UNIVERSITY OF CALIFORNIA,
IRVINE

Institutions, Infrastructure, and Development: Essays in German Economic History

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Paul Thomas Erich Lowood

Dissertation Committee:
Professor Daniel E Bogart, Chair
Professor Stergios Skaperdas
Associate Professor Vellore Arthi

2025

DEDICATION

To the workers of the world, past and present, for building and maintaining an environment where scholars can thrive. And above all to Mama and Papa, for their unconditional love and tireless support.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGMENTS

First, I would like to thank my dissertation committee for their time and support.

I am especially thankful for the guidance of my committee chair. Dan, Thank you for helping me turn my interests into a real research agenda.

Stergios, I am not sure if I would have continued with economics if I had not taken your Political Economy seminar as an undergraduate so many years ago. Thank you for introducing me to a new way of thinking.

I am also thankful to the Center for Global Peace and Conflict Studies at UCI for their financial support and for the stimulating environment they have created in SST 777. Thank you Lina, for keeping everything running smoothly, and thank you Professor Sadiq for your sincere enthusiasm when engaging with and supporting junior scholars.

Thank you to Kyle McWagner, for introducing me to The Writing Workshop, and to Professor Sarnecka for giving me the support and training to establish my own writing group.

Thank you to everyone cited in this dissertation, everyone who has provided me with useful comments at presentations, and all of my peers who have let me bend their ears. This work could never happen in a vacuum, and even though I can't thank you all individually I am eternally grateful for your community.

VITA

Paul Thomas Erich Lowood

EDUCATION

Doctor of Philosophy in Economics **2025**
University of California, Irvine *Irvine, California*

Master of Arts in Economics **2018**
California State University, East Bay *Hayward, California*

Bachelor of Arts in Economics
Bachelor of Arts in German Studies **2015**
University of California, Irvine *Irvine, California*

TEACHING EXPERIENCE

Teaching Assistant **2024–2025**
Paul Merage School of Business, University of California, Irvine *Irvine, California*

Teaching Assistant **2019–2024**
School of Social Sciences, University of California, Irvine *Irvine, California*

Grader **2017–2018**
Department of Statistics, California State University, East Bay *Hayward, California*

ABSTRACT OF THE DISSERTATION

Institutions, Infrastructure, and Development: Essays in German Economic History

By

Paul Thomas Erich Lowood

Doctor of Philosophy in Economics

University of California, Irvine, 2025

Professor Daniel E Bogart, Chair

In Chapter 1, I explore whether the local political institutions of the Holy Roman Empire had an influence of the growth trajectories of German cities. I compare the construction activity observed in cities within territories ruled by hereditary monarchs to those within territories that had some form of election. Although all regime structures observed were relatively autocratic, the constitutional law of the elected types institutionalized noteworthy constraints on the executive. Event studies suggest that oligarchies have an ambiguous effect on growth relative to hereditary monarchies, whereas ecclesiastical institutions have a positive effect, although not precisely estimated. I hypothesize that this is because election procedures in the ecclesiastical territories functioned as a significant constraint on rent-seeking, particularly after the Reformation.

Chapter 2 analyzes the relationship between fiscal capacity and railroad development in Germany. 19th century governments understood the benefits of railroads but were constrained when supporting network construction; not only by their budgets, but also by their willingness to relinquish control to private capital. I estimate how fiscal capacity's evolution across Germany influenced states' decisions to grant railway concessions to private companies or to expand public firms. I construct new fiscal capacity and railroad ownership datasets and find that increases in government revenues led to significant short-run shifts away from

public construction towards a concession based system without changing the overall rate of construction. I also collect data on annual debt servicing which suggests that the expansion of public firms had more to do with the capacity to issue debt than revenues.

Finally, Chapter 3 complicates the story of railway induced structural change. The macroeconomic benefits of the transportation revolution associated with the introduction of railways in Europe are well established, particularly in Germany where it was a key leading industry for catalyzing her industrial revolution. On the other hand, benefits in the aggregate can hide regional losers. Bavaria provides an excellent case study for understanding how large systemic shocks can leave certain groups behind. She had large endowments of metals, was the first state to introduce railways, and was hosted the factories at the forefront of the chemical revolution. However, by the early 20th century, Bavaria had largely failed to industrialize. It was also facing social crises such as high rates of emigration and infant mortality. In this paper, I explore how market access shaped population growth over this tumultuous 50-year period using an "incidental connections" identification strategy. I also explore how market access interacted with key geographic and socioeconomic variables, such as soil quality and average farm size, to explain why the negative effects of increasing market access were most acute in Bavaria's north.

Chapter 1

Princes, Merchants, and Prelates: City Growth in the Holy Roman Empire, 1400-1800

1.1 Introduction

In 1667, Samuel von Pufendorf famously labeled the Holy Roman Empire (HRE) as a "misshapen Monster" that would be difficult to evaluate "by the common Rules of Politicks and Civil Prudence". Pufendorf recognized that the semi-federal system of semi-sovereign territories comprising the HRE was a special case in Europe (Schröder, 1999). This irregular system perpetuated extreme political decentralization, which allowed idiosyncratic institutions such as the Free and Imperial Cities, Prince-Bishoprics, and Imperial Abbeys to survive into the 19th century.

In this paper, I divide the German territories into three institutional categories (Princely, Republican, and Ecclesiastical) to determine whether the persistence of these institutions influenced the growth trajectories of German cities. I contend that that the Republican and Ecclesiastical territories were constitutionally distinct from the Princely territories and significantly less autocratic. Even though the laws, feudal privileges, and style of leadership varied significantly within each category, each category has pertinent defining features.

The defining feature of the (secular) Princely territories was hereditary succession of noble titles. The specific rules of inheritance varied, with Primogeniture only slowly beginning to replace partition in the late 15th Century (Whaley, 2011a). Hereditary fiefdoms were extremely secure, as the *Reichstag* (Imperial Diet) rarely approved attempts by the Emperor to revoke them; by the 18th century few Princes even bothered to renew their vows or participate in enfoeffment ceremonies. Local estates did check the power of princes somewhat, though their influence varied significantly, and was almost certainly lower than similar institutions in England or the Netherlands (Van Zanden et al., 2012).

The Ecclesiastical Prince-(Arch)bishops, on the other hand, were elected by the local cathedral chapter. In the larger territories, this is analogous to the local estates, as the cathedral chapter would have been filled with the local nobility. The electoral procedure is significant

for two reasons: First, the newly elected Prince (Arch)bishop would be bound by a *Wahlkapituation* (electoral capitulation). This document was prepared by the cathedral chapter during each election and placed clear and usually effectively binding restrictions on the new sovereign's authority (Methuen, 2017). Second, political interference from outside actors in key elections was commonplace, making it significantly more difficult to ensure successive elections remained within the same family line.

Finally, there are the Republics, which include the Free and Imperial Cities, as well as some smaller, neighboring towns which were subordinated to them. Unlike the secular and ecclesiastical princes, these were not necessarily governed by nobles. Instead, they had oligarchic constitutions stipulating rule by a varying number of mayors and city councilmen. Election procedures varied, and were often tightly restricted to ensure the tenure of the ruling families. These procedures were not unique to the Republics; in many cases, Princes extended citizens of their cities the right to elect a mayor, or for certain guilds to hold permanent seats within their city councils (Wahl, 2019). However, the Republics were unique in that their constitutions were not granted by a local authority, but instead guaranteed by Imperial law. Only the Emperor could revoke these rights, and the political situation within the Empire generally precluded the possibility, at least until the Schmalkaldic War (Brady, 1985).

For many reasons, one might expect these constitutional differences to have economic consequences. The seminal paper in this tradition is De Long and Shleifer (1993), which argued that more absolutist governments would prioritize personal revenue over state revenue. In their analysis, Germany is treated as an institutionally homogeneous area ruled by "petty despots." I argue that the the election procedures in the Ecclesiastical territories and oligarchic rule by economic elites in the Republics should be recognized as significantly less absolutist than rule by hereditary princes. I adapt the methodology from the recent literature on the effect of democracy and democratic transitions (e.g., Papaioannou and Siourounis 2008; Acemoglu et al. 2019) to estimate the effect of institutional change within the German

territories of the Holy Roman Empire. I augment newly available data on the construction activity in German cities (Cantoni, 2020) and their local political histories (Cantoni et al., 2019). I augment this data by classifying territories as prince, republic, or ecclesiastic. For the empirical analysis I measure economic expansion in a city by the number of construction events occurring within 25 year periods. Construction data is more comprehensive in terms of geography and time coverage than the population data originally developed by Bairoch et al. (1988) which is commonly used in the literature. My initial results suggest that oligarchy has ambiguous effects that are difficult to measure precisely, whereas ecclesiastical institutions are positive, but only have a statistically significant effect on clerical construction.

Attempts to study the *effects* of the HRE's political institutions (in contrast to works focusing on *classification*, such as the aforementioned work by Pufendorf) go back to 1785 at least.[1] Modern scholarship has also begun to reevaluate the legacy of the HRE's local political institutions. Strauss (1978), Ogilvie (1992), Whaley (2011a,b), and Stollberg-Rilinger and Mintzker (2019) all describe a German state with institutions that despite (or perhaps because of) being cumbersome, were often very effective at maintaining a delicate status quo.

