered by their centralized neighboring territory. In this case, they cease to exist before they have a chance to centralize. We examine the relationship be-

Table 2.1: Predicting Centralization

	Centralized				
	(1)	(2)	(3)	(4)	(5)
ln Centralized Neighbors	1.378***			1.320***	1.170***
	(0.398)			(0.393)	(0.380)
ln Centralized Neighbors	-1.755**			-1.778**	-1.817**
× Post1650	(0.727)			(0.731)	(0.790)
Attack current decade		3.357***		3.254***	
		(1.085)		(1.066)	
Attack, one decade before		2.166**		2.113**	
		(0.868)		(0.854)	
Attack, two decades before		1.499**		1.402*	
		(0.762)		(0.748)	
Attack current decade		-0.431		-0.310	
× Post1650		(2.243)		(2.244)	
Attack, one decade before		1.235		1.301	
× Post1650		(1.951)		(1.946)	
Attack, two decades before		2.805		2.914	
× Post1650		(2.130)		(2.132)	
ln Military Neighbors			1.510***		0.843*
			(0.518)		(0.468)
ln Military Neighbors			-1.126		-0.0503
× Post1650			(1.238)		(1.371)
Controls	✓	✓	✓	✓	✓
R-squared	0.386	0.403	0.384	0.405	0.386
Observations	108,819	96,845	109,083	96,607	108,819

Note Table shows results of Regression 2.1. Controls are the natural logarithm of the number cities and the natural logarithm of the number of cities times a dummy for post 1650. Standard errors are clustered at territory level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

tween centralized neighbors and survival rates in the next section. Another possible explanation is that centralized territories no longer focus on solely conquering their neighbors after the Military Revolution, but use novel military technologies to conquer territories in all parts of the HRE. In this case the overall number of centralized territories instead of distance to a centralized territory matters for centralization. The overall number of territories is the same for all territories and is thus captured by year fixed effects.

Attacks in the current decade, as well as one and two decades ago are positively

related to centralization of a territory. This effect does not change with the onset of the Military Revolution (columns 2 and 4). Looking at the mere risk of war the same pattern emerges: an additional military building of another territory that lies within a 50 km radius increases the probability of centralization by 0.8 to 1.5 percent both before and after the Military Revolution (columns 3 and 5). This suggests that the so-called *threat of war*, which forms the basis of many models, can be understood both as the threat of an existing war or the threat of war occurring. With regard to our hypotheses the results suggest that conflict is related to centralization (in line with Hypothesis 2a), but this effect does only arise with the advent of the Military Revolution (contrasting Hypothesis 2b).

2.6 Effects of Fiscal Centralization

In this section, we first investigate investment activity of centralized compared to non-centralized territories at the local level, then their performance in war, and their survival probabilities. Last, we consider the size of centralized versus non-centralized territories.

2.6.1 Local Investments

In Hypothesis 3, we postulate that fiscally centralized territories invest more in administration and the military, and have higher rates of economic growth. To test this, we analyze investment activities at the city level and proxy investment activity by physical construction. In particular, we examine whether overall construction activity increases, and whether there is more administrative, military and economic construction. We interpret overall construction as conveying information on investment activity in general, administrative construction on investments into state capacity, military construction as investments into military, and economic construction as a proxy for economic activity. Aggregate construction patterns are presented

in Figure 2.5. Construction is increasing from 1400 to around 1750. Aggregate construction reacts to wars, and during the Thirty Years' War (1618 to 1648) and the Seven Years' War (1756 to 1763) overall construction drops.

We estimate the following model

$$100 \times Construction_{ijt} = \beta_1 Centralized_{ijt} + \zeta Controls_{ijt} + \alpha_i + \alpha_j + \alpha_t + \epsilon_{ijt} \tag{2.2}$$

where *Construction* is the number of construction events in city i belonging to territory j in year t. For readability, we multiply construction by 100, and thus values can be interpreted as construction per century. We differentiate between overall construction, administrative construction, military construction, construction of fortifications as a subgroup of military construction, and economic construction. $Centralized_{ijt}$ is a dummy that takes value 1 if city i is in a territory j that is centralized in year t. Based on the literature on fiscal capacity, we expect there to be an increase in administrative and military construction (Hypothesis 3a), as well as in economic construction (Hypothesis 3b). In this case, β_1 will be positive. *Controls* are a dummy indicating whether a city has a secondary ruler and a dummy for whether the territorial affiliation of a city is contested.

We include a number of fixed effects: α_i are city fixed effects, α_j territory fixed effects, and α_t year fixed effects. We can identify city and territory fixed effects separately, as several cities switch territories over time. By including these fixed effects, β_1 measures how construction within a city changes after it becomes part of a centralized territory, excluding all differences in construction of that city compared to other cities that are constant over time, that are constant over time for all cities in the same territories, and all time trends in construction over every year that affect all cities equally. This is important if cities that eventually are part of a centralized territory have different construction levels even before centralization. It also takes into account that centralization might occur during a time in which construction

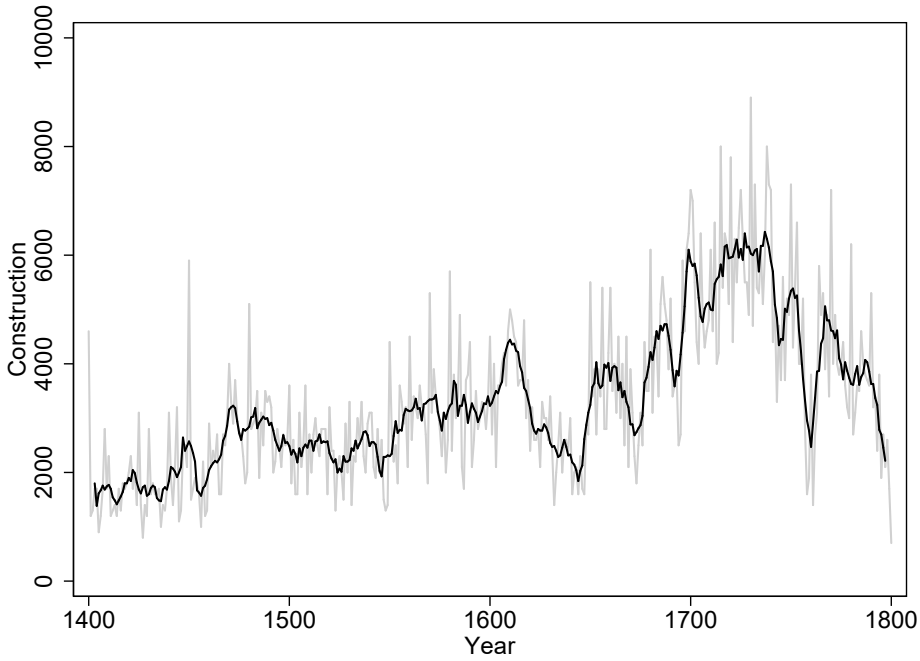


Figure 2.5: Aggregate Construction over Time

Note This figure shows aggregate construction over time. The gray line presents the raw numbers, the black line construction patterns based on a moving average of degree 3. Data sources: see text.

levels change in all cities equally, and controls for aggregate hits on construction during times of war. Standard errors are clustered at the territory level.

Table 2.2 presents results. Construction at the city level increases after a territory centralizes (Table 2.2, Panel A). Overall construction increases by 0.411 construction events per century and this effect is statistically significantly different from zero at the 5 percent level (column 1). This is around a third of the average construction of a territory in a century, which is 1.371. Centralization is also associated with an increase in administrative buildings (0.068, significant at 5 percent level, column 2), which indicates that there are investments into state capacity, as suggested by hypothesis 3a. In addition, military construction increases by 0.039 (significant at five percent level, column 3). This is an economically significant increase considering that on average 0.051 additional military buildings are constructed per century in a city. However, cities are not more likely to build fortifications, an extremely effective, but costly military technology that spread during the Military Revolution (column

Table 2.2: Construction in Cities

	Construction				
	All	Admin	Military	Fortification	Economic
	(1)	(2)	(3)	(4)	(5)
Panel A: All Cities					
Centralized	0.411**	0.068**	0.039**	0.002	0.037
	(0.195)	(0.031)	(0.017)	(0.013)	(0.024)
R-squared	0.027	0.006	0.008	0.005	0.007
Panel B: Core vs Peripheral Cities					
Centralized, core	0.464***	0.068**	0.049***	0.003	0.043*
	(0.131)	(0.028)	(0.014)	(0.011)	(0.023)
Centralized, periphery	0.038	0.068**	0.011	0.014	0.038
	(0.162)	(0.033)	(0.014)	(0.013)	(0.024)
R-squared	0.027	0.006	0.008	0.005	0.007
Controls	✓	✓	✓	✓	✓
City FEs	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓
Mean dep. var	1.371	0.205	0.051	0.024	0.065
Observations	890,241	890,241	890,241	890,241	890,241

Note Results of Regression 2.2. Controls are a dummy indicating whether a city has a secondary ruler and a dummy for whether the territorial affiliation of a city is contested. Standard errors are clustered at territory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

4). Positive effects on overall military construction support the claims of the war-state capacity literature as in Hypothesis 3a. There is no evidence for a differential increase in investments into novel military technologies after centralization.

Hypothesis 3b states that centralized territories have more economic growth. The relationship between centralization and economic construction, which we use as a proxy for economic growth, is presented in column 5. While the estimated coefficient is similar in size to the coefficients for overall, administrative and military construction, standard errors are larger for economic construction. There seems to be more variation with regard to economic growth than for the other variables, and overall fiscal centralization does not seem to be associated with an increase in economic construction, and consequently economic growth.

Do these increases occur equally within centralized territories? In Panel B we differentiate between cities that were in a territory during the time of centralization, and cities that become part of already centralized territories. We refer to cities that were part of a territory at the time of centralization as the *core*, and to cities that became part of an already centralized territory as *periphery*. A peripheral city is coded as centralized after it enters the already centralized territory. We find that only core cities experience a statistically significant increase in aggregate construction of 0.464 buildings per century (significant at 1 percent level, column 1). Administrative construction increases equally in core and peripheral cities after centralization, which indicates that investments into administration in general, and fiscal capacity in particular, are made in all areas of a territory. If a city enters an already centralized territory, administrative investments will thus increase as if the city had been part of the centralized territory when it centralized (column 2). However, military construction only increases in core cities of centralized territories (column 3). Military investments in theoretical models are seen as a public good, i.e. no citizen is excluded from benefiting from them. The finding that only core cities see investments into military are inconsistent with this. Since administrative investments are equal among core and peripheral cities, this difference cannot be due to a lack in the ability of the territory to enforce fiscal capacity in the peripheral cities. Economic construction which proxies economic growth, on the other hand, increased by 0.043 (significant at 10 percent level) in core cities, but not in peripheral cities (column 5).

Event study

To get an even better insight into the relationship between centralization and local

investments over time, we estimate event studies of the following form

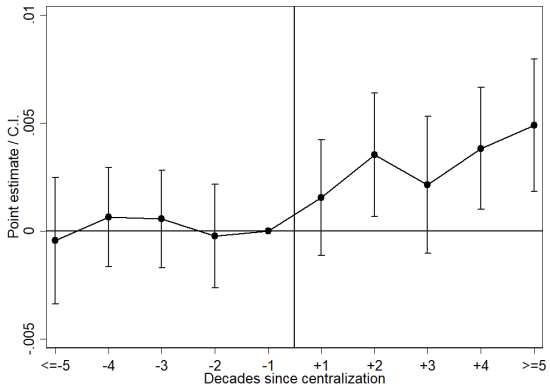
$$\begin{aligned} 100 \times Construction_{ijt} = \beta_{-5} Centralized_{ijt} \times Minus5DecadesAndEarlier_{ijt} + \\ + \sum_{t=-4, t\neq -1}^{4} \beta_t Centralized_{ijt} \times DecadeDummy_{ijt} \\ + \beta_5 Centralized_{ijt} \times Plus5DecadesAndLater_{ijt} + \\ + Controls_{ijt} + \alpha_i + \alpha_j + \alpha_t + \epsilon_{ijt} \end{aligned} \tag{2.3}$$

where $Construction_{ijt}$, $Centralized_{ijt}$, α_i, α_j and α_t are defined as above.

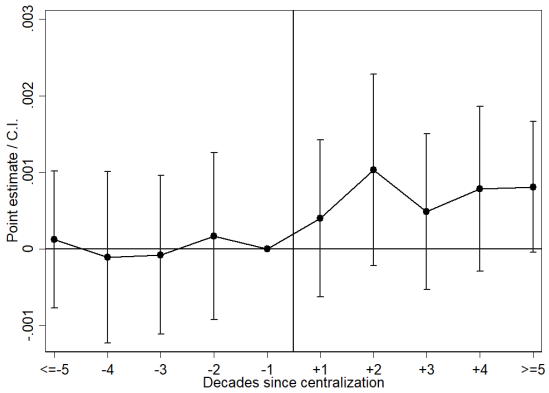
Centralization is interacted with a set of decade dummies for for the four decades before and after centralization, where the dummy for the decade prior to centralization is omitted. In addition, we include interactions with dummies for all decades that are at least 50 years before ($Minus5DecadesAndEarlier_{ijt}$) or after ($Plus5DecadesAndLater_{ijt}$) centralization. Thus, we estimate construction in cities that will eventually centralize for each decade relative to construction in the ten years before centralization. We control for whether there is a secondary ruler and whether sovereignty over the city is contested. We include city fixed effects, α_i, territory fixed effects, α_j, and year fixed effects, α_t. Thus, β_t is the estimate of the difference in construction in cities that centralize compared to their construction the decade before they centralize after excluding all differences that are due to city specific factors, territory specific factors or time specific factors that exist for all territories.[18] Standard errors are clustered at the territory level.