In this paper, I ask whether the three main forms of territorial constitutional law in 15th-18th century Germany affected the growth trajectory of German cities. My paper contrasts with similar papers by emphasizing the role of the most fundamental constitutional distinctions between German territories, rather than laws or institutions that vary at a more local level. For example,Wahl (2019) looks at the city-level procedures for choosing mayors and town councils and finds that more participative political institutions have little to no effect on city growth. On the other hand, Dittmar and Meisenzahl (2019) have shown that cities which

[1]In 1785, Phillip Anton von Bibra asked why the ecclesiastical states seemed to be less fortunate than they should be for not only were they blessed, but they benefited from good governance. He blamed their constitutional law *grundverfassung*. In response, von Moser (1787) wrote, "*konnte keine herrlicher und vor land und leute wohltaetige Anstalt, als diese, erfunden werden.*" (There can be no institution invented more agreeable to to the state and the people than this one.)

adopted *Kirchenordnungen* (Protestant church ordinances which shifted the responsibility of public goods provision from the church to the state) grew faster and attracted more human capital. Closest to my approach is Cantoni et al. (2024), which finds that territories which developed a centralized fiscal administration were more likely to survive and expand.[2]

Though I can not measure the influence of Protestantism directly, my results are closely connected to the literature assessing the broader impact of the reformation. Secularization[3] of Ecclesiastical territories accounts for at at least 30% of all observed institutional transitions, and roughly half of the transitions from ecclesiastical to secular principalities. Because these institutional transitions will be highly correlated with the adoption of Protestantism, Protestantism may be a significant omitted variable (conversely, previous studies which have focused on the adoption of Protestantism may be omitting the effect of replacing elections with hereditary rule). However, despite the persistence of Weber's 2016 protestant work ethic hypothesis, the evidence for it is mixed. Cantoni (2015) finds no evidence that Protestantism led to increased city growth when measured by population. Cantoni et al. (2018) does find that cities which adopted Protestantism were more likely to reallocate both human and physical capital from clerical to secular purposes in the early 16th century, however, one should not immediately assume that secular investment is inherently more productive. The majority of the positive results surveyed in Becker et al. (2016) come from either Prussia or Switzerland, with little discussion of the "heartland" of the HRE. Overall, the evidence seems to suggest that Protestantism itself was not particularly important for economic growth, but that it may have be correlated with particular reforms that led to higher rates of public good provision and human capital acquisition. Because ecclesiastical institutions become increasingly correlated with Catholicism over time, this is an important source of bias that will need to be addressed by subsequent research.

[2]A parallel work by Abramson (2017) looks at other determinants of state size.

[3]In the context of this paper, secularization is the legal process of transforming an ecclesiastical territory into a Princely one, for example the transformation of the Prince-Archbishopric of Magdeburg into the Duchy of Magdeburg in 1680. This is distinct from individual cities within Ecclesiastical territories being captured or purchased by secular princes.

What I do attempt to measure is the impact of transitioning between forms of government that are more or less autocratic. The interpretation of the results hinges on whether one is convinced that these constitutional forms are truly distinct. In the following section, I lay out the evidence and theoretical arguments for my tripartite classification system. Section III discusses the data, and sections IV and V report results. Section VI concludes.

1.2 Background on Institutions and Development in the Holy Roman Empire

Attempting the classify the institutions of the HRE is uniquely difficult for a a variety of reasons. Foremost among these is the fact that for most of its history, the constituent territories of the HRE were not territorial monopolies and instead were closer what Frey and Eichenberger (1999) termed "Functional, Overlapping, and Competing Jurisdictions".[4] The territories of the HRE possessed *landeshoheit*, sovereignty bounded by the laws of the Empire. Matters are further complicated by the fact that the boundary between local and imperial law was different between territories. For example, the largest territories eventually all enjoyed the *privilegia de non appellando* which made the territorial courts the supreme courts within their jurisdiction by precluding appeals to the Imperial courts. Certain types of cases, however, were exempt and could always be appealed (Oestmann, 2018).

Compounding the difficulty of mapping out the feudal nexus of overlapping rights and privileges are the sheer numbers of both territories and layers of centralization. The Empire had two competing supreme courts, a parliament, and was subdivided into ten *Reichskreisen* (Imperial Circles) which acted as federations within a federation. Some of these circles were impotent, but others played important roles in financial and security regulation Whaley

[4]See Volckart (2002) for an in depth discussion of why this form of government obtained in Medieval and Early Modern Europe.

(2011a). With the exception of the largest territories, it is essentially impossible to know the precise details of the legal landscape.[5] As for the small territories, there is no consensus regarding their exact number, particularly since the territories of the Imperial Knights could be as small as a single manor.

It is important, then, to focus on what *can* be defined. The most straightforward way to define a territory, rather than trying to set some threshold level of independence, rights, or privileges is based on "Imperial Immediacy" (*Reichsunmittelbarkeit*). Immediate fiefs were those granted directly by the Emperor, with no intermediate lord. Immediacy generally implied a vote in the Imperial Diet as an Estate of the Empire (some especially small territories did not receive a vote, or only received a collective vote as a member of a "bench"), and receiving multiple titles could imply multiple votes.

The territories generally organized themselves into corporate bodies along three different dimensions, depending on the prevailing issues of the day. The first axis of division followed the official branches of the Imperial Diet: The College of Electors, the College of Princes, and the College of Imperial Cities. The Electors and Princes were further split into Ecclesiastical (*Geistliche*) and Secular (*Weltlichen*). Finally, following the Peace of Westphalia, the Diet was split into the *corpus Catholicorum* and *corpus Evangelicorum*. That these divisions were officially recognized in the proceedings of the Diet reflects the distinct special interests of diet members according to their position and confession.

To simplify the groups, I ignore two divisions–Catholic/Evangelical and Elector/Prince–which were not relevant to the *local* political situation. That is to say, the laws of the Empire did not generally specify differing rules for how Catholics and Evangelicals could rule their territories. Similarly, the constitutional law of an electoral Prince-Archbishopric

[5]For example, consider that Van Zanden et al. (2012), the most comprehensive attempt to measure the prevalence of parliaments in Europe, only has data for the parliamentary meetings of six individual territories within the HRE, despite evidence that *all* territories within the HRE could not levy new taxes without consent from the appropriate local body (Whaley, 2011a)

was similar to that regular Prince-Bishopric. This reduces the types of territories to three: the ecclesiastical principalities (Prince-(Arch)bishoprics and Imperial Abbeys), the secular principalities (Dukes, Counts, Margaves, etc...), and the Republican Free and Imperial Cities. This grouping is natural for two reasons: first, it is consistent with how the territories viewed and organized themselves as corporate interest groups; second, the constitutional laws of these territories were clearly and consistently distinct.

The defining institutional feature of the ecclesiastical territories is that they were elective monarchies. However, the electing body was extremely small and entry was tightly regulated. In the Prince-(Arch)bishoprics, the cathedral chapters responsible for electing the Prince-(Arch)bishop were dominated by nobles due to restrictive rules requiring members to have multiple generations of noble ancestry (Methuen, 2017). Not only was entry to the electing body restricted, but candidacy was tightly restricted as well, and typically on genealogy rather than qualifications. These restrictions not only specified a requisite number of noble agnates, but often attempted to limit eligibility to local or regional families, although locality requirements were subject to more legal challenges than simple residency requirements (Feine, 1921). Theological training was only required in certain cases, and dispensations were often made to allow ecclesiastical Princes to delegate their religious responsibilities to suffragan bishops. The fact that some of these so-called bishops could not speak Latin was one of the many pressures that lead to the Reformation. Furthermore, upon election, many of these rulers were bound by Electoral Capitulations (*Wahlkapitulationen*), which clearly delineated the boundaries of their executive power. Taken together with the strict requirements of membership in the Cathedral Chapter, it becomes clear that the local political elite were highly influential in the Ecclesiastical territories, in contrast to the Princely territories where the influence of local Estates is not well understood.

Participation in Republican government was also relatively constrained, although the specific rules of participation were quite heterogeneous. Generally, the Free and Imperial Cities were

administered by a pair of mayors and one or two town councils. Political access was limited to citizens, but recent research by Minns et al. (2020) suggests higher rates of citizenship than previously believed. However, political access did not always translate real political influence, as some cities filled vacancies with co-optation rather than election. Furthermore, following an Imperial intervention in the mid-16th century, guild members (seen as responsible for the spread of Protestantism) were systematically disenfranchised in favor of elite patricians (Brady, 1985). These restrictions persisted even in cities which formally adopted Protestantism.

Given that political participation in both the election of ecclesiastical princes and republican magistrates, one might suggest that neither form of government was significantly less autocratic than princely hereditary monarchy. There are two ways to refute this argument. First, one could collect much more granular data on the specifics of the election procedures in each territory, and use a more continuous measure of representation that would not code "false positives" as relatively more democratic. For example, omitting Republican territories which practiced co-optation. As this lies outside the scope of the current draft of this paper, I dedicate the remainder of this section to making a theoretical argument supporting my classification system.

It could be said that restrictions on cathedral chapter membership led to non-competitive elections, particularly if the entire electorate comes from a single dynasty and its allies. Although some lines did repeatedly win elections, this does not necessarily mean that elections were non-competitive. For example, it is often written that the Archbishopric of Cologne was effectively a secundogeniture inheritance of the Wittelsbach dynasty as Wittelsbach second sons inherited the title uninterrupted between 1583 and 1761. During the same period, the Wittelsbachs also attempted to treat the bishoprics of Muenster and Liege as secundogeniture inheritances, but with less success, showing that perfect capture did not exist and there were limits to the ability to transform elected positions into hereditary ones; repeated

victory is not the same as true capture, and there is not yet sufficient data to say whether obtaining repeated victory did not involve significant repeated costs. One significant check on the ability to capture elections was that the emperor retained the right to nominate the first new member of any cathedral chapter following his coronation, which could be used to tip the balance in competitive elections.

Because success in repeated elections was never guaranteed and required substantial political and economic investments to fend off competitors, I argue that these elections aligned the incentives of rulers with the local elites. The continued support of the electorate is more likely to be contingent on the economic success of the local area than the larger "foreign" territory the elected dynast is concerned with. This is particularly pertinent in the case of ecclesiastical rulers elected to multiple simultaneous positions who made have needed to make separate capitulations to the cathedral chapters of each territory. This argument (and the existence of the electoral capitulations) is consistent with theory from Olson (1993), who states that "An independent capacity to install a new ruler would imply that this capacity can be used to remove or constrain the present autocrat."