Results are presented in Figure 2.6, and add more nuance to the results of Table 2.2. There is no pre-trend in overall construction before centralization. Overall construction only starts to increase in the first decade after centralization, and continues to increase further over the next decades compared to construction levels in

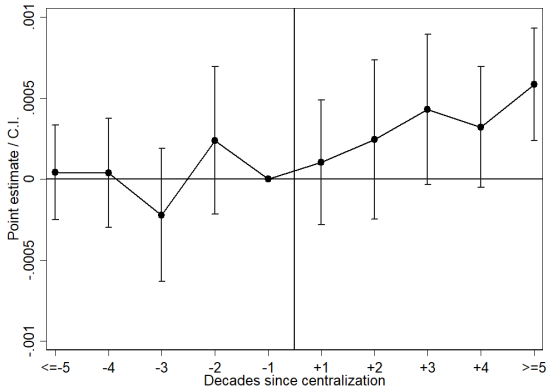
[18]In Appendix B.2, we show results of Regression 2.2 when we limit the analysis to all cities that will centralize eventually. Results are nearly identical, as is expected as we include city fixed effects in both regressions. Omitting cities that do not centralized will have an impact on the estimates of the time fixed effects.



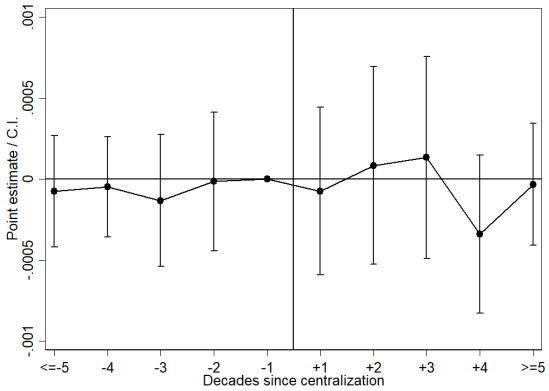
Overall construction



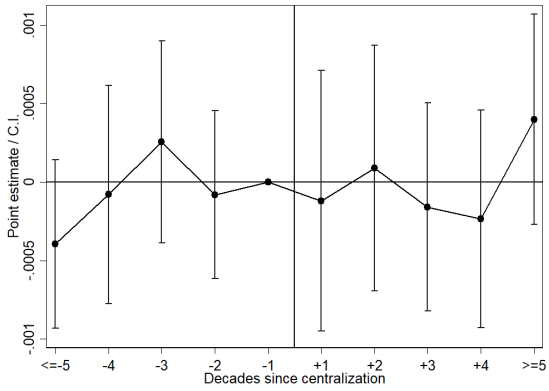
Administrative construction



Military construction



Fortifications



Economic construction

Figure 2.6: Differences in Construction over Time

Note Results of Regression 2.3 with 95 percent confidence intervals. Standard errors are clustered at territory level. Data sources: see text

the decade before centralization. Before centralization, administrative construction, conditional on city, territory, and year fixed effects, also shows no pre-trends. After centralization occurs, administrative construction jumps up, and remains on this higher level. Military construction is slightly lower in the third decade before and slightly higher in the second decade before centralization compared to the decade before centralization. After centralization, military construction in centralized cities increases over the next decades. The construction of fortifications does not differ for centralized territories before they centralize, but is overall a bit lower in the fourth decade after centralization. The pattern for economic construction looks less smooth than that for overall or administrative construction. Economic construction seems to be lower in all years that are at least 50 years prior to centralization compared to the period before centralization. After centralization occurs, there is at first no change in construction patterns compared to non-centralized territories. There is evidence for a positive effect starting 50 years after centralization. The patterns thus add further support for Hypothesis 3a, i.e. that fiscally centralized territories invest more in their administration and their military, as the positive effects begin right after centralization was introduced. There is no evidence strengthening Hypothesis 3b, which stated that centralized territories experience more growth.

2.6.2 Warfare in Centralized Territories

Military investments are higher in centralized cities that lie in the core of centralized territories, but does this have an effect on war activities within centralized territories as is commonly assumed in the literature? We estimate the following regression to look at the number of attacks cities in centralized territories are subjected to

$$100 \times Attacks_{ijt} = \beta_1 Centralized_{jt} + \beta_2 Centralized_{jt} \times Post1650_t + \alpha_i + \alpha_j + \alpha_t + \epsilon_{ijt} \tag{2.4}$$

where $Attacks_{ijt}$ is the number of attacks in a year in city i in territory j in year t. We multiply this times 100 for easier readability. Coefficients can thus be interpreted as the change in the number of attacks occurring in a century. In further regressions, we differentiate between the number of attacks that lead to physical destruction and the number of attacks that are associated with monetary losses, for example because the city was looted, troops were billeted in it, or the city made payments to hostile troops. This captures the cost of war for the local population. According to the literature on fiscal capacity people are willing to pay taxes, because by doing so they are protected from the negative impacts of war. By looking at the relationship between fiscal centralization and attacks with costs for the local population, we estimate whether this link existed in the Holy Roman Empire. $Centralized_{jt}$ is a dummy that takes the value 1 if territory j is centralized in year i. If $\hat{\beta}$ is larger than 0, cities in centralized territories are subject to more attacks, which we interpret as indicating that territories are involved in more war in general. $Post1650_t$ is a dummy for all years after 1650, and can be seen as a dummy for the Military Revolution having taken place. In a second set of regressions we differentiate between cities that were part of a centralized territory at the timing of centralization and cities that become part of centralized territories after centralization, i.e. core and peripheral cities. α_i are city, α_j territory and α_t time fixed effects. Including city and territory fixed effects is important as attacks predict centralization. By including fixed effects we only consider changes in the number of attacks, not the overall level, this means we control for the possibility that centralized territories might experience more attacks over the entire period under consideration. Standard errors are clustered at the city level.

Over the entire time period under consideration, the number of attacks on cities in centralized territories does not change (column 1, Panel A). However, once we look at the effect before and after the Military Revolution separately, we find that cities in centralized territories experience 0.003 fewer attacks per year (significant at

5 percent level) compared to cities that are in non-centralized territories. Next, we focus on attacks that have negative effects on the affected cities in columns 4 to 6 of Panel A. The number of attacks that lead to physical destruction is slightly higher in centralized cities before 1650, but does not differ from the number of attacks with physical destruction in non-centralized cities after 1650 (column 4). Cities in centralized territories experience fewer attacks that lead to a loss of money after the Military Revolution (column 6). After the Military Revolution, when military investments arguably become important for success in war, increased military investments of cities in centralized territories are effective in reducing overall attacks, and as a result they also experience less attacks that lead to losses for the local population. This confirms the argument made in the literature about the incentives of citizens to accept the introduction of fiscal institutions.

In Panel B we explore the implication of regional inequalities in military investments between cities in the core and in the periphery of territories. Column 2 shows that decreases in the number of attacks only occur in core cities – and only after 1650. Peripheral cities, where military investments did not increase after they become part of centralized territories, do not experience a drop in the number of attacks in general (columns 1 and 2) or the number of costly attacks (columns 3 to 6) either before or after the Military Revolution.

2.6.3 Vanishing of Territories

Does the ability of centralized territories to finance more military investments translate into a lower probability of vanishing, as stated in Hypothesis 4a? We estimate the following OLS model to examine the relationship between fiscal centralization,

Table 2.3: Centralization and Attacks on Cities

	Attacks					
	Number		With physical destruction		With loss of money	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All cities						
Centralized	0.000	0.001	0.000	0.001*	-0.000	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Centralized × Post 1650		-0.003**		-0.001*		-0.002**
		(0.001)		(0.001)		(0.001)
R-squared	0.034	0.034	0.015	0.015	0.023	0.023
Panel B: Differentiating between core and new cities						
Centralized core cities	0.000	0.002	0.000	0.001*	-0.000	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Centralized core cities × Post 1650		-0.004**		-0.001*		-0.002**
		(0.002)		(0.001)		(0.001)
Centralized peripheral cities	-0.001	-0.004	-0.000	-0.000	-0.001*	-0.003
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.002)
Centralized peripheral cities × Post 1650		0.002		-0.001		0.001
		(0.005)		(0.001)		(0.003)
City FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.034	0.034	0.015	0.015	0.023	0.023
Observations	890,245	890,245	890,245	890,245	890,245	890,245

Note Results of Regression 2.4. Standard errors clustered at city level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively. Data sources: see text

and the disappearance of territories

$$100 \times Vanish_{jt} = \beta_1 Centralized_{jt} + \beta_2 Centralized_{jt} \times Post1650_t + \zeta Controls_{jt} + \alpha_t + \epsilon_{it} \quad (2.5)$$

where $Vanish_{jt}$ is a variable that takes on the value 1 if territory j vanishes. We multiply this with 100 for readability. In further regressions we look at different potential reasons for vanishing, such as conflict and extinction of the ruling family.[19] After a territory vanishes, it is dropped from the sample. $Centralized_{jt}$ is a dummy for centralized territories. $Post1650$ is a dummy for all years after 1650. We interact centralization with this to estimate whether their connection with territorial survival changes after the Military Revolution. If $\hat{\beta}_1$ is smaller than 0, centralization is associated with a lower probability of a territory vanishing before the Military Revolution. If $\hat{\beta}_1 + \hat{\beta}_2$ is smaller than 0, centralization is associated with a lower probability of a territory vanishing after the Military Revolution. $Controls_{jt}$ are the lagged natural logarithm of the number of cities, a dummy for the existence of any centralized territories within a 50 km radius, whether there was an attack on the territory in the current period or the period before. We interact all controls with a dummy for all years post 1650, and this allows the relationship of the control variable and vanishing to differ before and after the Military Revolution. α_t are year fixed effects that capture shocks that are common for all territories, which includes the overall number of centralized territories that exist in each year. We do not include territory fixed effects because these would predict survival perfectly. Standard errors are clustered at the territory level.

Table 2.4 looks at the relationship between $Vanish$ and $Centralized$ as outlined in Regression 2.5. Centralized territories are 0.401 percentage points less likely to cease to exist in a given year (column 1), which is a very large effect considering that the

[19] A territory is coded as vanishing due to conflict or extinction if the territory loses at least one city in the year they vanish due to the respective reason. Results are robust to only regarding cases where at least 50 percent of all lost cities are lost due to the respective reason.

Table 2.4: Probability of Vanishing

	Vanishing					
	All		Due to Conflict		Due to Extinction	
	(1)	(2)	(3)	(4)	(5)	(6)
Centralized	-0.401***	-0.378***	-0.049**	-0.053**	-0.449***	-0.539***
	(0.113)	(0.104)	(0.023)	(0.025)	(0.106)	(0.083)
Centralized × Post 1650	-0.224	-0.116	0.008	0.066	-0.022	0.101
	(0.179)	(0.177)	(0.051)	(0.054)	(0.145)	(0.135)
Centralized Neighbors		0.065		0.019**		0.056
		(0.099)		(0.009)		(0.058)
Centralized Neighbors × Post 1650		0.200		0.036*		0.042
		(0.139)		(0.019)		(0.092)
Attack		0.308***		-0.017		0.019
		(0.107)		(0.018)		(0.061)
Attack × Post 1650		0.014		0.193**		-0.050
		(0.208)		(0.094)		(0.123)
Attack one decade before		-0.156		0.010		0.150
		(0.190)		(0.025)		(0.157)
Attack one decade before × Post 1650		-0.247		-0.188**		-0.223
		(0.261)		(0.092)		(0.195)
Controls	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.006	0.006	0.006	0.006	0.004	0.004
Mean dep. Var	0.444	0.444	0.030	0.030	0.188	0.188
Observations	109,604	109,604	109,604	109,604	109,604	109,604

Note Table presents results of 2.5. Controls are the natural logarithm of the number of cities, and the interaction natural logarithm of the number of cities with Post 1650. Standard errors are clustered at the territory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively. Data sources: see text

baseline probability of vanishing is 0.444 percentage points each year. Controlling for the existence of centralized neighbors and attacks, the effect decreases slightly (0.378, column 2). This supports Hypothesis 4a, i.e. centralized territories are less likely to vanish. In addition we are able to elicit a number of additional interesting patterns about which territories vanish. The existence of centralized neighbors has no effect on the general probability of vanishing before or after the Military Revolution. Being attacked increases the probability of vanishing after 1650, indicating that the novel technologies in warfare introduced during the Military Revolution increase the ultimate cost of war for sovereigns: warfare can lead to extinction.

Next we turn to different reasons for vanishing. Centralized territories are around 0.05 percentage points less likely to vanish because of conflict, a sizable effect compared to a baseline probability of vanishing because of a conflict of 0.03 percent (see column 3 and 4). This is in line with the common interpretation in the literature, where centralization makes territories better at conflict (either by increasing the probability to win, or by discouraging other territories from engaging in conflict in the first place, as suggested in Table 2.3), and thus centralized territories are more likely to survive. In addition, having a centralized neighbor increases the probability that a territory vanishes because of conflict and this probability increases even further after the Military Revolution. This might explain why in Table 2.1 we find that territories are no longer more likely to centralize if they have a centralized neighbor after the Military Revolution: while having a centralized neighbor increases the incentives to centralize to be able to compete militarily against centralized neighbors, it also increases the probability that they vanish because centralized neighboring territories attack them. Centralization is also associated with a decrease in the probability of vanishing because of extinction of the ruling family, holding the number of attacks on cities within the territory constant (0.45 to 0.54 percentage points lower probability, columns 5 and 6) compared to a baseline probability of 0.18 percent.

Overall the results in Table 2.4 provide evidence in favor of the hypothesis that centralization decreases the probability that a territory ceases to exist because they are more successful at fighting wars (Hypothesis 4c).