Thus, the elected monarchs face a trade-off between short-term looting for personal benefit, and cooperation with local economic elites to ensure the title remains within their dynasty. Unfortunately, the effect of political competition on the sovereign's time horizon is likely non-monotonic. If there is no real competition and the monarch is from a non-local dynasty there is little to dissuade them from engaging in short-term looting or adopting policies that favor the dynastic homeland at the expense of the ecclesiastical territory. Similarly, if they are certain they will lose the next election, there is little incentive not to engage in looting. A healthy amount of competition is necessary to prevent this outcome; if there are a variety of credible candidates, there can be a "race to the bottom" effect, which results in the eventual winner agreeing to a stricter electoral capitulation which is most favorable to the local elites. This leads to policies with a longer time horizon, or at least which favor local elites at the

expense of distant dynastic lands, leading to relatively more local economic activity.

Surprisingly, there is less evidence to believe that Republicanism is associated with growth. The main argument for expecting Republicanism to be associated with higher growth is that the Free and Imperial Cities were at least as autonomous as other autonomous cities (and likely more), and autonomy has been associated with growth since Weber (2019). Furthermore, particularly in the 15th and 16th centuries, the leadership class of the Free and Imperial Cities was distinct. The oligarchies were non-noble and identified with the "common man"; Citizenship was based on oaths that affirmed a communal way of thinking (Brady, 1985). This, perhaps, is why Jacob (2010) finds persistently higher social capital in these cities today. Thus, we should view these cities as governed by a self-ruling economic elite, rather than an local elite in cooperation with a dynastic prince. Because this elite was closely linked to the economy, princes often delegated their minting rights to them to resolve commitment issues related to seigniorage (Volckart, 2007).

Specific economic evidence about the free cities is hard to find, but Stasavage (2014) is much more pessimistic. He looks at all autonomous cities, which also includes cities within Principalities that received limited rights, and finds that an initially positive effect on city growth turns negative after about 160 years of autonomy. This is consistent with arguments from Olson (1982), Acemoglu (2008), or Ogilvie and Carus (2014), which all generally agree that when economic elites are also political elites, they will eventually use their control over property rights for rent seeking, rather than policies that might benefit competitors.

Note that these arguments are not specific to the independent Free and Imperial Cities, but refer generally to cities which received some degree of autonomy. Because this evidence for autonomy is ambiguous, it is difficult to say whether more or less autonomy would be preferable, or under what circumstances. For example, a strong prince may want to prevent the local elites in an autonomous city from engaging in excessive counter-productive rent-seeking. One piece of supporting evidence the helps slightly, is evidence from Wahl (2019)

which suggests that elections (in contrast to co-optation or direct appointment) are the most beneficial form of autonomy. Wahl's data only includes 10 of the 90 cities I find were ever under a Republican government, but he finds 5 of them held elections, a much higher rate than among cities under monarchs.

In summary, these two forms of limiting autocracy function through different mechanisms, and theoretical or qualitative arguments have ambiguous predictions. Elective monarchy implies that local political elites acted as a significant check on dynastic ambitions; ensuring their cooperation could have spilled over into local economic growth in many ways, particularly if the local political and economic elites overlapped. However, the effects of this check depend on how confident the dynasty is that it can secure reelection. Essentially, when the local elites have sufficient power to maintain a credible check on the executive, they can force dynasties (whose interests are generally selfish and broad) to care about their local context.

Republican government, however, involves a local political elite which governs itself that is usually also the local economic elite. The concerns are entirely local, and the trade-off is instead is about whether institutions should be set up to maximize overall wealth or to secure rents. The recent literature on this trade-off discussed above is extremely pessimistic about the choices that oligarchies usually make, especially when the oligarchies are long-lived.

This paper thus proposes two questions: First, what the ramifications of important checks on autocracy in Germany's early modern period? Second, which checks were most effective?

1.3 Data

The independent variable of interest for this study is a classification of territorial institutions I derive from Cantoni et al. (2019). The core source is the *Deutsches Staedtebuch*, which includes information compiled by local historians on all 2390 locales within the 1937 borders

of Germany that "at one point in the history of the Holy Roman Empire was awarded the status of a city" (Bogucka et al., 2019). Using the information within the *Staedtebuch* and supplementing it with other sources, Cantoni et al. create a timeline of each city's territorial history. This tracks which territory any city belonged to at a given time.[6] I use the territory names in Cantoni et al, supplemented with additional sources such as Bühner (2019), to assign a classification to each territory code of either "princely", "ecclesiastical", or "republican". Thus, at each point in time, I observe whether a city was part of a noble's hereditary lands, an elected prelate's jurisdiction, or a merchant oligarchy.[7]

The distribution of institutions over time is visible in Figure 1.1. There is a clear trend away from ecclesiastical rule in the Protestant north. The two major events here are the creation of the Duchy of Prussia out of lands held by the Teutonic Order, and the Peace of Westphalia, which formally made many of the protestant "administrators" (as the lay rulers of protestant ecclesiastical principalities were styled) into secular princes. We can also see that institutional diversity increases as one moves South or West. By 1750, nearly all of the cities east of the Elbe are under Princely institutions.

To measure the effect of institutions, I use data from Cantoni (2020) on significant construction events in each city, which also uses the *Staedebuch* as its primary source. The *Staedtebuch* does not have objective criteria for determining significance, but examples of inclusions are town halls, churches, and castles. The date for recorded constructions ranges from 100 to 1800, so it is possible to calculate an initial stock of buildings in 1400 and trace the evolution of this stock as more constructions occur. For simplicity I assume zero depreciation. Thus, we have data on both economic growth (construction events) and eco-

[6]The publicly available dataset only reports territory in 1400, 1450, 1550, 1650, 1750 and 1850. I interpolate to a 25 year resolution and manually adjust the timing of transitions when the institutional category changes.

[7]Some cities belong to multiple territories due to power sharing arrangements. Currently, I code all arrangements according to which institutional category was dominant. Instances where the balance of power was evenly split between territories with different institutions are given a special case.

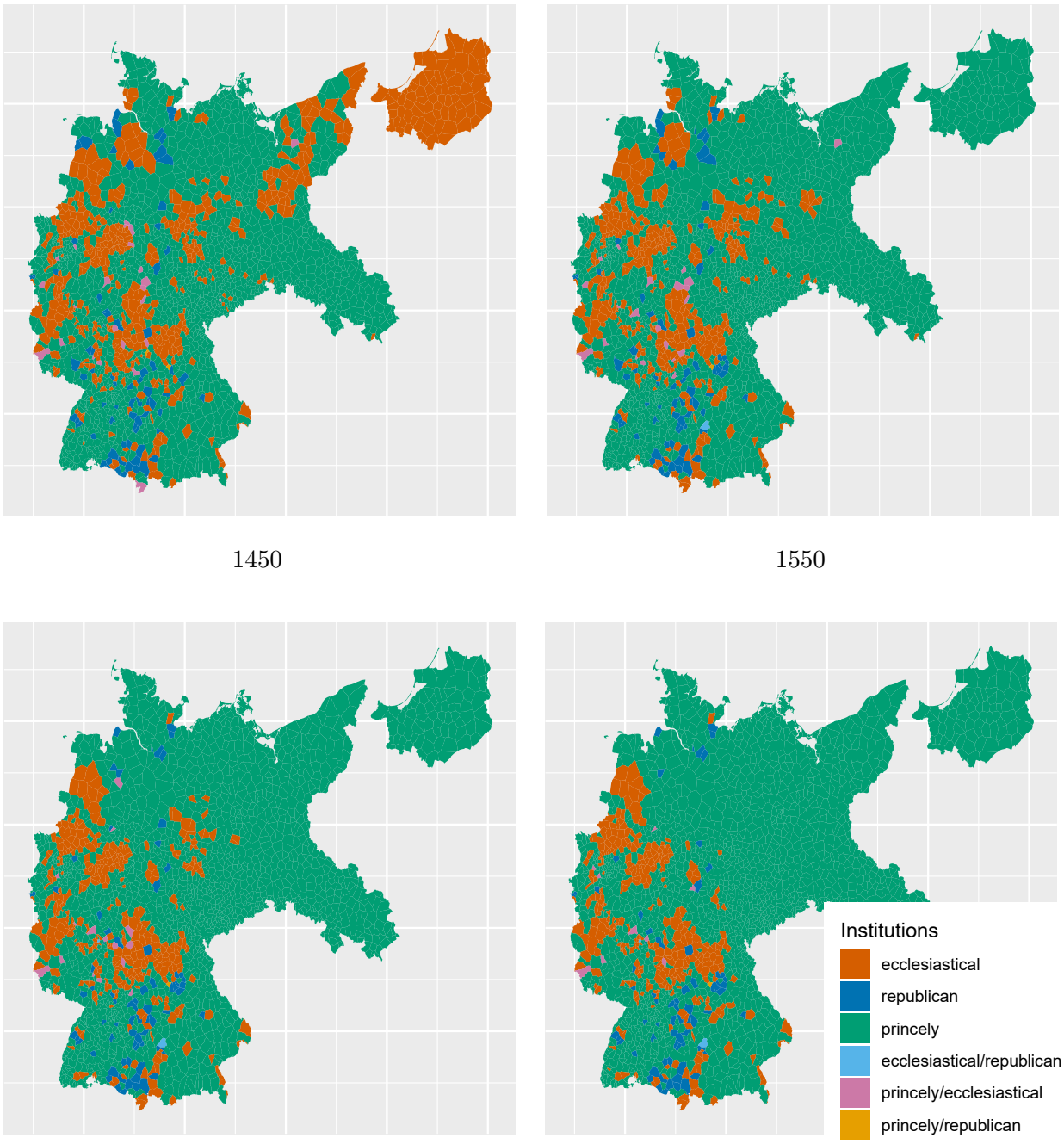


Figure 1.1: Institutional Forms over the Centuries.

nomic levels (total stock).[8] In the public version of the data-set, construction events are recorded at 25 year intervals. These constructions are also classified by type (e.g. clerical, administrative, economic...). I aggregate all non-clerical categories into a single new category. Construction events are rare, which is exacerbated by many of the "cities" in the panel being relatively rural.[9] On average, cities have one recorded construction event every half-century. The distributions of clerical and non-clerical buildings are nearly identical,

Typically, papers use population data from Bairoch et al. (1988) as an outcome. However, construction data has two significant advantages over the population data, conditional on accepting this measure of construction activity as a similar quality proxy for economic growth. The first is that it allows us to study an order of magnitude more cities, and thus capture much more variation in institutions. The second is that there are no missing values. If there are no recorded construction events in a period, it can be interpreted as no construction being noteworthy enough to survive in the historical record, rather than there actually having been no construction. Unfortunately, because the criteria for a construction event to be noteworthy enough to be entered into the *Staedtebuch* is nebulous, it is difficult to test the quality of construction as a proxy for growth, although it can be shown that construction and population are significantly correlated.