2.6.4 Territorial Expansion

We now want to examine whether centralized territories grow in size after they centralize, as claimed in hypothesis 4b. To test this, we estimate the following regression

$$\begin{aligned} NumberCities_{jt} = \beta_1 Centralized_{jt} + \beta_2 Centralized_{jt} \times YearsCentralized_{jt} \\ + \gamma_1 Attacks_{jt} + \gamma_2 Attacks_{jt} \times Centralized_{jt} \\ + \alpha_j + \alpha_t + \epsilon_{jt}, \end{aligned} \tag{2.6}$$

where $NumberCities_{jt}$ is the number (or natural logarithm) of cities that belong to territory j in year t. In different regressions, we use all cities and uncontested cities (results in Table 2.5 in main text) as well as contested cities and cities with only a single ruler (results in Table B.3 in the Appendix). Uncontested cities are cities over which a territory claims sovereignty without this claim being challenged by other territories, whereas for contested cities this claim is challenged. Cities with a single ruler are cities in which there is just one territory that claims sovereignty over the city. Not all cities with several rulers are contested, in some cases several territories agree on exerting joint control over a city. An increase in the number of uncontested cities thus measures the increase in the size of territories that is not disputed by other territories, and offers a measure of an increase in factual control over a region. $Centralized_{jt}$ is a dummy indicating whether territory j was centralized in year t, $YearsCentralized_{jt}$ is the number of years territory j has been centralized. We include this term to allow the effect of centralization on size to grow (or fall) over the duration of centralization. To see whether the potential relationship between

centralization and size of territories is driven by attacks, we control for $Attacks_{jt}$, the number of attacks in the current period. We interact this with $Post1650_t$ to allow attacks to have a different effect on territory size before and after the Military Revolution.

α_j and α_t are territory and time fixed effects. If centralized territories are larger before they centralize, this will be accounted for by the territory fixed effects. Including time fixed effects accounts for any increase in territory size after centralization that is explained by general time trends and not centralization. This is important if territories centralize during a time period in which (surviving) territories in general get larger. Standard errors are clustered at the territory level.

Results are presented in Table 2.5. Territories are larger after they centralize, holding territory and time fixed effects constant. Centralized territories on average hold 17 additional cities (significant at 10 percent level) after centralization or around 23 percent (significant at 5 percent level) more cities (column 1 in Panel A and B respectively). This increase in size occurs over time (column 2). A centralized territory grows by one city every five years (column 2, Panel A) or by 1 percent every ten years (column 2, Panel B). Controlling for the number of attacks on cities within the territory in the current decade does not have an effect on these coefficients (see column 3). Attacks, which we interpret as alluding to war activities in general, are associated with an increase in territory size by around 1.4 cities (column 3, Panel A). Centralized territories grow by gaining uncontested sovereignty over cities: in columns 4 to 6 we only look at the number of cities within territories which belong to it uncontested by claims of sovereignty by other territories and where this claim is not contested by other territories. The coefficients are nearly identical to those in column 1 to 3.

To understand the reasons for the increase in the size of centralized territories, we calculate the number of cities that enter and leave centralized territories compared to non-centralized territories using Regression 2.6. Results are summarized in Table

Table 2.5: Territory Size

	Cities					
	All			Uncontested		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Number Cities						
Centralized	16.571*	-4.990	-5.030	16.511*	-5.438	-5.473
	(9.722)	(3.617)	(3.564)	(9.649)	(3.604)	(3.551)
Centralized × Years Centralized		0.231*	0.225*		0.235*	0.229*
		(0.125)	(0.118)		(0.124)	(0.117)
Attack, this decade			1.416*			1.319*
			(0.786)			(0.777)
Attack, this decade × Post 1650			2.348			2.497
			(3.140)			(3.103)
R-squared	0.824	0.837	0.838	0.822	0.836	0.837
Panel B: Natural logarithm of Cities						
Centralized	0.234**	0.097	0.096	0.225**	0.061	0.061
	(0.111)	(0.108)	(0.109)	(0.114)	(0.115)	(0.116)
Centralized × Years Centralized		0.001***	0.001***		0.002***	0.002***
		(0.000)	(0.000)		(0.000)	(0.000)
Attack, this decade			0.023			0.023
			(0.028)			(0.028)
Attack, this decade × Post 1650			0.010			0.019
			(0.032)			(0.031)
R-squared	0.946	0.946	0.946	0.942	0.943	0.943
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Observations	109,799	109,799	109,799	109,799	109,799	109,799

Note Table presents results of Regression 2.6. Standard errors are clustered at territory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively. Data sources: see text

2.6.

Centralized territories do not gain or lose more cities overall compared to non-centralized cities, holding constant their current size, and territory and year fixed effects (columns 1 and 4). There is also no relationship between centralization and gaining (losing) cities due to conflict (column 2 and 4). Territories that are engaged in war activities are more likely to gain or lose cities via conflict (columns 2 and 4 in Panel B).

How can we reconcile the finding that centralized territories are larger than non-centralized territories, but at the same time they do not gain more or lose less cities than non-centralized territories, ceteris paribus? All territories grow over time, and centralized territories exist longer, as they are less likely to vanish (see Table 2.4). Centralized territories thus have the opportunity to grow larger because they grow over a longer period of time, not because they grow at a higher rate than non-centralized territories.

2.7 Conclusion

The emergence of fiscal administrations that are able to levy and collect taxes are an important part of modern state formation (Weber, 1919; Tilly, 1975). In this Chapter, we document the history of fiscal capacity and how it links to the survival of territories in the Holy Roman Empire and analyze the causes and effects of the introduction of the first institutionalized and centralized fiscal organization, the *Chamber*.

We show that territories that are exposed to more incentives to centralize – either because neighboring territories are already centralized or because threat of war is higher – are more likely to introduce a *Chamber*. This confirms Tilly (1975) influential argument that wars led to the formation of states with fiscal administrations. We also look at the consequences of fiscal centralization, and show that central-

Table 2.6: Gains and Losses of Cities

	Cities gained			Cities lost		
	all	via conflict	purchased	all	via conflict	sold
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Cities gained and lost: Baseline						
Centralized	5.183	3.690	-1.833	-5.507	-0.777	-0.374
	(6.425)	(5.749)	(2.234)	(7.283)	(2.295)	(0.300)
Centralized × Years Centralized	0.00151	0.0153	0.0167	-0.0391	0.00337	-0.00523***
	(0.0648)	(0.0744)	(0.0302)	(0.0295)	(0.0100)	(0.00159)
R-squared	0.034	0.023	0.014	0.018	0.014	0.018
Panel B: Cities gained and lost: Controlling for Warfare						
Centralized	5.065	3.630	-1.814	-5.889	-0.862	-0.390
	(6.399)	(5.720)	(2.244)	(7.258)	(2.312)	(0.303)
Centralized × Years Centralized	0.004	0.013	0.019	-0.030	0.005	-0.005***
	(0.063)	(0.068)	(0.033)	(0.027)	(0.011)	(0.002)
Attack, this decade	2.682*	1.823*	-0.718	8.369**	1.955*	0.357
	(1.544)	(1.098)	(0.470)	(3.292)	(1.007)	(0.318)
Attack, this decade × Post 1650	-1.201	0.952	-0.779	-5.145	-0.800	-0.159
	(1.734)	(3.029)	(1.256)	(3.133)	(1.029)	(0.414)
R-squared	0.034	0.023	0.015	0.019	0.015	0.018
Observations	109,083	109,083	109,083	109,083	109,083	109,083

Note Table presents results of Regression 2.6, with the number of cities gained (columns 1 to 3) or the number of cities lost (columns 4 to 6) as a dependent variable. Standard errors are clustered at the territory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively. Data sources: see text

ized territories are less likely to vanish, and are thus able to become larger than non-centralized territories. We show that centralized territories invest more in their administration and in their military. However, military investments do not occur equally across the territory: increases in investments only occur in those cities that were already part of the territory at the time it centralized (*core* territory), not in cities that enter the territory later (*peripheral* territory). This has important implications for the way we should think about fiscal centralization. In theoretical models citizens agree to have fiscal centralization because they expect taxes to be spent on a public good, and thus to profit from them. We find evidence for this in the Holy Roman Empire. However, we add that there is a second important layer: after centralization was introduced it increases the level of fiscal capacity in the entire territory. Cities that become part of an already centralized territory do not enter a bargain with the sovereign on whether fiscal centralization should be introduced, and are thus not offered any "rewards", and consequently see no increase in military investments. Centralization can thus have large effects on inequality within territories. Higher military investments in the core of territories decreases their exposure to war when financial resources become important to win wars, whereas there is no such relationship for peripheral cities.

B Appendix to Chapter 2

B.1 Explanation of the Coding Process of Territories

Coding the history of the Holy Roman Empire (HRE) entails coding the history of its ruling families. Under loose regency of the Emperor, countless territorial entities existed, some large (Fürsten- und Herzogtümer), some small (Graf- und Herrschaften). We aim to assign every entity a unique ID and track its territorial holdings using said ID. A correct dataset thus depends on identifying ruling units, and identifying their respective territories.

One can roughly distinguish two types of territories and thus rulers: ecclesiastical and secular. We understand the largely stable ecclesiastical states under clerical rule as one entity throughout their existence. Secular territories, however, were continuously broken up, re-structured, and unified as their rulers changed. These dynamics were often determined by familial structure: Inheritances defined succession, warring siblings or heirs split territories, and marriages proved pivotal when no direct male heir was in a lineage. Understanding and consistently coding this history thus entails a full understanding of the relevant noble families. For this reason, we combine territorial information with lineage information.

Regularly, sons inherited their father's possessions. This could in principle take on many forms: sons sometimes ruled jointly, split the territory between them, or chose a unique successor. Most commonly adapted was the *Primogenitur*; here, the oldest son inherited all possessions from his father. The Golden Bull of 1356 instituted the *Primogenitur* in all electorate territories of the Holy Roman Empire, and other minor territories followed suit. Succession became more intricate when a ruler died without eligible (i.e. male) heirs. While surrounding rulers might have tried to bolster an inheritance claim through strategic marriage, or negotiating a contract to this effect, there were often multiple claims, causing dispute and sometimes war.

We trace lineages (that is, a string of male rulers in one family) throughout their existence by assigning a unique individual code. If a lineage dies out, its Territory ID vanishes with it. In the occasion of a split inheritance between siblings, we attest that there is always one favourable part of the territory. The sibling who inherits the favourable territory inherits the lineage dummy. For the newly founded lineage, a new code is established. Free cities and ecclesiastical territories are assigned one code throughout their history.

To record the territorial holdings, cities are then assigned to their respective rulers as expressed through the lineage codes. For every change, we record its specific reason. If a city is outside the Holy Roman Empire in a given period, we record the state it belongs to, and the family ruling the state, adhering to the rules stated above.

The rule structure of many cities was in reality multi-layered: one family could own estates but enfeoff others, for example. To account for this, we trace secondary in addition to primary rulers, conscious that we will not be able to depict actual power relations accurately. We include secondary rulers as robustness checks and for extended analyses.

Finally, we adjust the territorial dataset to fit the specific application regarding fiscal centralization of states. In order to accurately depict treatment, we "stitch together" states where a ruling family dies out but its institutions survive, and revert to the territorial definitions most suitable to the specific question.

B.2 Figures

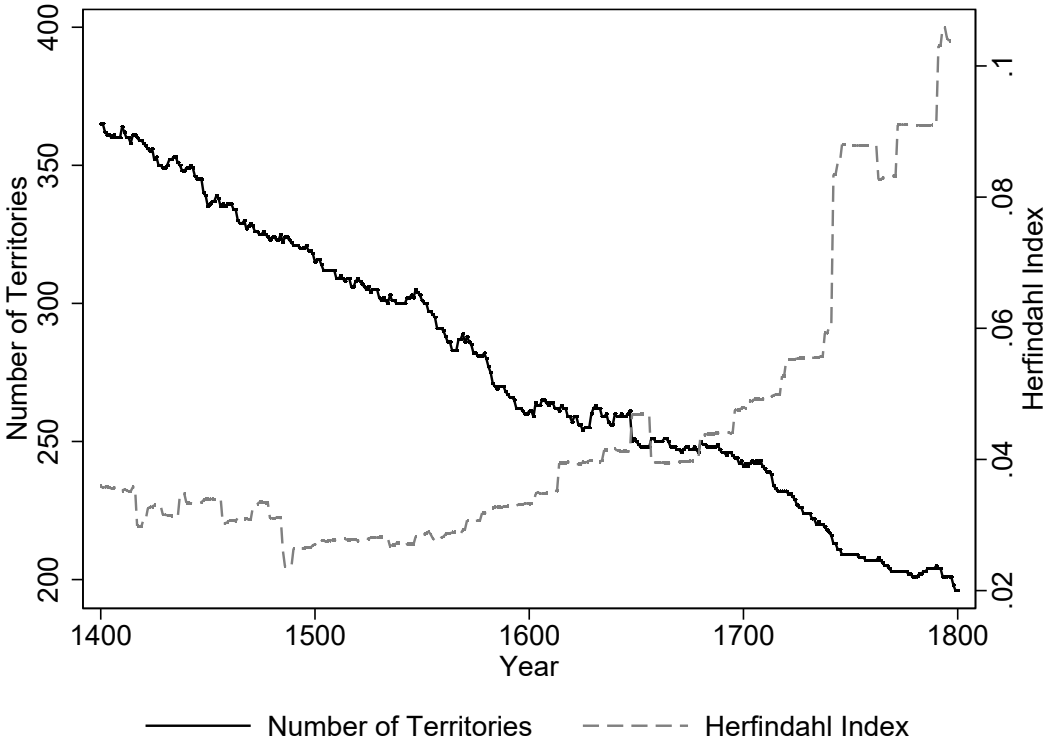
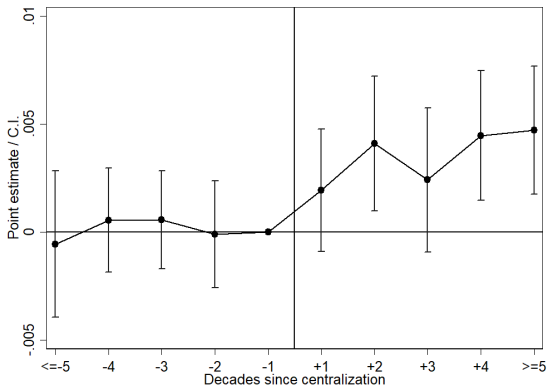
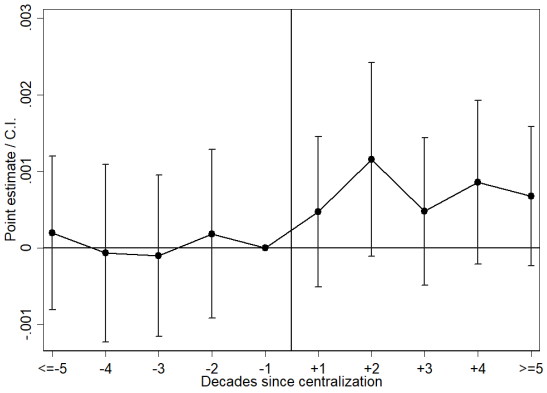