I also incorporate rudimentary geographic controls. First, I use the region variable from Cantoni et al. (2019). These regions correspond to *Staedtebuch* volumes which are grouped in a manner consistent with important political boundaries. I also code whether the city is on a river using data from data from noa.

I restrict the sample to my preferred subset which is based on three conditions: First, I

[8]Because stock is simply a running total of all construction events, it only increases. This no depreciation assumption certainly overestimates levels, particularly in areas disproportionately affected by war. Unfortunately, I am not aware of any comprehensive data that can be used to estimate a depreciation rate.

[9]The commonly used Bairoch et al. (1988) dataset includes 245 cities within the borders of modern Germany. This suggests that approximately 2,000 of the cities observed here never achieved a population of 5,000 before 1850.

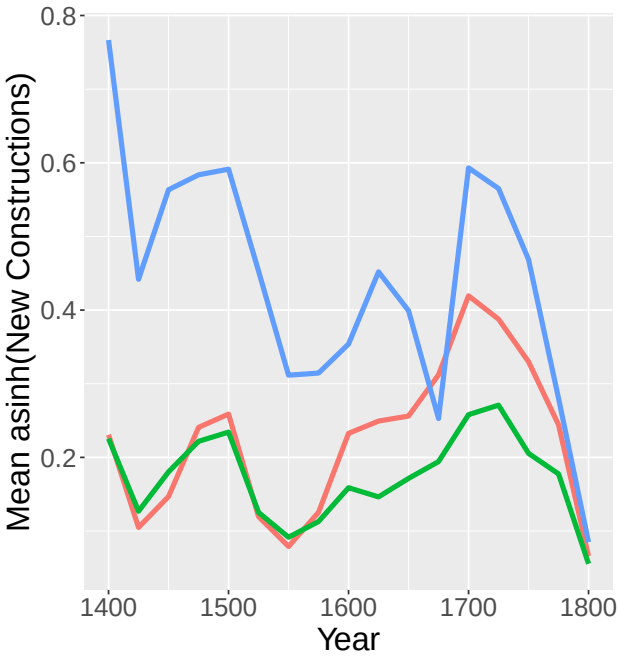
drop any city which was ever at one point not German (n = 435), e.g. cities temporarily conquered by France or Sweden, or part of a foreign kingdom such as Denmark or Poland. I also drop cities in Bohemia during this step, as Bohemia was exempt from many of the laws of the HRE. Second, I drop any city where there was a power-sharing agreement between two territories with different institutional forms that did not have a dominant member (n = 55). Finally, I drop any city which ever transitioned between Ecclesiastical and Republican institutions (n =4), for reasons explained shortly. Summary statistics for the main variables after subsetting are reported in Table 1.1.

Table 1.1: Summary Statistics

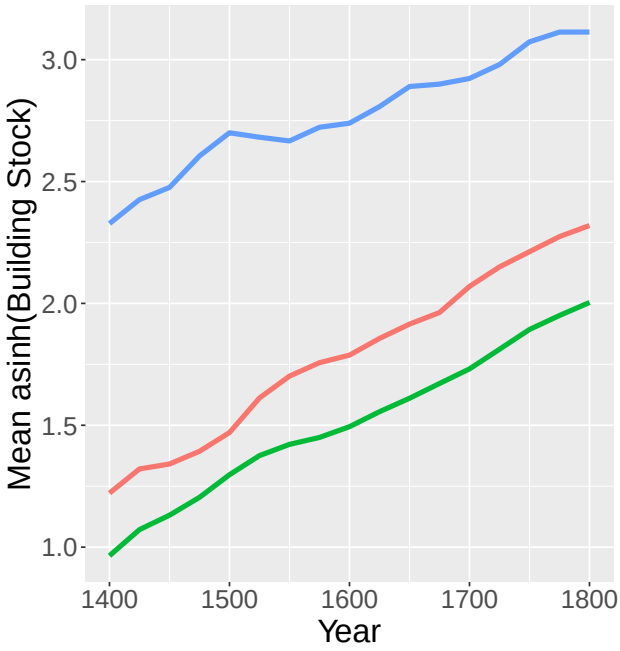
Statistic	N	Mean	St. Dev.	Min	Max
Princely	32,266	0.747	0.435	0	1
Ecclesiastical	32,266	0.218	0.413	0	1
Republican	32,266	0.036	0.185	0	1
newbuilds	32,266	0.489	1.083	0	31
newbuilds_clerical	32,266	0.249	0.649	0	21
newbuilds_nonclerical	32,266	0.240	0.701	0	21

Balanced panel: 1898 cities observed at 17 25-year intervals between 1400 and 1800. asinh() refers to the Inverse Hyperbolic Sine function.

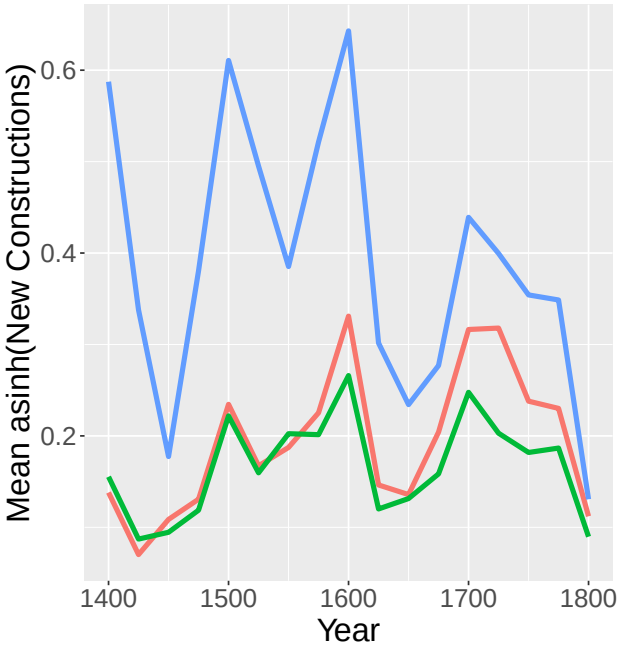
Figure 1.2 provides some useful intuition about the data by plotting the average growth and stock of construction grouped by institutions. There are a few key facts to note: republican cities start significantly larger and grow noticeably faster. Ecclesiastical cities start very similar to princely ones, but these trajectories diverge over time as ecclesiastical growth accelerates, particularly after 1600. It is important to note that institutions are not fixed, thus these trends are partly driven by cities switching between institutional types. In the following section, I exploit these changes to estimate the causal effect of institutions.



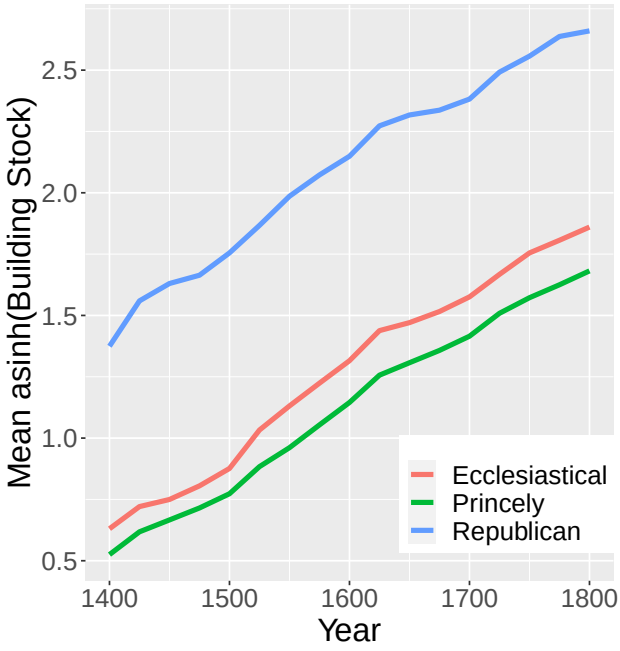
Mean Clerical Construction



Mean Clerical Building Stock



Mean Non-Clerical Construction



Mean Non-Clerical Building Stock

Figure 1.2: Construction Trends by Institutional Form

1.4 Baseline Model: Panel Regressions

The baseline model (Table 1.2, Column 1) regresses construction (using an inverse hyperbolic sine transformation, an alternative of the log transformation which is defined for 0) on dummy variables for institutional categories as defined above:

$$Construction_{i,t} = \beta_1 Ecclesiastical_{i,t} + \beta_2 Republican_{i,t} + \alpha_i + \alpha_t + \sum_{t=1}^{17} \rho_t X_i \cdot t + \beta_3 stock_{i,t-1} + \sum_{j=1}^{8} \gamma_j Construction_{i,t-j} + \epsilon$$

Where α_i and α_t are standard two-way fixed effects, $\sum_{t=1}^{17} \rho_t X_i \cdot t$ allows the effect of fixed observables (such as geography) to vary over time. Finally, we control for current building stock levels (again transformed by the inverse hyperbolic sine function), measured prior to construction in period t, as well as the previous growth trajectory.[10]

Table 1.2, Column 1 shows that the differences in Figure 1.2 that were apparent from a visual inspection are in fact statistically significant. However, all significance disappears in Column 2, which includes two-way fixed effects. Columns 3-5 each add one of the remaining terms, and the results do not substantively change. Based on this regression, there does not appear to be a significant difference between institutions. However, this baseline specification should not be over-interpreted for two reasons: First, it averages the effect of transitioning either into or out of princely institutions, but as the next section will show, the effect of these transitions is not necessarily symmetrical. Second, the "control group" is not precisely defined since the reference group (ecclesiastical = 0 and republican = 0) includes cities which could at some point be in either non-prince category. Both of these facts can strain

[10]Because this is a "high n, low t" setting, Nickell bias (described in Nickell (1981)) is a serious concern. As data for more covariates becomes available, it may become possible to use the Arellano and Bond estimator (from Arellano and Bond (1991)) to account for this.

the parallel trends assumption. To account for this, in the next section I conduct event studies with precisely defined control groups so that the coefficients are consistent with a canonical difference-in-differences approach.