Figure B.1: Number of Territories and Concentration of Cities

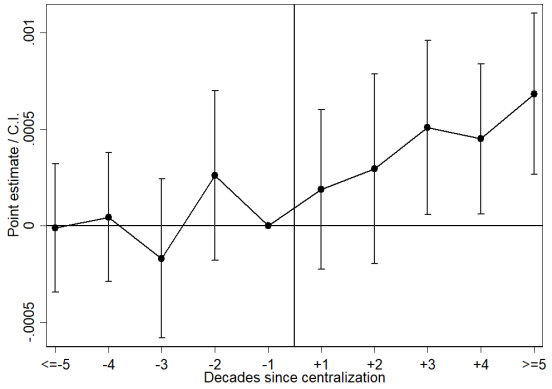
Note Left axis shows number of territories, right axis the Herfindahl Index that measures the concentration of cities across territories. Data sources: see text



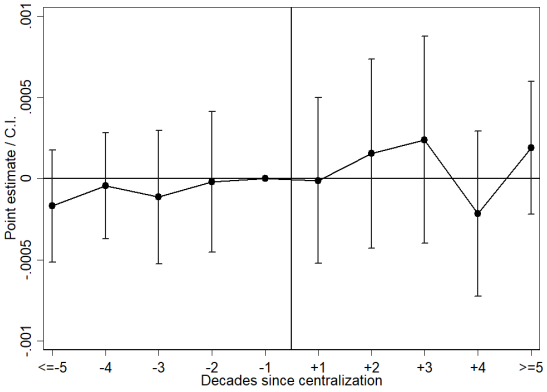
Overall construction



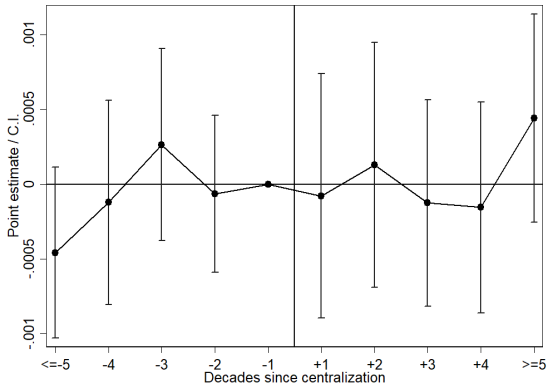
Administrative construction



Military construction



Fortifications



Economic construction

Figure B.2: Time Patterns of Construction, Intensive Margin

Note Results of regression 2.3 limiting the sample to cities that will at some point be part of a centralized territory with 95 percent confidence intervals. Standard errors are clustered at territory level. Data sources: see text

B.3 Tables

Table B.1: Centralized Territories and Dates of Centralization

Territory	Year	Name	Selected Sources
Albertine Saxony	1524	Rentkammer	Jeserich et al. (1983, p. 816)
Prince-Bishopric of Augsburg	1718	Hofkammer	Wüst (1987, p.39)
Margraviate of Baden-Baden	1588	Rentkammer	Taddey (2000, p. 168) Carlebach (1906, p. 43)
Margraviate of Baden-Durlach	1578	Rentkammer	Jeserich et al. (1983, p. 630), Taddey (2000, p. 168)
Prince-Bishopric of Bamberg	1638	Hofkammer	Weiß (2010)
Duchy of Bavaria	1550	Hofkammer	Jeserich et al. (1983, p. 581)
Margraviate Brandenburg	1577	Amtskammer	Schultze (2004, p. 142-3)
Electorate Cologne	1587	Hofkammer	Wüst (1987, p. 37)
Bishopric of Eichstätt	1651	Hofkammer	(Braun, 1991, p. 94)
Landgraviate of Hesse-Darmstadt	1595	Rentkammer	Jeserich et al. (1983, p. 648)
Landgraviate of Hesse-Kassel	1558	Rentkammer	Jeserich et al. (1983, p. 648)
Duchy of Jülich-Berg	1547	Rechenkammer	Jeserich et al. (1983, p. 708)
Duchy of Cleve Mark	1557	Rechenkammer	Schottmüller (1896, p. 66)
Electoral Palatinate	1557	Rechenkammer	Press (1970, p. 99-100)
Electorate of Mainz	1532	Hofkammer	Wüst (1987, p.37)
Duchy of Mecklenburg-Schwerin	1659	Kammer	Hamann (1965, p. 83)
Duchy of Mecklenburg-Strelitz	1701	Kammer	Hamann (1965, p. 99)
Prince-Bishopric of Münster	1573	Rechenkammer	Jeserich et al. (1983, p. 732)
Prince-Bishopric of Paderborn	1723	Hofkammer	Jeserich et al. (1983, p. 735)
Ernestine Saxony	1633	Kammer	Jeserich et al. (1983, p. 853)
Electorate of Trier	1719	Hofkammer	`http://www.rheinische-geschichte.lvr.de/orte/Gebiete_1789/herrschaften/Seiten/Kurtrier.aspx`
Duchy of Württemberg	1521	Rentkammer	Bernhardt (1971, p. 32-3)
Bishopric of Würzburg	1553	Kammer	Reuschling (1984, p. 232-4)

Table B.2: Attacks on Territories and Gains and Losses of Cities

	Gains		Losses	
	Number	ln	Number	ln
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Attack	90.906***	0.119***	38.984	0.075***
	(32.187)	(0.028)	(26.791)	(0.028)
Observations	18,233	18,233	18,233	18,233
R-squared	0.235	0.309	0.183	0.205
Panel B: Controlling for attacks in past				
Attack	91.058**	0.100***	37.866	0.066**
	(37.520)	(0.028)	(24.333)	(0.026)
Attack, one year before	-24.572	0.003	49.860**	0.043*
	(42.988)	(0.027)	(22.691)	(0.025)
Attack, two years before	5.118	0.009	4.065	-0.024
	(29.314)	(0.021)	(31.949)	(0.028)
Attack, three years before	16.337	0.002	-22.200	-0.010
	(27.518)	(0.022)	(37.512)	(0.025)
Attack, four years before	-7.569	-0.016	-4.398	-0.001
	(15.285)	(0.019)	(23.370)	(0.024)
Attack, five years before	11.401	0.014	58.783*	0.014
	(22.264)	(0.024)	(32.883)	(0.027)
Observations	12,842	12,842	12,842	12,842
R-squared	0.225	0.315	0.170	0.211

Note Column 1 and 3 look at the absolute number of gains/losses, column 2 and 4 at the natural logarithm of 1 plus the number of gains/losses. Standard errors are clustered at the terrtitory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Table B.3: Territory Size

	Cities					
	Contested			Single Rule		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Number Cities						
Centralized	0.060	0.448	0.442	17.686**	-3.130	-3.143
	(0.190)	(0.281)	(0.280)	(8.966)	(3.218)	(3.180)
Centralized × Years Centralized		-0.004	-0.004		0.223**	0.217**
		(0.003)	(0.003)		(0.111)	(0.106)
Attack, this decade			0.097**			0.856
			(0.047)			(0.528)
Attack, this decade × Post 1650			-0.149			2.581
			(0.096)			(2.728)
R-squared	0.470	0.472	0.472	0.806	0.823	0.823
Panel B: Natural logarithm of Cities						
Centralized	-0.053	0.000	-0.000	0.275**	0.102	0.101
	(0.073)	(0.058)	(0.059)	(0.108)	(0.102)	(0.103)
Centralized × Years Centralized		-0.001	-0.001		0.002***	0.002***
		(0.001)	(0.001)		(0.000)	(0.000)
Attack, this decade			0.017			0.017
			(0.013)			(0.025)
Attack, this decade × Post 1650			-0.015			0.025
			(0.028)			(0.032)
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
R-squared	0.770	0.770	0.770	0.938	0.938	0.938
Observations	109,799	109,799	109,799	109,799	109,799	109,799

Note Results of Regression 2.6 using the number of contested cities (columns 1 to 3) and the number of cities with a single ruler (columns 4 to 6) as an outcome. Standard errors are clustered at territory level. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

3 | Exorcizing Hitler: Anti-Semitism and the Denazification of Germany

Our ability to adapt is amazing. Our ability to change isn't quite as spectacular.

Lutz (2010)

3.1 Introduction

Attitudes and beliefs can persist over surprisingly long periods. For example, Guiso et al. (2016) show that self-governance promoted cooperation among city dwellers in medieval Italy, and that the same locations are richer today, have a more developed civic society, and have access to more financial services. Areas of Africa exposed to the slave trade in the 17th and 18th century are still poorer today, and they exhibit lower levels of interpersonal trust (Nunn and Wantchekon, 2011). The German occupation of Russia and the annihilation of Jewish life continues to shape local political and economic conditions (Acemoglu et al., 2011). Similarly, Voigtländer and Voth (2012) find that German towns and cities that had anti-Semitic progroms at the time of the Black Death in 1348-50 were still much more anti-Semitic in the interwar period. Persistence of beliefs may reflect strong patterns of parent-child transmission of attitudes (Bisin and Verdier, 2000, 2001; Bénabou and Tirole, 2011;

This Chapter is joint work with Nico Voigtländer and Hans-Joachim Voth.

Zumbuehl et al., 2013).

At the same time, there is abundant evidence of changes in attitudes and beliefs – from behaviors like duelling to attitudes towards pre-marital sex, women's rights, and gay marriage, culture can change surprisingly rapidly (Fernández-Villaverde et al., 2011). During the Protestant Reformation culture changed in large parts of Europe (Becker and Woessmann, 2009; Becker et al., 2016; Cantoni, 2015). Alesina and Fuchs-Schündeln (2007) show that Germans who grew up in East Germany believe more in redistribution and government intervention than their peers in the West.

What is less clear, however, is when, how, and why beliefs change over time. Recent experimental evidence suggests that the right kind of institutional framework can quickly influence attitudes such as trust (Cassar et al., 2014). There is evidence that schooling plays a role; teaching styles, compulsory schooling, and school curricula have been shown to shape political and civic attitudes of students (Algan et al., 2013; Bandiera et al., 2018; Cantoni et al., 2017). At the same time, policies aimed at integrating immigrants in 16 different European societies appear to have only limited effects (Aleksynska and Algan, 2010). Restrictions on beliefs can also lead to a backlash; in Fouka (2016) language restrictions strengthen the value individuals assign to their ethnic identity, and lead to an increase in investments into it. Under what conditions can persistence of beliefs and attitudes be overcome?

In this chapter, we examine the effectiveness one of the largest social experiments in history: denazification, i.e. the systematic attempt by the victorious Allies to re-educate the entire German population after 1945 aiming to stamp out racial hatred, authoritarianism, and militarism (Biddiscombe, 2007). Allied policy during the war established denazification as a priority for the time after victory. The occupying forces took over the administration of Germany, ran and licensed all newspapers and other media, revamped school curricula, and incarcerated hundreds of thousands of citizens who had been involved with the Nazi regime. Millions of

Germans had to submit detailed questionnaires and hundreds of thousands were tried in Allied courts; many were dismissed or imprisoned. Anti-Semitism changed from an officially sanctioned principle of policy to a public taboo; citizens were forced to visit former concentration camps and attend films depicting the horrors of the Holocaust.

There are many reasons to believe that the denazification program was not successful: there was personnel continuity at many levels in German society, and for example around 94 percent of Bavarian judges and prosecutors, and 77 percent of employees in the finance ministry were former members of the Nazi Party in 1951 (Judt, 2005). 60 percent of West Germany's reconstituted diplomatic corps had served either in the SS or the Gestapo. Nor did attitudes in the overall population change quickly: more than a third of Germans in the 1950s felt that Germany should not have Jews living within its borders, and a quarter still held Adolf Hitler in high regard. Even today, opinion polls regularly find that a significant share of the German population holds anti-Semitic views (Bergmann and Erb, 1997).

Our indicator of denazification success in this paper are profanations of Jewish cemeteries – an expression of extreme levels of anti-Semitism. To control for historical anti-Semitism, we link profanations after World War II to the occurrence of profanations during the Weimar period, i.e. before the Nazi regime. There is local persistence of anti-Semitism, but only in the American occupation zone; municipalities in which Jewish cemeteries were profaned during the Weimar Republic are more likely to experience profanations after 1945 in the American sector, controlling for the number of Jewish cemeteries. There is no persistence in the British sector.

We then study when attitudes persist or change in this unique historical setting. Denazification policies differed significantly by Allied zone of occupation after 1945. The US pursued a highly ambitious and highly punitive program that saw mass arrests and mass dismissals, based on multiple-choice questionnaires. In particular, cases of minor perpetrators were often dealt with quickly and with greater harshness

than those of major war criminals, who often received light sentences after a long investigation. In contrast, British authorities largely focused on a small group of major perpetrators. Survey and anecdotal evidence at the time already suggested that the US approach largely failed.