Table 1.2: Two-Way Fixed Effects Panel Regressions

Dependent Variable:	asinh(Construction)				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Constant	0.32***				
	(0.007)				
Ecclesiastical	0.05***	0.02	-0.006	-0.001	0.004
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Republican	0.40***	0.09	0.07	0.07	0.08
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
asinh(stock)				-0.17***	-0.21***
				(0.01)	(0.02)
Newbuilds.L1-L8	No	No	No	No	Yes
Fixed-effects					
city_id		Yes	Yes	Yes	Yes
Year		Yes	Yes	Yes	Yes
Year-region_id			Yes	Yes	Yes
Fit statistics					
Observations	32,266	32,266	32,266	32,266	32,266
Adjusted R^2	0.02	0.25	0.27	0.28	0.28

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

1.5 Difference-in-Differences

1.5.1 Methods

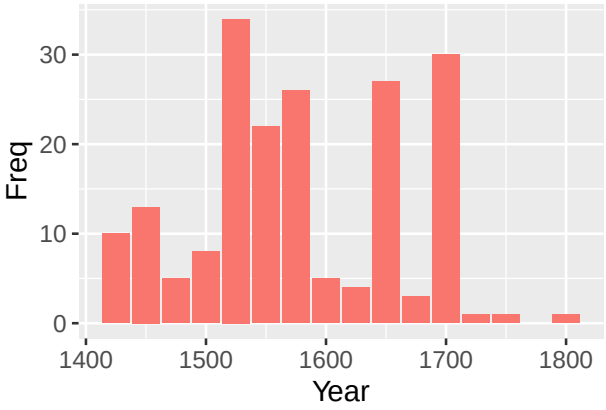
In some cases, institutional transitions were only temporary[11] and two cities (Goerzke and Herford) even experience a double reversal. To simplify the interpretation of the following event studies, I restrict the set of transitions to the 242 cities which experience a single permanent transition. For each treatment, the sample is also further restricted so that the coefficients have a canonical difference-in-differences interpretation. For example, when studying the effect of transitioning from Ecclesiastical to Princely institutions, the control group is limited to only those cities which were remained ecclesiastical for all periods. Figure 1.3 presents the distribution of transitions over time. We can see that the vast majority of institutional transitions occurred prior to the 18th century; the institutional framework of the HRE was mostly stable for the hundred years before it began to collapse after the French Revolutionary Wars.

I treat each type of transition as a treatment, and run event studies based on the following specification:

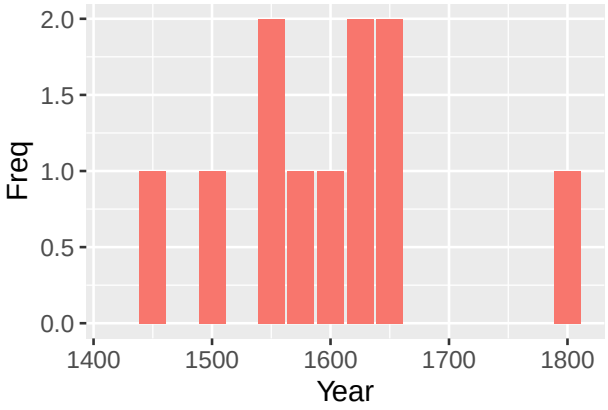
$$\text{asinh}(Construction_{i,t}) = \sum_{k \in [-16,16]} \beta_k TTT_{i,k} \cdot Treated_i + \alpha_i + \alpha_t + \sum_{t=1}^{17} \rho_t X_i \cdot t + \epsilon_{i,t}$$

Where $TTT_{i,k}$ is "time to treatment" following the standard event study framework, where $t = 0$ is defined as the year in which the institutional transition for city i is observed. As we have shown in Table 2 (Columns 4 and 5) that the omission of lagged construction levels or growth did not significantly bias the coefficients on institutions, lagged construction and

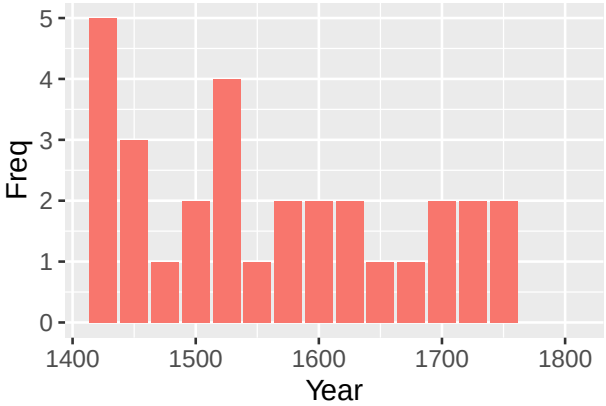
[11] 43 Prince to/from Ecclesiastical transitions revert back to Princely/Ecclesiastical institutions, while 6 Prince to/from Republic transitions revert back to Princely/Ecclesiastical institutions.



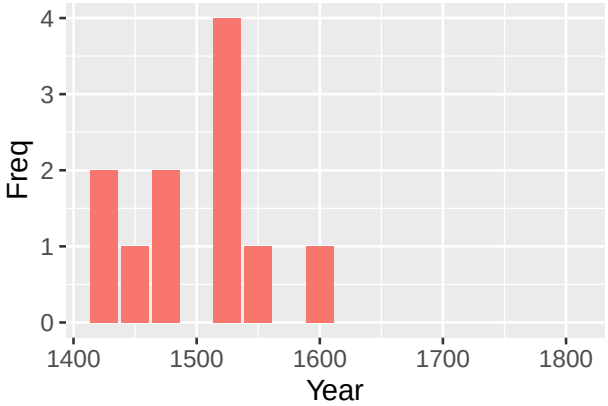
Ecclesiastic to Prince, n = 190



Prince to Ecclesiastic, n = 11



Republic to Prince, n = 30



Prince to Republic, n = 11

Figure 1.3: Frequency of Permanent Institutional Change.

building stock are omitted to avoid potential Nickell bias.[12]

Because treatment timings are heterogeneous, the results are susceptible to biases if treatment effects are also heterogeneous across time Goodman-Bacon (2021). To address this, I use the "interaction-weighted" estimator proposed by Sun and Abraham (2021) which reweights the coefficient estimates according to the size of treatment cohorts. This estimator is robust to bias introduced by heterogeneous treatment effects, but still requires the "parallel trends" and "no anticipatory behavior" assumptions to be satisfied. For brevity, the following section reports only the statistics estimated using this method.

Unfortunately, it is difficult to argue that the assumptions for a causal interpretation of the difference-in-differences coefficients hold. With few exceptions, one cannot argue that transitions between institutions are random, as shown in Figure 1.4. Purchase, conflict, and conquest are all deliberate actions made with the explicit purpose of taking ownership of a city. Furthermore, it is hard to say that targeting is random; one expects leaders to be strategic in their expansion choices, although purchase was frequently opportunistic rather than planned. Most purchases are due to insolvent princes pawning off territory, rather than a concerted effort by the purchaser to expand their territory. Failing cities might be easier to conquer or cheaper to purchase, so poor cities see more transitions. The correlation between social unrest and institutional transition remains a challenge to many papers in the literature, and the IV approaches typically used are hardly convincing.

Common shocks are also likely to bias estimates, particularly in the long run. Using a staggered implementation approach reduces the bias around the transition period when transitions are evenly distributed, but causes increasing bias in the long run. For this reason, I only report coefficient estimates of effects within 100 years of the transition. Pertinent examples of such common shocks are the Reformation, 30 Years' War, and Counter Reformation, which all significantly shocked how the institutions of the Ecclesiastical territories

[12]Including either variable does not substantively change the results, which are not reported for brevity.