To investigate the mechanisms behind the large-scale failure of the denazification project, we collect new archival data on Allied policies to test three potential channels. First, we examine how lenient the local tribunals that decided on punishments for individuals for their Nazi past were. Lenience in this case means to what extent rulings were less strict than what the Allies thought was appropriate punishment for perpetrators of different degrees. For each county in the US zone, we gather information on the quality of denazification tribunals from a survey by the special branch in the US military responsible for denazification. We conduct a similar exercise for the British zone of occupation, using individual-level data on denazification questionnaires and subsequent tribunal decisions. Where there is evidence of lenient denazification practices, anti-Semitism disappears. Second, we look at the cooperation between the local, German-led administration and the Allied Forces. We collect information on the existence of street names that should have been renamed during denazification, but were not, in the vicinity of each municipality to capture this. There is no effect of this on the levels or persistence of anti-Semitism. Third, we explore the role of stressing "collective guilt" during denazification. We compare persistence of anti-Semitic attitudes in locations that were and that were not exposed to forced visits of concentration camps, a policy that stressed the responsibility of every German for the atrocities of the Third Reich. This does not have an effect on anti-Semitic behavior or its persistence. Overall, our findings lend support to the view that punitive justice is not an effective way to modify beliefs or to win "hearts and minds".

Research on modern-day anti-Semitism in Germany has already shed some light on the role of the past, and on factors influencing transmission. Individuals that were

exposed to Nazi ideology at school between 1933 and 1945 are still more anti-Semitic in 1996 and 2006 (Voigtländer and Voth, 2015). Anti-Semitism is also transmitted across generations; based on self-reported survey returns, for example, Jacob et al. (1999) find that right-wing Germans have twice as many grand-parents who were members of the Nazi Party or the SS.

This chapter also relates to a growing literature on the historical causes and effects of the Holocaust, and of the effects of World War II. Acemoglu et al. (2011) show that parts of Russia occupied by the Germans — where the majority of domestic Jews died -– experienced slower city-growth, and still have a greater proclivity to vote for the Communist Party. Grosfeld et al. (2011) argue that the extermination of Jews in the pale of settlement in Eastern Europe has contributed to a persistent anti-market culture. Peer effects during the Nazi period have been investigated by Waldinger (2010, 2011), who finds that the purge of German universities after 1933 lowered the quality of research amongst PhD students. Akbulut-Yuksel and Yuksel (2015) argue that expelling Jewish school teachers had major effects on the educational accomplishments of German students after the Nazi takeover.

On the theory side, our research is related to work modelling the transmission and change of cultural norms. Bisin and Verdier (2000, 2001) assume that parents choose preferences for their children that will both make them more similar to themselves but also equip them to prosper among the cultural norms prevalent in broader society. In Greif and Tadelis (2010) individuals can publicly support cultural norms, while they secretly hold on to preferences that are banned under the current political regime. If restrictions on norms are lifted, these secretly held believes quickly resurface. Acemoglu and Jackson (2014) analyze how historically evolved norms of co-operation can change through the influence of prominent agents. In Giuliano and Nunn (2017) cultural persistence arises in settings with less cross-generational instability in the surrounding environment, and cultural change arises if there is more cross-generational instability in the surrounding environment.

Relative to the existing literature, our contribution is threefold: first, in contrast to studies using self-reported evidence on anti-Semitism, we study revealed anti-Semitism by using profanations of Jewish cemeteries. This allows us to avoid the potential for bias in self-reporting. Second, we document regional persistence of an attitude that is heavily discouraged. Official policies banning anti-Semitic behavior and extensive efforts to re-educate Germans have not been able to fully eradicate the transmission of racial hatred. Third, we show under which conditions cultural norms cannot be changed by governments, and when they are malleable. Anti-Semitism is lower in areas that witnessed relatively "fair" and pragmatic denazification efforts, and persistence exists when the population perceives denazification as "unfair".

The Chapter proceeds as follows: Section 3.2 provides an overview of the history of anti-Semitism on German territory since World War I and discusses Allied denazification policies after World War II . Section 3.3 presents our data and section 3.4 the main empirical results. Section 3.5 focuses on channels. Robustness checks are summarized in section 3.6. Section 3.7 concludes.

3.2 Historical Background and Context

Anti-Semitism and anti-Semitic behavior in Germany exist at least since the Middle Ages (Cohn, 2007). In this section, we briefly summarize the history of anti-Semitism in Germany during the interwar years, denazification efforts in the British and American zone[1], and anti-Semitism after World War II.

3.2.1 Weimar Period

During World War I, anti-Semitism in Germany experienced new heights. In the course of the war, Jews were blamed for food shortages and involvement in the black market. The army ordered a census of all Jewish personnel, allegedly to counter

[1]These are the two zones the empirical analysis will be based on.

claims that few German Jews served in front-line positions. It never published the results. With the war lost, many right-wing politicians started to blame Jews (in addition to pacifists and socialists). The leading role of Jewish politicians in the revolution of 1918 fanned the flames of anti-Semitic sentiment even further.

Anti-Semitic sentiments during the Weimar Republic were expressed in a variety of forms. There were hate-speeches, pogroms, as well as several murders of prominent Jewish politicians. The number of profanations of Jewish cemeteries increased (Diamant, 1982, p. III). Many political parties campaigned with an anti-Semitic agenda (Striesow 1981). One of the most radical parties was the German National Socialist Worker's Party (NSDAP), even though it toned down the more radical anti-Semitic parts of its agenda after 1928 (Stachura 1978, Heilbronner 2004). During the final years of the Weimar Republic, Nazi paramilitary units directed violence against Jewish shopkeepers, synagogues, and cemeteries (Walter 1999).

The extent of racial hatred at the core of National Socialism only became fully visible after 1933. Starting with boycotts of Jewish establishments and the exclusion of Jewish civil servants and doctors, persecutions grew more comprehensive and vicious. As the German sphere of influence expanded after 1939, these policies eventually culminated in systematic genocide in the extermination camps of Central Eastern Europe.

3.2.2 Denazification

After World War II, Germany was divided into four occupation zones (American, British, French, and Soviet). In each zone, the occupation powers engaged in *denazification*. Denazification was a set of different practices aimed at re-educating Germans, and at removing National Socialist ideology. Implementation in each zone differed, but attitudes towards Jews took center stage in the Western occupation zones. When the denazification program came to an end, millions of cases had been

processed and some two million Germans were punished (Biddiscombe, 2007).

In the American zone, the denazification program was comprehensive and highly punitive – in total, a quarter of the population living under American occupation was affected (Teschke 2001). Many Germans were forced to visit concentration camps, or to attend public viewings of films showing the horrors of the Holocaust (Judt, 2005). In 1946, local tribunals under German administration (*Spruchkammern*) were established. While initially denazification started resolutely, practical concerns and social pressure slowed the process thereafter (Herz, 1948). Since the most important cases were left for last, perpetrators of smaller crimes were often punished quickly and severely, while many important Nazis escaped punishment altogether or were only mildly punished.

Initial German enthusiasm for the process quickly gave way to scepticism amid complaints about unfairness. A popular joke at that time went "What is the difference between a *Spruchkammer* and a fish-net? A fish-net catches the big ones, and lets the little ones get away!" (Taylor, 2011, p. 292). In the American zone, German support for denazification dropped from 57 percent in 1946 to 17 percent by 1949. In some areas, there was no co-operation of the German administration with the process whatsoever. In the rural Bavarian community of Wolfratshausen, where 8,000 of the 40,000 inhabitants had been Nazi party members, the conservative Landrat (district head) and his associate stamped every single questionnaire with the words "nothing prejudicial known" (Biddiscombe, 2007, "nichts Nachteiliges bekannt"). The program was also regarded as a failure by the American military government. The Jewish Adviser to the American Military Governor, Rabbi Philip Bernstein, summed this up when he said in 1947: "If the United States Army were to withdraw tomorrow, there would be pogroms on the following day" (quoted in Ferguson, 2015, p. 196).

The British Control Commission for Germany (CCG) considered the American approach of mass arrests and massive re-education as impractical and counter-

productive (Teschke 2001). The British focused on removing powerful Nazi party members, minimizing dismissals in a bid to balance practicality and justice. By 1946, the British turned the process over to German denazification panels. These processed 2 million questionnaires (in a population of 22 million). Judgments became milder the more control was ceded to lower-level German local tribunals. While some historians have been sceptical of the British approach, it bred less resentment than the American one – in 1946, the German public generally urged greater rigor and comprehensiveness (Turner, 1989). This shows the pragmatic and limited nature of British efforts at denazification, as well as the significant public support the program enjoyed.

3.2.3 Anti-Semitism after Denazification

The overall effects of denazification have been difficult to assess. The American military government immediately began conducting surveys, and estimated in 1946 that almost 40% of Germans were anti-Semites; of these, close to half were labelled "hard-core". A study in 1948 found similar values (Bergmann and Erb, 1997). German surveys in the early 1950s also put the proportion of anti-Semites at approximately a third of the population. Later studies found similar proportions all the way into the 1970s (Silbermann, 1982). The 1980s saw an increasing tendency to discuss the need for Schlußstrich — drawing a line under discussion of the past and Germany's historical guilt for the Holocaust. By 1989, in West-Germany, one survey classified only 46% of the population as not anti-Semitic, and put the proportion of those with extreme or significant negative views of Jews at 14% (Emnid, 1989).

With the fall of the Berlin Wall and the collapse of East Germany, public attention focused on anti-democratic and violent tendencies in the former East.[2] Neo-Nazi

[2]The Soviet zone of occupation covered East Germany. The Soviet authorities were primarily concerned with establishing Communist administrative control. Denazification was secondary. Nazi members were dismissed from administrative positions, but party members without major crimes on their record were encouraged to join the Communists. In general, the Soviet approach is considered "relatively tough denazification" (Biddiscombe, 2007). The process had some degree

violence against foreigners is a particular concern. Overall, radical right-wing parties have had only limited success in post-war Germany. In the late 1960s, the NPD (Nationaldemokratische Partei – National Democratic Party) won seats in regional elections; it never reached the required 5 percent of the vote in federal elections (Bromba and Edelstein, 2001). Since 2004, it is represented in the Saxon diet, and receives a measure of support in most areas of East Germany. In 2011, new information revealed that a string of murders in Germany since 2000 had been perpetrated by a terror group called Nationalsozialistischer Untergrund (National Socialist Underground). Several of those implicated in these crimes were close to the NPD.

3.3 Data

We use profanations of Jewish cemeteries as a measure for the local existence of severe anti-Semitism. While offenders are not always known, most profanations in the time frame of our study were carried out by anti-Semitic groups (Diamant, 1982, p. III). We collect information on Jewish cemeteries and their profanations from a handbook on Jewish cemeteries (Diamant, 1982), which lists all known Jewish cemeteries in West Germany, based on Jewish handbooks from 1918 to 1933 and newer regional sources. The book also includes all known profanations until 1981.

We collected data on the effectiveness of denazification activities at the local level. From the records of the Office of the Military Government in the US Zone (OMGUS), we obtained detailed reports on the state of denazification activities. These were compiled by the Special Branch of the US Army, which supervised the process. Each location received a county-level score on a scale from 1 (poor) to 6 (superior). Based on the scores given in 1946, we generate a dummy for districts that were

of legitimacy because high-ranking, tainted officials, especially judges, were removed thoroughly; communists tended to cooperate with denazification. At the same time, the harsh behavior of Soviet troops towards civilians in 1945 undermined the population's support for policies of the occupying power, and it was considered by many as a form of victor's justice.

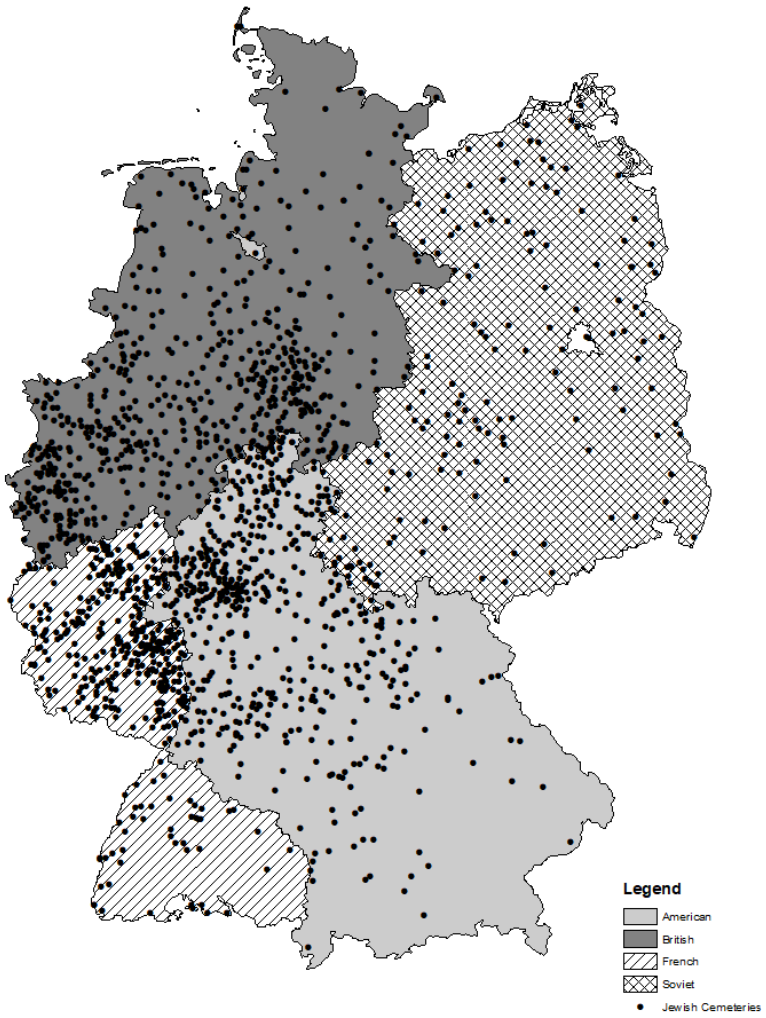


Figure 3.1: Cemeteries and Occupation Zones in Germany

Note This map of Germany in its current borders depicts the four allied occupation zones after World War II and the location of Jewish cemeteries. Data on cemeteries in the Soviet zone are likely to be incomplete. The data on Jewish cemeteries is from Diamant (1982).

rated as having *poor*, *fair* or *satisfactory* denazification activities. Worse scores are associated with more lenient rulings. For example, in a report from 1947 on the district of Büdingen, which received a *poor* score it is stated that party members that should have been categorized as offenders were classified as mere *followers*, i.e. classified in the denazification category IV:

"To my question of why so many small office holders are found in category IV they answered that the Tribunals don't consider the formal incrimination of a respondent as a real incriminantion. If the PP's (public prosecutors, authors' note) *can't enumerate specific charges such as manhandling, denunciation etc. the formal charges fall by the wayside."*[3]

Another example comes from the district Ziegenhain, which also received a *poor* score. The Denazification Branch of the Civil Administration Division notes

"Generally the case decisions favour the defendant. Charges brought by witnesses and exonerating circumstances are given major priority"[4]

For the British zone, no elaborate system of repeated assessments on the US model existed. Tribunals staffed by British authorities assessed a much smaller number of cases, predominantly involving public sector employees; it made decisions quickly and in a relatively uniform fashion. To assess local variations in the nature of decision-making, we use 748 denazification files of employees of the Labor and Social Insurance Office. For each employee, we know their function and their involvement with various NS organizations including their duration and the person's rank, as well as the final decision rendered by the British authorities. To construct a measure

[3]For the original document, see Figure B.2 in Appendix B (Source: Hessisches Staatsarchiv Darmstadt, Q 4 Nr. 8/78-2/6, p. 16).

[4]For the original document, see Figure B.3 in Appendix B (Source: Hessisches Staatsarchiv Darmstadt, Q 4 Nr. 8/78-3/14, p. 106).

of local variation, we compare actual decisions with predicted ones (based on the observed organization membership(s) and function). Counties where the local tribunal systematically imposed more lenient penalties than would be expected given the observables are classified as "lenient denazification".

To measure the cooperation of local elites with the denazification policies of the occupation forces, we consider street names in 2017.[5] During the Nazi period, many streets were (re)named in line with Nazi ideology, for example after war heroes, important figures of the NSDAP, or Hitler himself. During the denazification program, these streets had to be renamed by local German authorities (Azaryahu, 1990, 2012). We regard the existence of street names that should have been renamed, but were not, as a proxy for absence of cooperation by the German local administration. In particular, we examine streets that were named after *Braunau* (Hitler's birthplace), *Erwin Rommel* (a German general), and *Hindenburg* (former President of the Weimar Republic who appointed Hitler as Chancellor).[6] Data on street names are based on maps in OpenStreetMap.[7]

To test whether differences in persistence between zones are driven by a different emphasis on collective guilt during denazification, we look at one particular policy that is likely to stress this: forced visits of concentration camps. When concentration camps on German soil were liberated, outraged Allied commanders often forced the German population to visit the camps. What began as spontaneous actions by low-level army commanders – mainly in the US zone of occupation – became a systematic element of re-education programs. Visits were intended to shock and create feelings of guilt. They were seen as "dramatic sites for some of the earliest accusations of German collective guilt for the war, Nazism and what we now call the Holocaust"

[5]For the use of street names in quantitative analysis refer to Oto-Peralías (2017).

[6]Street names that contained Hindenburg were to be renamed according to the occupation powers. This is for example stressed by the Ministry of Interior of Northrhine Westphalia, see Figure B.1 in Appendix B (Source: Stadtarchiv Münster, Amt 47, Nr. 3).

[7]The streets were collected by Geofabrik and made available by the German newspaper *Die Zeit* at https://www.zeit.de/feature/strassenverzeichnis-strassennamen-herkunft-deutschland-infografik (last visited February 14th, 2019). The dataset includes all streets that were included in the online maps on October 10th, 2017.

(Mauriello, 2017, p. 27). Many Germans resented the practice. As contemporaries recall, they could be made to attend, but not to watch or engage (Judt 2005). To the best of our knowledge, there is no comprehensive dataset on forced camp visits. However, we do know that camp visits were forced on local populations; given logistical conditions after 1945, lifting people who lived at distances greater than 30 km was not feasible. Instead of using actual visits, we simply use potential visits to known sites of concentration camps, indicated by a camp being located within 30 km.

Election results and socio-economic statistics are from Falter and Hänisch (1990), who digitized statistics from the interwar period. This data is available for all municipalities with more than 2,000 inhabitants, or as the average value for all municipalities with less than 2,000 inhabitants in a county.

3.4 Persistence

To analyze persistence of profanations of Jewish cemeteries in the American and British occupation zone, we estimate the following linear probability model

$$\begin{aligned} Profanation_i = {} & \beta_1 American + \beta_2 ProfanationWeimarRepublic_i \\ & + \beta_3 ProfanationWeimarRepublic_i \times American_i \\ & + \gamma Controls_i + \epsilon_i \end{aligned} \tag{3.1}$$

where $Profanation_i$ is a dummy for profanations of Jewish cemeteries between 1946 and 1981 in municipality i that has a Jewish cemetery. In additional regressions, we use the natural logarithm of one plus the number of profanations as the dependent variable. $ProfanationWeimarRepublic_i$ is a dummy for the occurrence of at least one profanation of a Jewish cemetery during the Weimar Republic. We limit our analysis to places in which Jewish cemeteries exist. *American* is a dummy for

all municipalities that belonged to the American zone of occupation after WW II (for the extent of the American zone refer to Figure 3.1). β_2 and $\beta_2 + \beta_3$ measure the persistence of extreme anti-Semitism in the British and American sector respectively. *Controls* are the natural logarithm of the number of cemeteries, the share of protestants as well as the share of Jews in 1925, and the natural logarithm of population in 1920 in municipality i.[8] ϵ_i are robust standard errors.

Table 3.1: Persistence of Anti-Semitism

	Profanations after WW II			
	Dummy	ln Number	Dummy	ln Number
	(1)	(2)	(3)	(4)
American	-0.012	-0.008	0.030	0.030
	(0.035)	(0.031)	(0.128)	(0.114)
Profanation Weimar Republic	-0.119*	-0.087		
	(0.066)	(0.059)		
Profanation Weimar Republic	0.276**	0.213**		
× American	(0.123)	(0.107)		
Profanation Nazi period			-0.008	-0.013
			(0.076)	(0.062)
Profanation Nazi period			-0.024	-0.024
× American			(0.131)	(0.116)
Controls	✓	✓	✓	✓
Observations	874	874	874	874
R-squared	0.089	0.150	0.083	0.146

Note This table presents results of regression 3.1. The dependent variable in columns 1 and 3 is a dummy for the existence of any profanations after World War II. The dependent variable in columns 2 and 4 is the natural logarithm of one plus the number of profanations. Controls are the natural logarithm of the number of cemeteries, share protestants 1925, share jews 1925 and natural logarithm of population in 1920. Data sources see text. Robust standard errors reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

Results are presented in Table 3.1. Profanations are persistent in the American occupation zone; municipalities with profanations during the Weimar Republic are more likely to experience profanations after 1945. Interpreting the model as a linear probability model, the probability of having a profanation after 1945 is around 15 percentage points higher for municipalities in the former US zone in which Jewish

[8]If socio-economic data from the time of the Weimar Republic are not available at the municipality level, we assign the average value of all municipalities in the corresponding county for which no municipality level data is available.

cemeteries were profaned during the Weimar Republic compared to municipalities in the US zone without profanations (column 1). The number of profanations is around 13 percent higher in American municipalities with a profanation during the Weimar Republic than in all other municipalities (column 2). There is no persistence of profanations in the British zone of occupation. In columns 3 and 4 we link profanations after World War II to profanations during the Nazi period from 1933 to 1945. During the Nazi period, anti-Semitic behavior was seen favorably, and in 93 percent of municipalities the Jewish cemetery was profaned. Thus, we would expect profanations during this time to be driven less by extreme forms of local anti-Semitism compared to profanations during the Weimar Republic, when profanations were officially sanctioned. In line with this, we find that there is no link between profanations during the Nazi era and the post war era (columns 3 and 4). This is in line with the interpretation that profanations during the Weimar period capture local differences in anti-Semitism.

3.5 Channels

What explains the persistence of extreme anti-Semitism in the American and the lack of persistence in the British sector? We examine the role of three potential differences in the denazification process in the American and British sector that might drive this. First, individuals were punished more severely in the American sector in the course of individual level denazifiction in denazification tribunals. Germans felt that the American denazification was stricter when dealing with "average" Germans than when dealing with former Nazi elites, and thus considered the process as inherently unfair. This might have influenced the ability of Americans to influence anti-Semitic preferences, and could have led to the observed failure of re-education efforts. Second, it could be the case that there were differences in how well the American and the British occupation government worked together with

local German authorities, which carried out many of the denazification policies. If cooperation was worse in the American sector, and denazification was thus implemented to a lesser extent at the local level in the American sector, this could explain the persistence of anti-Semitic attitudes in the American sector. Third, the American occupation government put more emphasis on the concept of collective guilt in their denazification strategy, i.e. it was much more vehement in stressing that all Germans collectively were responsible for the horrible crimes committed in the Third Reich. Germans resented this, and if they were thus unwilling to take part in the denazification efforts this could explain the observed pattern.

3.5.1 Lenience of the Denazification Tribunals

First, we turn to within zone variation of the working of the civilian courts. We estimate model 3.1, additionally controlling for a dummy capturing lenience of denazification within a district in the American and British sector, which indicates whether German perpetrators were punished less severely than official guideline by the Allies recommended. Lenient denazification in the American zone is defined as scoring *poor*, *lenient* or *satisfactory* on a rating of the quality of denazification conducted by the US military in 1946. In the British zone, lenient denazification is having less strict verdicts in denazification tribunals than would be expected given individual's memberships in Nazi organizations. Results are presented in Table 3.2, where columns 1 and 5 present the results of the baseline model for comparison. Lenient tribunals are associated with a 16 percentage points lower probability of having any profanations after 1945 (column 2) or 12 percent fewer profanations after 1945 (column 5). This effect is the same for municipalities with and without profanations in the past. In addition, once we control for lenience, there is no longer any persistence of profanations in the American (or British) sector. Differences between the American and British sector thus seem to be the result of the harsh rulings of local tribunals in the American sector. This finding is in line with the

Table 3.2: Potential Channels

	Profanations after World War II							
	Dummy Profanations				ln (1 + Profanations)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
American	-0.012	0.038	-0.012	-0.008	-0.008	0.041	-0.018	-0.003
	(0.035)	(0.062)	(0.035)	(0.036)	(0.031)	(0.050)	(0.031)	(0.032)
Profanation Weimar Republic	-0.119*	0.011	-0.106	-0.125	-0.087	-0.024	-0.073	-0.104
	(0.066)	(0.200)	(0.092)	(0.079)	(0.059)	(0.156)	(0.078)	(0.067)
Profanation Weimar Republic × American	0.276**	0.115	0.276**	0.272**	0.213**	0.093	0.221**	0.210*
	(0.123)	(0.191)	(0.123)	(0.123)	(0.107)	(0.145)	(0.105)	(0.108)
Lenient Denazification		-0.162**				-0.119**		
		(0.064)				(0.057)		
Lenient Denazification × Weimar Republic		0.011				0.009		
		(0.202)				(0.154)		
Nazi Street Names			0.003				0.004	
			(0.032)				(0.029)	
Nazi Street Names × Weimar Republic			-0.028				-0.042	
			(0.114)				(0.100)	
Forced Camp Visits				-0.024				-0.029
				(0.034)				(0.030)
Forced Camp Visits × Weimar Republic				0.028				0.066
				(0.123)				(0.119)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.089	0.088	0.089	0.089	0.150	0.130	0.167	0.151
Observations	874	438	874	874	874	438	874	874

Note This table presents results of regression 3.1 with additional controls. Controls that are not shown are the natural logarithm of the number of cemeteries, share protestants 1925, share jews 1925 and natural logarithm of population in 1920. Data sources see text. Robust standard errors reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

qualitative assessment of denazification policies in the US sector, and the reasons for their limited success.

3.5.2 Cooperation of Local Authorities

Denazification effectiveness was also subject to the cooperation of local authorities with the Allied forces; differences in the extent to which American and British officials were able to work with local German authorities could drive the observed differences in persistence. We proxy the willingness of local authorities to foster denazification by the existence of street names in 2017 that reflect Nazi ideology, and that should have been renamed according to denazification directives, i.e. a dummy for the existence of any Hindenburg, Braunau or Rommel streets within 10 km of municipalities.[9] We control for the number of streets within 10 km of the center of municipality i. Standard errors account for heteroskedasticity.

Lack of cooperation from local authorities does not explain persistence of profanations in the American sector; the coefficient of profanations during the Weimar Republic interacted with a dummy for the American sector remains unchanged (Table 3.2, columns 3 and column 7). Controlling for the existence of street names that were not changed does not influence the pattern of persistence in the US sector. The coefficients of the baseline models remain unaffected. There is thus no evidence suggesting that denazification in the US zone was less successful than in the British zone, because of a lack in cooperation by the local administration in the former zone.