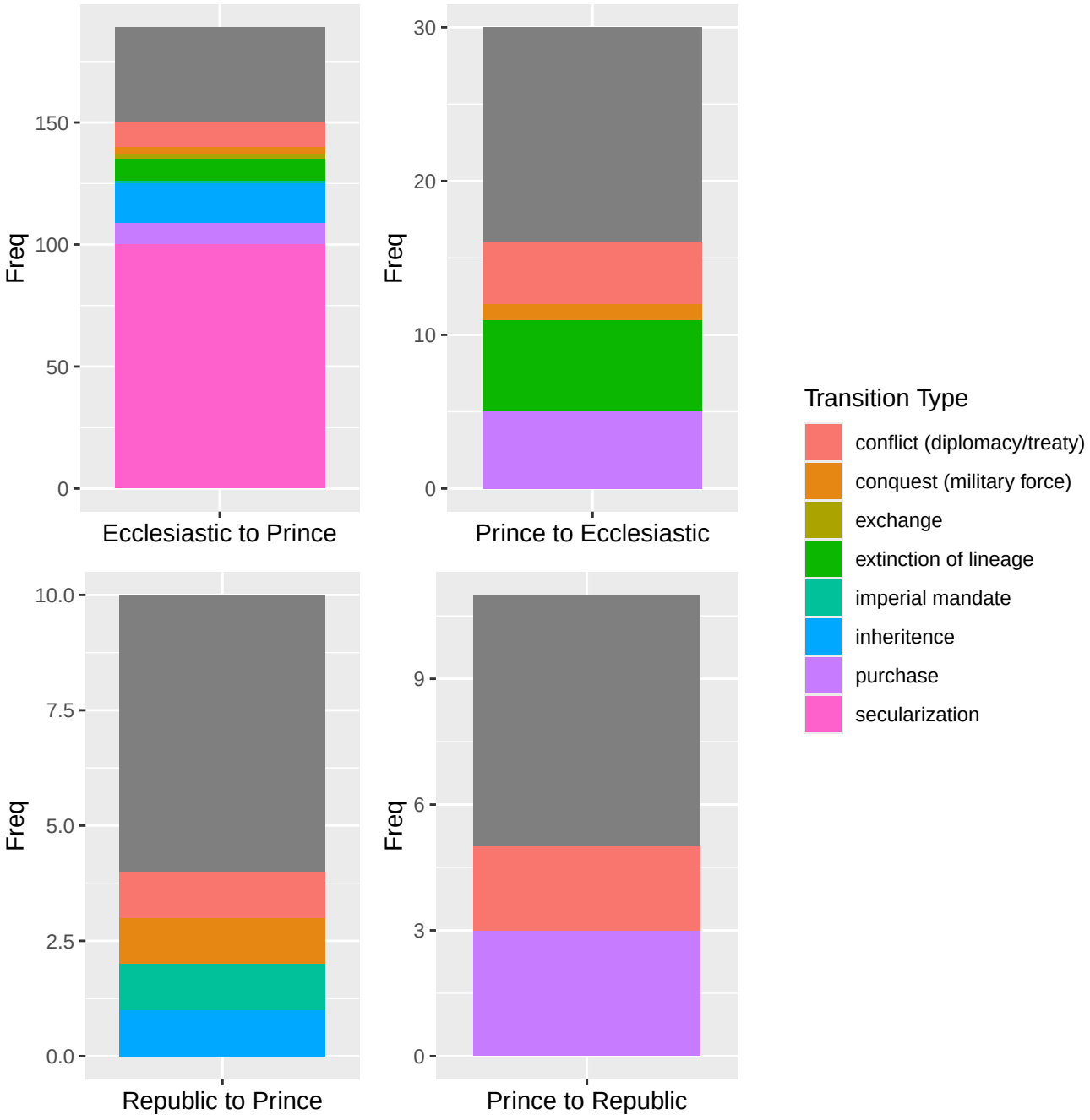


Figure 1.4: Circumstances of Government Transitions

interacted with the broader laws of the HRE. For the Republics, the most important shock was the Swabian War, which led to Emperor imposing changes in the constitutional laws of the Free and Imperial Cities, specifically the reduction of craft guild influence in favor of merchant patricians.

1.5.2 Results

Figure 1.6a reports the benchmark event study results. Panel A shows that there does not appear to be a significant effect when transitioning from Princely to Ecclesiastical institutions. The parallel trends assumptions appears reasonably satisfied, but it is difficult to argue in favor of there being no anticipation effect. Secularization was an extremely politicized process, and often required drawn out legal procedures before changes were legally recognized. Furthermore, many ecclesiastical territories in Protestant territories were *de facto* secularized when they elected "lay administrators" instead of Bishops. Where possible, I use the dates of *de facto* secularization in favor of *de jure* secularization, but it is possible that these results are contaminated by the systematically different procedures of secularization that occurred in the Protestant regions.

Panel B shows that there is a positive effect of transitioning from Prince to Ecclesiastic, although it is not precisely estimated in all post-periods. The coefficient implies .6 more construction events per century, which is non-trivial considering the mean for new buildings is about 2. There does not appear to be a pre-trend, and anticipation effects are possible but less likely. Many Prince to Ecclesiastical transitions occurred when Princes desperate for money mortgaged or pawned their lands and titles; since the transitions we observe are permanent, we can assume that the original holders defaulted. It is reasonable to assume these circumstances are not anticipated in the time scale we are looking at.

Panels A and B have a similar shape, with the effect of transitioning from elective to heredi-

tary monarchy appearing to be negligible, while the effect of adopting ecclesiastical elections is very weakly positive. Further research is necessary to explore why the direction of the transition matters. On the other hand, Panels C and D, which report the effects of transitioning between Republican and Princely institutions, do appear to be symmetrical. However, it is important to note the statistical power of the lower panels is substantially lower, as there are far fewer observations.

Panel C shows that Free and Imperial Cities losing their imperial immediacy may have only recently regressed to the mean after periods of below-average growth. Transition is associated with an immediate negative effect, but rapid stabilization. This suggests that transitions may have been destructive, but the actual rule of Princes was not substantially worse than autonomy for these cities. It is possible that the most destructive oligarchies weakened themselves to the point that they could no longer withstand Princely encroachment upon their freedoms, at which point Princes were no worse than Oligarchs, but more research is necessary to support this hypothesis.

Panel D is more challenging to interpret, as the pre-trend has an irregular shape suggestive of an anticipation effect. Transitions to Republicanism can take two forms: first, if a city becomes sufficiently prosperous it may be able to buy or fight for its freedom; second, a prosperous city might expand outside of its walls. Because most instances of the first type of transition occurred prior to 1400, the transitions observed here are generally the second type. This means that cities transitioning to republicanism are likely benefiting from spillover effects of being near prosperous republican cities just before the transition. These effects are not particularly persistent, and again there is some weak evidence that the transition led to reduced growth in the long run, as the coefficients are all negative after 100 years.

Figures 1.8a and 1.10a repeat the analysis, but look specifically at either clerical or non-clerical construction. In general, the results are qualitatively similar but there are two noteworthy differences: First, Figure 1.8a (clerical building) Panel B (prince to ecclesiastic)

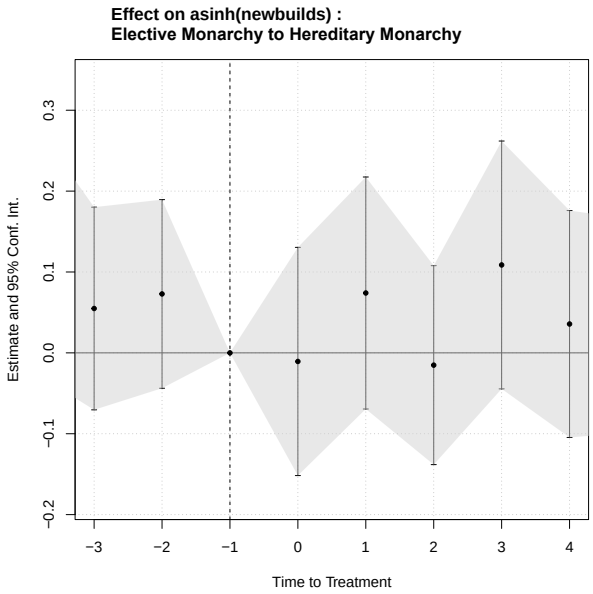
is the only panel which has a jointly significant overall post-treatment effect. However, the post-treatment effect is not much different in magnitude from a persistent pre-trend. It is unclear why a transition into ecclesiastical institutions would be associated with a significant and transient decrease in clerical construction. Figure 1.10a provides a view of Republicanism consistent with the hypotheses discussed above, as there appears to be a short run bump in non-clerical construction that turns weakly negative after around 100 years.

1.6 Conclusion

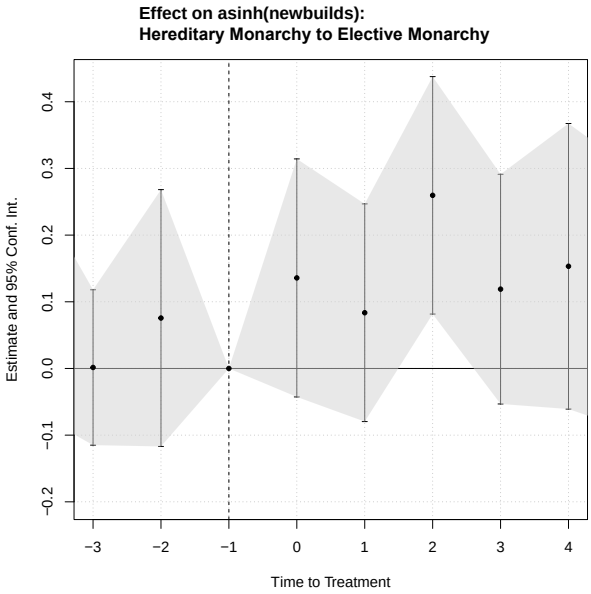
This paper has focused on providing a proof of concept, rather than performing exhaustive robustness checks. As the available data is revised and expanded, it may be fruitful to repeat the analysis here using more sophisticated methods, such as Poisson regressions or explicitly modeling the selection on observables (e.g. by propensity score matching) to control for pre-trends. Unfortunately, there simply is no readily compiled data on observables for the vast majority of the cities in this sample yet.

Ultimately, these preliminary results lead to more questions than answers. Transitions into Republicanism appear to result in significant but transitory increases in construction activity, which does not strongly support the De Long and Shleifer hypothesis that these forms of government led to long run growth relative to more autocratic ones. On the other hand, the support for a long run negative effect is not there either, because the negative coefficients after 100 years are not statistically significant.

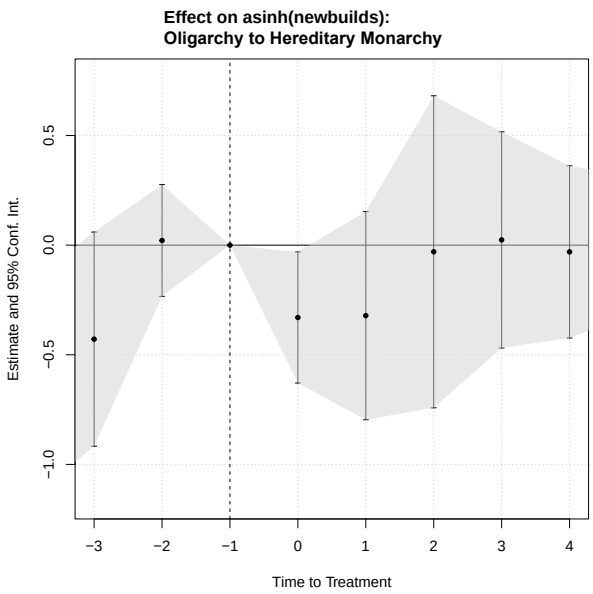
For ecclesiastical institutions, it is difficult to separate the observed increase in construction from the fact that after 1600 most ecclesiastical territories were Catholic, and engaged in much more clerical construction. Since we do not see a significant change in non-clerical construction, it is possible that this is driven by counter-reformation efforts and not by local