3.5.3 Collective Guilt

The third potential channel we test is the role of collective guilt by examining within zone variation of forced concentration camp visits. Forced concentration camp vis-

[9]Shapefiles for municipalities borders after 1945 are not available, thus we refer to this measure.

its were seen as a way to establish the feeling of collective guilt among Germans. The American denazification policy put more emphasis on establishing this feeling of collective guilt than the British policy. If this explains the difference between the American and British zone, we would expect that places that were exposed to forced camp visits in both zones to demonstrate higher levels of persistence of anti-Semitism. No comprehensive dataset on forced camp visits exists, thus we measure the intention to treat effect by looking at the existence of a concentration camp with forced visits within a 30 km radius. We interact this with *American* to allow the effect of forced camp visits to vary between the two occupation zones.

Forced visits to concentration camps do not affect the existence or number of profanations in the American or British occupation zone (see Table 3.2, columns 4 and 8). They also do not affect the persistence of historic profanations in the American sector; the coefficient of profanations in the American sector and its standard errors do not change when we control for forced visits. This suggests that the different emphasis on collective guilt in the American and British sector does not drive the differences in persistence of anti-Semitism between the two sectors.

3.6 Robustness

In our analysis, we have used a linear probability model to look at the existence of any profanation as an outcome. However, a disadvantage of such models is that they do not take into account that an event can either occur or not, and thus predict probabilites below zero or above one. To account for this, we repeat the analysis using Probit and Logit models which take the nature of the binary outcome variable into account. Table 3.3 shows that results on persistence and channels also hold when estimating a probit model (columns 1 to 4) or a logit model (columns 5 to 8). The magnitude of the effects is also similar. For the baseline effect, the predicted

Table 3.3: Robustness: Non-linear Model, Marginal Effects

	Dummy profanations							
	Probit				Logit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
American	-0.011	0.036	-0.072*	-0.008	-0.011	0.036	-0.071*	-0.007
	(0.035)	(0.060)	(0.042)	(0.035)	(0.035)	(0.060)	(0.042)	(0.036)
Profanation Weimar Republic	-0.119	0.009	-0.107	-0.121	-0.111	0.004	-0.096	-0.114
	(0.080)	(0.193)	(0.077)	(0.092)	(0.081)	(0.184)	(0.077)	(0.094)
Profanation Weimar Republic × American	0.258**	0.101	0.246**	0.254**	0.247**	0.097	0.232**	0.243**
	(0.117)	(0.176)	(0.113)	(0.117)	(0.115)	(0.170)	(0.110)	(0.116)
Lenient Denazification		-0.145***				-0.143***		
		(0.054)				(0.051)		
Lenient Denazification × American		0.013				0.021		
		(0.179)				(0.172)		
Nazi Street Names			-0.090**				-0.092**	
			(0.044)				(0.045)	
Nazi Street Names × American			0.128**				0.128**	
			(0.059)				(0.060)	
Forced Camp Visits				-0.024				-0.025
				(0.035)				(0.035)
Forced Camp Visits × American				0.015				0.019
				(0.127)				(0.126)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	874	438	874	874	874	438	874	874

Note This table presents results of regression 3.1 using a logit (columns 1 to 4) or probit (columns 5 to 8) regression instead of OLS. Controls that are not shown are the natural logarithm of the number of cemeteries, share protestants 1925, share jews 1925 and natural logarithm of population in 1920. Columns 3 and 6 include the natural logarithm of the number of streets within a 10km radius as an additional control. Data sources see text. Robust standard errors reported in parentheses. *, **, and *** denote significance on the 10 percent, 5 per cent, and 1 percent level, respectively.

increase in the probability of a profanation for the average municipality[10] in which a Jewish cemetery was profaned during the Weimar period and that lies in the American sector is around 26 percentage points in the probit (column 1) and 25 percentage points in the logit specification (column 5), compared to 28 percentage points in the linear probability model. Coefficients for the regressions exploring the channels of the persistence are also nearly identical in size and magnitude in the probit and logit model compared to the linear probability model. Again, we find that once we account for the lenience of the rulings of the local tribunals, there is no longer persistence of anti-Semitism at the municipality level in the American sector.

3.7 Conclusion

Hostility towards outsiders has been common in human societies for millennia (Choi and Bowles, 2007; Alexander and Christia, 2011; Bernhard et al., 2006). Theories that seek to explain this hostility often emphasize the importance of interactions with the minority group. For example, many interpretations of lingering racism in the United States have emphasized repeated interactions between whites and African-Americans (Blalock, 1967). Alternatively, scholars have emphasized indirect benefits for groups such as enhanced co-operation and trust within networks of insiders (Bowles and Gintis, 2004; Choi and Bowles, 2007; Henrich et al., 2001). Economic explanations also focus on personal gains for "hate entrepreneurs" (Glaeser, 2005). These explanations of out-group discrimination are predicated on the object of animosity being present.

We focus on an environment where the object of animosity – Jews – was notably absent after 1945. Our study demonstrates the persistence of racial prejudices at the local level in the American zone, and across one of the greatest discontinuities in recorded history. The Allied forces implemented massive programs of denazification

[10]By average municipality we mean, a municipality for which all other variables take on the mean values that are found for the entire sample.

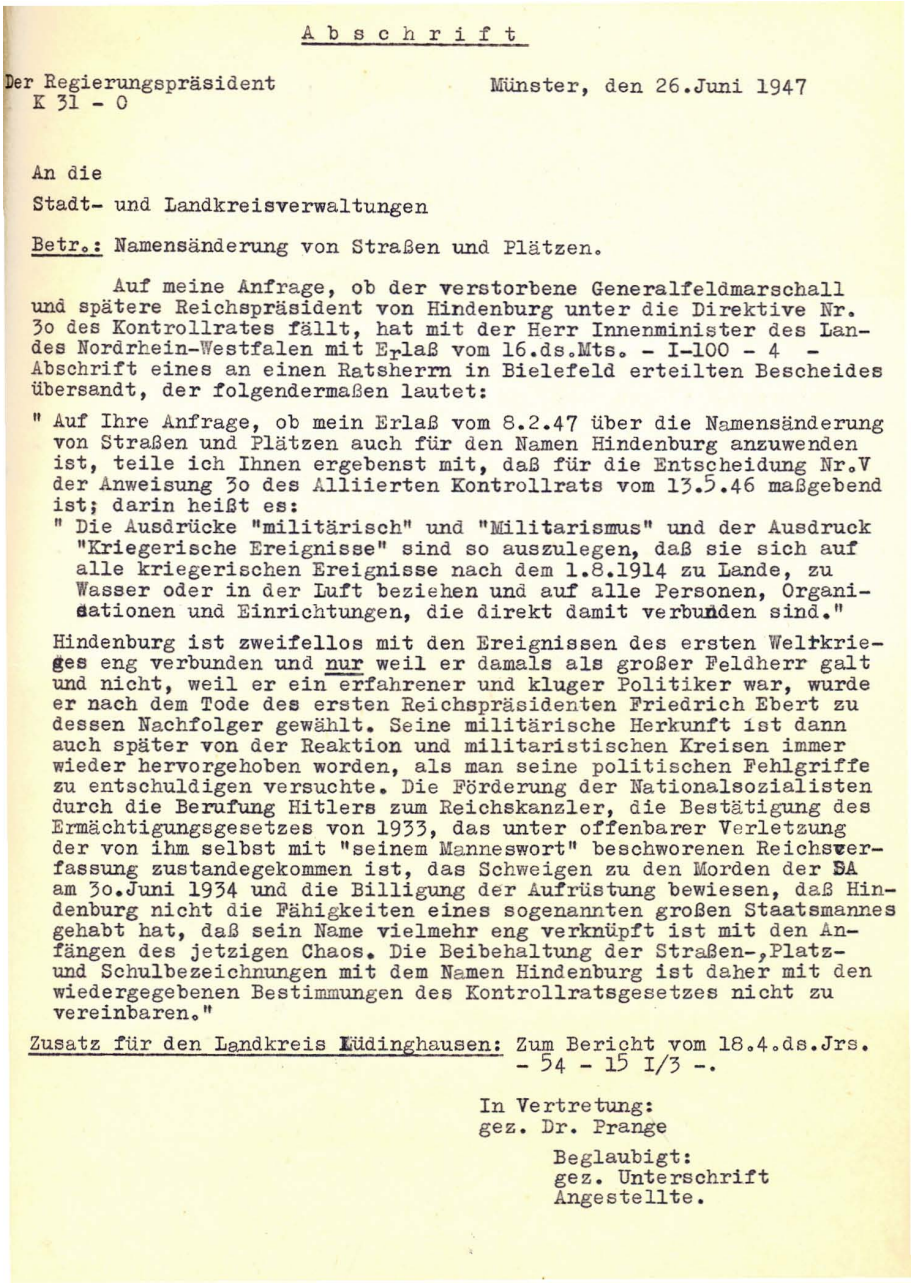
that varied across regions. In the US zone, which pursued a highly ambitious, highly bureaucratic, and highly punitive approach, there is evidence of persistence of anti-Semitism. The US approach to denazification is widely considered as a failure (Herz, 1948). It was overly ambitious, and inconsistently implemented – especially the rapid and harsh punishment of low-level officials, while higher-ranking perpetrators escaped lightly – undermined the program's credibility and perceived fairness. The US zone registers markedly higher rates of Jew-hatred today, even after controlling for pre-existing historical differences. In contrast, the British denazification was relatively limited in scope, and is generally described as pragmatic (Biddiscombe, 2007). It focused on high-ranking officials involved in major crimes, and made removing them a priority. This policy enjoyed wider support among the public. In the British zone of occupation there is no evidence for persistence of historical anti-Semitism.

To examine the mechanisms behind the striking differences in persistence further, we compile measures of within-zone variation in policies. We find that more lenience in tribunal decisions during the occupation is systematically associated with lower levels of anti-Semitism today and explains the persistence of anti-Semitism in the American zone. Second, we find no evidence that cooperation of local authorities or additional punitive policies, such as forced camp visits, had an effect on attitudes.

Our findings relate to an important strand in the literature on cultural economics – the making of "oppositional identity". In models where agents derive utility from holding particular beliefs, threatening them can lead to a backlash — an overinvestment in the trait that is under attack. While the theoretical literature describes how such a pattern could arise (Bénabou and Tirole, 2011; Bisin and Verdier, 2000) our study is one of the first to provide empirical evidence for an adverse reaction. To the best of our knowledge, Fouka (2016) is the only other paper that makes a related argument in the context of language policies in the US after 1918.

B Appendix to Chapter 3

B.1 Figures



Abschrift

Der Regierungspräsident
K 31 - 0

Münster, den 26.Juni 1947

An die

Stadt- und Landkreisverwaltungen

Betr.: Namensänderung von Straßen und Plätzen.

Auf meine Anfrage, ob der verstorbene Generalfeldmarschall und spätere Reichspräsident von Hindenburg unter die Direktive Nr. 3o des Kontrollrates fällt, hat mit der Herr Innenminister des Landes Nordrhein-Westfalen mit Erlaß vom 16.ds.Mts. - I-100 - 4 - Abschrift eines an einen Ratsherrn in Bielefeld erteilten Bescheides übersandt, der folgendermaßen lautet:

" Auf Ihre Anfrage, ob mein Erlaß vom 8.2.47 über die Namensänderung von Straßen und Plätzen auch für den Namen Hindenburg anzuwenden ist, teile ich Ihnen ergebenst mit, daß für die Entscheidung Nr.V der Anweisung 3o des Alliierten Kontrollrats vom 13.5.46 maßgebend ist; darin heißt es:
" Die Ausdrücke "militärisch" und "Militarismus" und der Ausdruck "Kriegerische Ereignisse" sind so auszulegen, daß sie sich auf alle kriegerischen Ereignisse nach dem 1.8.1914 zu Lande, zu Wasser oder in der Luft beziehen und auf alle Personen, Organisationen und Einrichtungen, die direkt damit verbunden sind."

Hindenburg ist zweifellos mit den Ereignissen des ersten Weltkrieges eng verbunden und nur weil er damals als großer Feldherr galt und nicht, weil er ein erfahrener und kluger Politiker war, wurde er nach dem Tode des ersten Reichspräsidenten Friedrich Ebert zu dessen Nachfolger gewählt. Seine militärische Herkunft ist dann auch später von der Reaktion und militaristischen Kreisen immer wieder hervorgehoben worden, als man seine politischen Fehlgriffe zu entschuldigen versuchte. Die Förderung der Nationalsozialisten durch die Berufung Hitlers zum Reichskanzler, die Bestätigung des Ermächtigungsgesetzes von 1933, das unter offenbarer Verletzung der von ihm selbst mit "seinem Manneswort" beschworenen Reichsverfassung zustandegekommen ist, das Schweigen zu den Morden der SA am 3o.Juni 1934 und die Billigung der Aufrüstung bewiesen, daß Hindenburg nicht die Fähigkeiten eines sogenannten großen Staatsmannes gehabt hat, daß sein Name vielmehr eng verknüpft ist mit den Anfängen des jetzigen Chaos. Die Beibehaltung der Straßen-,Platz- und Schulbezeichnungen mit dem Namen Hindenburg ist daher mit den wiedergegebenen Bestimmungen des Kontrollratsgesetzes nicht zu vereinbaren."

Zusatz für den Landkreis Lüdinghausen: Zum Bericht vom 18.4.ds.Jrs. - 54 - 15 I/3 -.

In Vertretung:
gez. Dr. Prange

Beglaubigt:
gez. Unterschrift
Angestellte.

Figure B.1: Directive on Renaming of Hindenburg Streets
Note: Directive by the Ministry of Interior of Northrhine Westphalia on the renaming of streets named after *Hindenburg* (Source: Stadtarchiv Münster, Amt 47, Nr. 3).