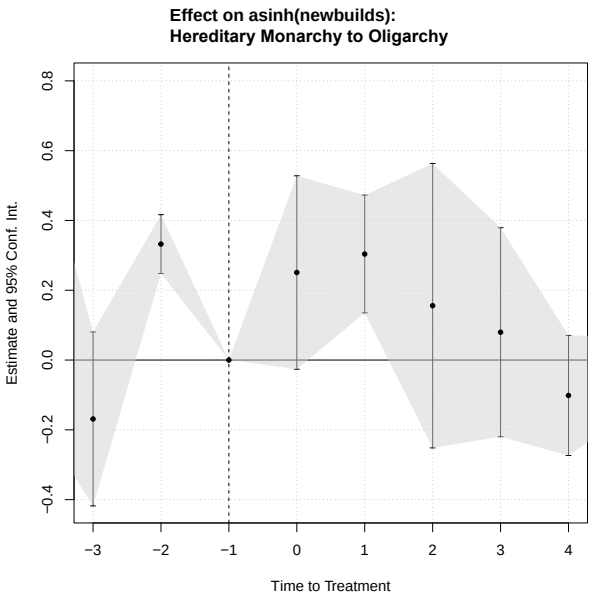
Ecclesiastic to Prince, n = 8,432
t value: .26



Prince to Ecclesiastic, n = 21,760
t value: 1.33



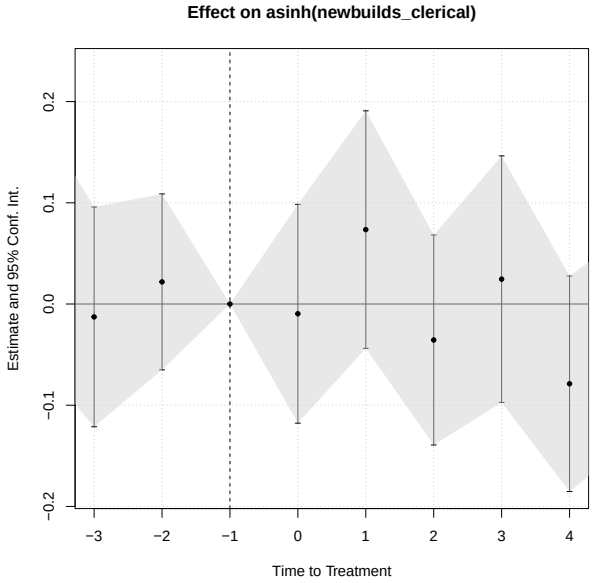
Republic to Prince, n = 1,071
t value: -.15



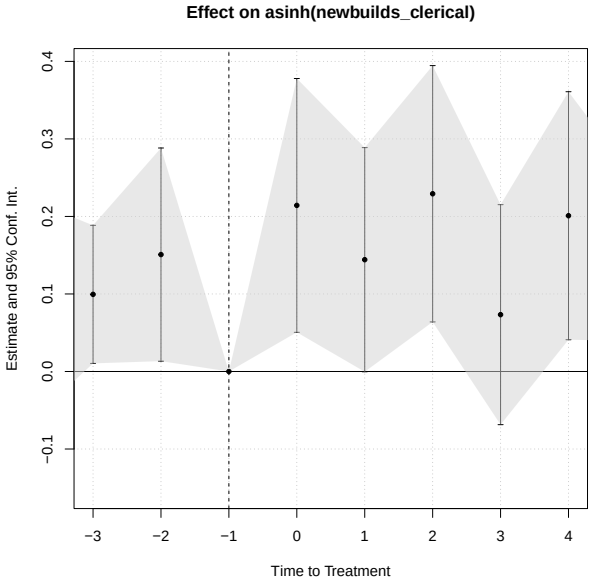
Prince to Republic, n = 21,437
t value: .34

Figure 1.5: Event Study: Institutional Change and Total Construction

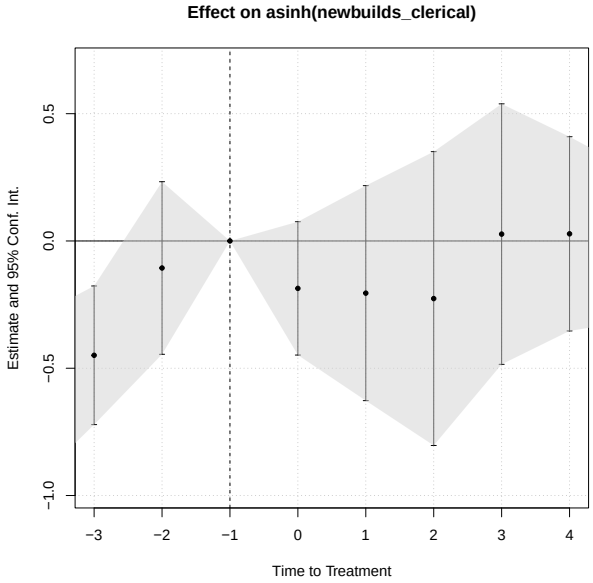
The effect of institutional transitions on construction event frequency, using the Sun and Abraham (2021) IW estimator. Relative periods represent 25 year intervals. T value reports the significance of the "Average Treatment Effect for the Treated", the weighted average total post-treatment effect.



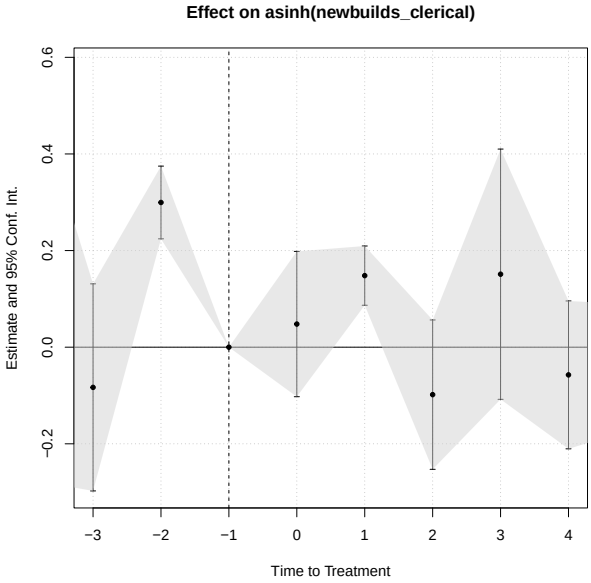
Ecclesiastic to Prince, n = 8,432
t value: -.19



Prince to Ecclesiastic, n = 21,760
t value: 3.12



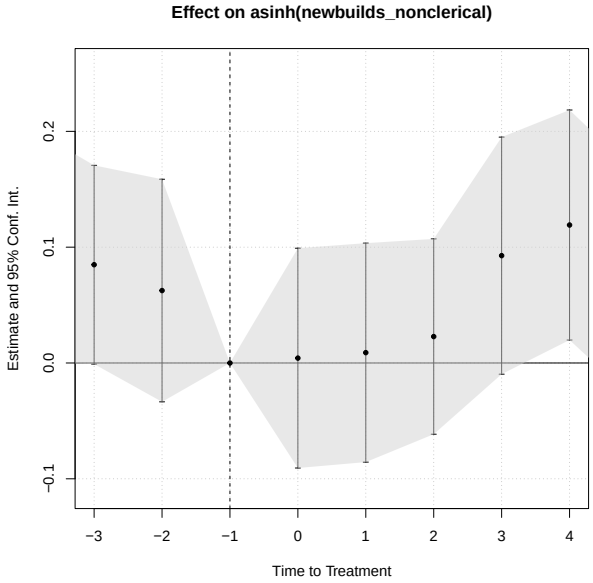
Republic to Prince, n = 1,071
t value: -.22



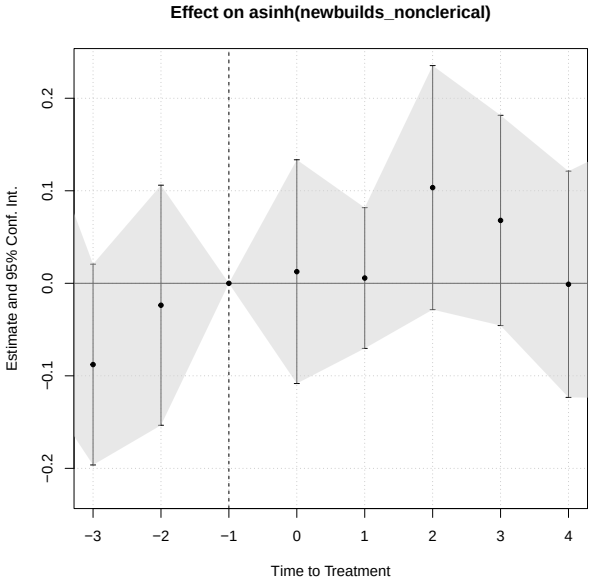
Prince to Republic, n = 21,437
t value: .09

Figure 1.7: Event Study: Institutional Change and Clerical Construction

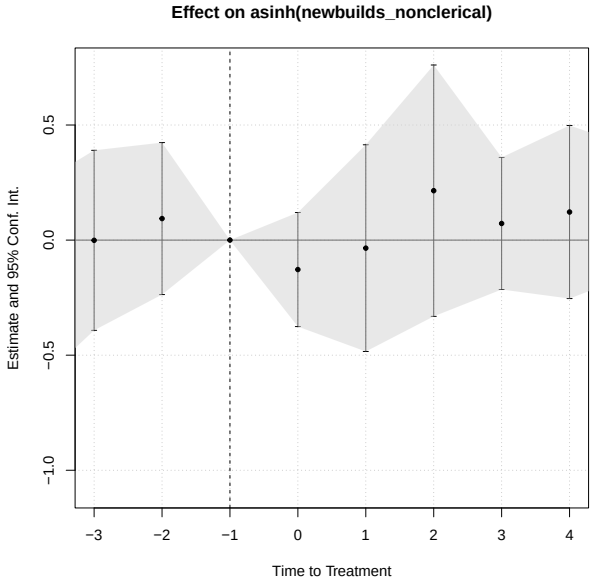
The effect of institutional transitions on construction event frequency, using the Sun and Abraham (2021) IW estimator. Relative periods represent 25 year intervals. T value reports the significance of the "Average Treatment Effect for the Treated", the weighted average total post-treatment effect.