File Buedingen

OFFICE OF MILITARY GOVERNMENT FOR HESSE
LIAISON & SECURITY OFFICE
SK — LK FULDA
APO 633, US ARMY

FULDA (H-3818)
24 Oct 47

Report on Spruchkammer Budingen

Called in only the PP's this first meeting. There are 4 PP's and 5 Chairmen over which incidently, they complained. They can't turn out the quantity in quality they say, and keep the Chairmen busy. Have written to Knappstein about it without result so far.

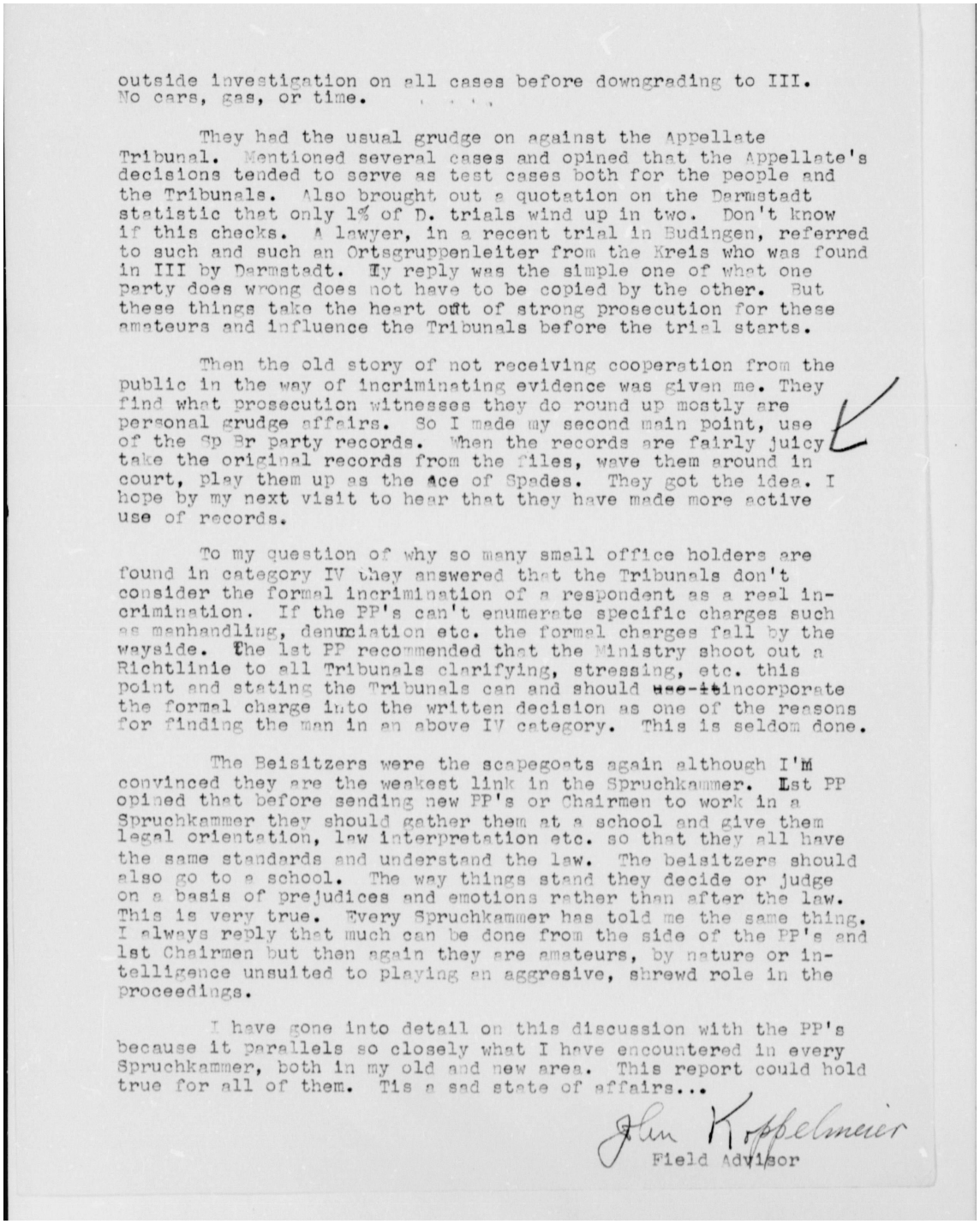
I harped, between rounds of cigarettes, on mainly two points. Other points I figured hopeless at least at this time. One was the writing of Klageshriften in a style that was pretty good defense pieces. Dissected examples of what they had written. They got around to admitting that they noticed that their style had assumed a defense trait. The 1st PP avowed they would change and pattern themselves closer to what I expounded.

The reasons they had slipped into such a style they explained were the following. First they considered the jump from a Cl II Ch to the usual IV decision too big. Secondly, if they charged most of the inevitable IV cases with II the Kammer would not take their II charges seriously when they really wanted to see someone wind up in II. Thirdly, the Ministry had instructed them thru model Klageschriften and Amtsblatt directions to justify specifically a downgraded charge from II to III and to clearly state, as another instance, when they believed the person should fall under an Amnesty (Amtsblatt 35). Fourthly, they assumed they had to justify in detail their downgraded charge for the benefit of the local Sp Br.

I answered with a long spiel on their role of PP, on stressing the incrimination, twisting sentences properly etc. since the Tribunal would be giving undue weight to the defense points as it is. Also the Sp Br could not use such charges as they were writing them, for either evaluating the case or ascertaining if they were correctly fulfilling their duties as PP. Got them to agree that when they specify their reasons for downgrading where it seemed justified the whole charge was diluted to where a decision of IV by the Tribunal was automatic. And so forth and so on.

Further explained why and how to rip apart such false defense points as church membership which they claimed invariably was accepted by the beisitzers. Get in there and pitch line.

Biggest majority of cases there have been charged in III. The automatic PC account blocking; PP's opinion that the man does not really belong in II; and the above mentioned reasons account for this unwillingness to charge in II. Their investigators stink in their estimation. Furthermore, they cannot possibly make an



outside investigation on all cases before downgrading to III. No cars, gas, or time.

They had the usual grudge on against the Appellate Tribunal. Mentioned several cases and opined that the Appellate's decisions tended to serve as test cases both for the people and the Tribunals. Also brought out a quotation on the Darmstadt statistic that only 1% of D. trials wind up in two. Don't know if this checks. A lawyer, in a recent trial in Budingen, referred to such and such an Ortsgruppenleiter from the Kreis who was found in III by Darmstadt. My reply was the simple one of what one party does wrong does not have to be copied by the other. But these things take the heart out of strong prosecution for these amateurs and influence the Tribunals before the trial starts.

Then the old story of not receiving cooperation from the public in the way of incriminating evidence was given me. They find what prosecution witnesses they do round up mostly are personal grudge affairs. So I made my second main point, use of the Sp Br party records. When the records are fairly juicy take the original records from the files, wave them around in court, play them up as the Ace of Spades. They got the idea. I hope by my next visit to hear that they have made more active use of records.

To my question of why so many small office holders are found in category IV they answered that the Tribunals don't consider the formal incrimination of a respondent as a real incrimination. If the PP's can't enumerate specific charges such as manhandling, denunciation etc. the formal charges fall by the wayside. The 1st PP recommended that the Ministry shoot out a Richtlinie to all Tribunals clarifying, stressing, etc. this point and stating the Tribunals can and should ~~use it~~ incorporate the formal charge into the written decision as one of the reasons for finding the man in an above IV category. This is seldom done.

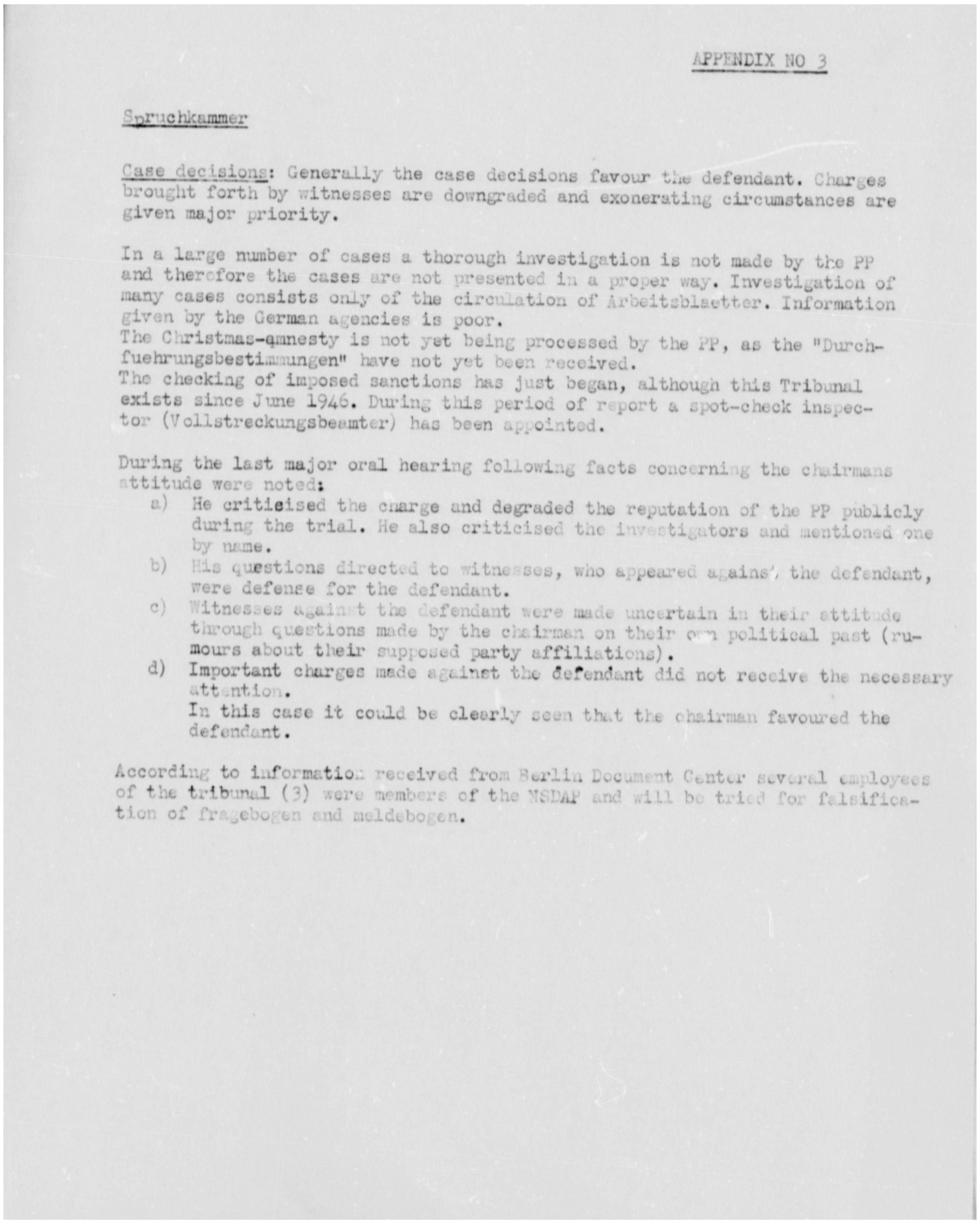
The Beisitzers were the scapegoats again although I'm convinced they are the weakest link in the Spruchkammer. 1st PP opined that before sending new PP's or Chairmen to work in a Spruchkammer they should gather them at a school and give them legal orientation, law interpretation etc. so that they all have the same standards and understand the law. The beisitzers should also go to a school. The way things stand they decide or judge on a basis of prejudices and emotions rather than after the law. This is very true. Every Spruchkammer has told me the same thing. I always reply that much can be done from the side of the PP's and 1st Chairmen but then again they are amateurs, by nature or intelligence unsuited to playing an aggresive, shrewd role in the proceedings.

I have gone into detail on this discussion with the PP's because it parallels so closely what I have encountered in every Spruchkammer, both in my old and new area. This report could hold true for all of them. Tis a sad state of affairs...

John Koppelmeier
Field Advisor

Figure B.2: Report on Local Tribunal in Büdingen

Note: Report by the US military government for Hesse on the local tribunal in Büdingen (Source: Hessisches Staatsarchiv Darmstadt, Q 4 Nr. 8/78-2/6, p. 16).



APPENDIX NO 3

Spruchkammer

Case decisions: Generally the case decisions favour the defendant. Charges brought forth by witnesses are downgraded and exonerating circumstances are given major priority.

In a large number of cases a thorough investigation is not made by the PP and therefore the cases are not presented in a proper way. Investigation of many cases consists only of the circulation of Arbeitsblaetter. Information given by the German agencies is poor.
The Christmas-amnesty is not yet being processed by the PP, as the "Durchfuehrungsbestimmungen" have not yet been received.
The checking of imposed sanctions has just began, although this Tribunal exists since June 1946. During this period of report a spot-check inspector (Vollstreckungsbeamter) has been appointed.

During the last major oral hearing following facts concerning the chairmans attitude were noted:

a) He criticised the charge and degraded the reputation of the PP publicly during the trial. He also criticised the investigators and mentioned one by name.
b) His questions directed to witnesses, who appeared against the defendant, were defense for the defendant.
c) Witnesses against the defendant were made uncertain in their attitude through questions made by the chairman on their own political past (rumours about their supposed party affiliations).
d) Important charges made against the defendant did not receive the necessary attention.
In this case it could be clearly seen that the chairman favoured the defendant.

According to information received from Berlin Document Center several employees of the tribunal (3) were members of the NSDAP and will be tried for falsification of fragebogen and meldebogen.

Figure B.3: Report on Local Tribunal in Ziegenhain
Note: Report by the US military government for Hesse on the local tribunal in Ziegenhain (Source: Hessisches Staatsarchiv Darmstadt, Q 4 Nr. 8/78-3/14, p. 106).

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Eidesstattliche Versicherung

Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht. Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht. Sofern ein Teil der Arbeit aus bereits veröffentlichten Papers besteht, habe ich dies ausdrücklich angegeben.

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