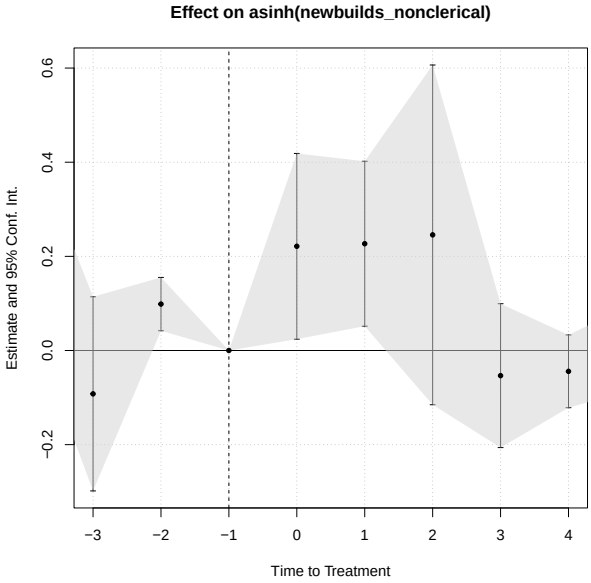
Ecclesiastic to Prince, n = 8,432
t value: .72



Prince to Ecclesiastic, n = 21,760
t value: .37



Republic to Prince, n = 1,071
t value: .78



Prince to Republic, n = 21,437
t value: .70

Figure 1.9: Event Study: Institutional Change and Non-Clerical Construction

The effect of institutional transitions on construction event frequency, using the Sun and Abraham (2021) IW estimator. Relative periods represent 25 year intervals. T value reports the significance of the "Average Treatment Effect for the Treated", the weighted average total post-treatment effect.

growth. The fact that we do not see a matching decline in non-clerical construction suggests that resources are flowing into these cities rather than being reallocated. It is also possible that the Church was very effective at diverting resources away from private projects and towards clerical ones.

The two most promising avenues forward involve looking closer at the circumstances of transition. For example, transitions can be classified into violent or non-violent. Focusing on transitions as shocks would also expand the sample size, since all transitions could be included and not just permanent ones. It is also likely that the impact of transitions varied depending on regional or other factors, the most important of which to explore would be the impact of confessionalization. After the Reformation, the culture and purview of ecclesiastical territories changed drastically.

In the end, it appears there is no reliable causal statement that can be made about the effect of this form of institutional variety in Germany. There are two possible ways to interpret this null result. First, it is possible that the institutional classifications used here are not appropriate for a variety of reasons. For example, following the *Bauernkrieg* of 1525, the constitutions of over 20 republics were amended to reduce the influence of guilds in favor of patricians more amenable to the Emperor (Brady, 1985). Similarly, the Reformation and other pressures altered fundamentally what it meant to be a Prince-Bishop; by the 18th century the titles were held by an increasingly insular and well-connected pool of candidates from preeminent dynasties. Not only were the fundamental characteristics of these groups changing, but election procedures simply may not have been important relative to the other institutional variables.

On the other hand, election procedures were fundamental distinctions that persisted across the entire sample. The robustness of the null result here runs contrary to expectations. This may suggest that democracy has prerequisites that were unmet in this setting. As research develops and richer sets of controls become available for the larger sample of cities

contained within the *Staedtebuch* it may be possible to study the conditions for successful early democratic institutions.

Chapter 2

Fiscal Capacity, Railway Federalism, and German Railway Development 1835-1885

2.1 Introduction

Fiscal capacity is an important determinant of economic development, as increased fiscal capacity allows governments to support the provision of public goods such as infrastructure or contract enforcement.[1] The specific channels connecting fiscal capacity to development are less understood. For example, states face choices about how they choose to utilize their capacity, and it is not *a priori* obvious that state fiscal capacity will support or crowd out private enterprise. On one hand, fiscal capacity allows governments to subsidize, guarantee the returns of, or directly invest in private companies providing key public goods. On the other hand, fiscal capacity can also be used to invest directly in public enterprise or cover the costs of nationalization. In this paper, I explore how fiscal capacity shaped 19th century German states' policies supporting railroad investment.[2] Understanding the drivers of railroad construction in Germany is of particular importance, because, as Fremdling (1977) shows, rail was the leading industry in the industrial revolution that transformed Germany into Europe's manufacturing powerhouse.

Germany is also unique because there was no cohesive central planning of the railroad network. Each German state (e.g., Prussia, Bavaria, Saxony) made their own railroad policies; even after unification in 1871, there was no central railroad authority and attempts to impose one all failed miserably until the first world war. Each state faced its own incentives and constraints that were tied to their political cultures, locations on trade routes, and endowments of key resources that could be exploited in the rapidly globalizing economy.

The common view (e.g, Millward (2004)) that German railways were mostly public hides a substantial amount of regional variation in early railroad policy. While it was true in 1910 that 94% percent of railroads were publicly owned in Germany (the highest among all states

[1] See Besley and Persson (2011); Besley et al. (2013); Dincecco (2011); Dincecco and Prado (2012) and Johnson and Koyama (2017).

[2] For other studies connecting fiscal capacity to railroad expansion, see Bignon et al. (2015) (Latin America), Dincecco and Katz (2016) (Europe), and Bogart and Chaudhary (2012) (India).

studied in Bogart (2010)), there was little indication that public firms would end up playing such a large role when the first railroad line between Nuremberg and Fürth was laid by the private *Ludwigs-Eisenbahn Gesellschaft* in 1835. Prussia, Saxony, and many of the smaller states had entirely private networks until the late 1840s. In fact, the first year that the German state railway authorities would lay more miles of track than private firms would not be until 1850, and private construction outpaced public construction in 37 out of the 51 years between 1835 and 1885 observed in this study.

By tracking what percentage of railroad miles within each state was owned by that state over time, one can see different state ownership policies for railroad construction. The full range of percentages exists in all periods, and while the percentage was increasing over time for most states, it decreased for others. This paper is the first to explore how fiscal capacity may have been a key determinant of ownership policy, and lays the groundwork for more detailed analysis on how that policy may have influenced network efficiency and development in a wider sense.[3]

This paper focuses on a state-level analysis of the effect of fiscal capacity on railroad construction, and crucially it allows private and state railway networks to react differently. By matching entries in primary sources that list which companies constructed each rail line in Germany to existing GIS data of rail line locations, I construct a new dataset that tracks the state ownership policies of the different states over time. Combining this with data on nationalizations and privatizations, it is possible to calculate how many miles of railroads in any state were publicly or privately owned in any given year.

Through dynamic panel regressions based on a partial adjustment model (and an instrumental variable strategy exploiting sequential moment restrictions) I estimate the effect of increased state revenues on the growth rates of the public and private railroad networks

[3]The consequences of such policy for construction costs and operating efficiency in Britain are well explored by Foreman-Peck (1987), but Germany lacks a comparable analysis. Appendix **??** provides some cursory statistics.

within eight of the largest German states. I propose several channels linking state revenues to railroad construction: First, state revenues could be used to directly invest in state-owned enterprise. Second, revenues could be used to subsidize private firms. Though the exact scale of subsidization is currently unknown, there are numerous examples of states guaranteeing returns for investors or simply buying shares directly. Third, revenue growth might make the state a more credible debtor, raising its capacity to issue bonds for railroad construction. I hypothesize that the second and third channels were particularly important, as railway construction required staggering amounts of capital that states simply could not provide on their own.[4]

My results suggest that increases in government revenues are associated with reductions in public construction and corresponding increases in private construction. However, these changes cancel each other out and do not result in any increase to the overall railroad network growth rate. These results are consistent with my hypothesis that the channel leading directly from state revenues into state railroad construction is not utilized. I speculate that this is because year-to-year revenue changes were not large enough to fund new lines for state railways, but were sufficient for insuring private investors against the risks of investing in private firms. It is more difficult to explain why the overall effect on construction is null. If the pace of railroad construction was constrained solely by the availability of capital, having more free cash available for subsidies and interest rate guarantees should have increased private construction while public construction stayed stable. Instead, windfalls predict a substitution.

Dividing state revenues into two components provides possible answers to this puzzle. One of the most important sources of state revenues were the state railroads themselves. When these are considered separately, we see that the substitution effect is driven entirely by non-rail

[4]Using data from Fremdling et al. (1995), one can show that annual investments in fixed capital stock for state owned firms were frequently more than 20% of the size of the states' budgets, and sometimes even two to four times as large. Furthermore, state owned firms only accounted for half of all fixed capital stock until the 1880s.

revenues, i.e., tax and demesne revenues. On the other hand, increases in state rail profits strongly predict increased state railroad construction with little to no countervailing effect on private construction. One possibility is that as state railroads grew, their growth became increasingly self-sustaining. Increased profits mean more capital available for reinvestment. Another possibility is that the increasingly reliable stream of revenues from railroads was easier or more politically acceptable to borrow against than earlier revenue streams. In a final extension, I look at debt capacity directly and find that issuing new debt is strongly associated with increased public construction.

My approach offers a different perspective on fiscal capacity compared to other studies which use only indirect or approximate measures. To my knowledge, the only other estimate of the relationship between fiscal capacity and railroad construction in Europe can be found in Dincecco and Katz (2016), which finds that fiscal centralization and constitutional reform led to greater economic growth. The estimation strategy is not causal, nor can it be applied to the German states, since most of them had already introduced constitutions with parliamentary oversight of the budget in the fallout of the Napoleonic wars. Prussia was one of the only major states in Germany to delay implementing a constitution until the mid 19th century, and fiscal centralization occurred far earlier with the development of cameralism in the 16th century (Cantoni et al., 2024). In an extension, Dincecco and Katz look at tax revenues per capita as a measure of fiscal capacity, but I will show that this is a very incomplete measure for Germany, where much of the innovation in generating state revenues came from state enterprise, as first documented by Fremdling (1980).

Bignon et al. (2015) cleverly estimate plausibly causal estimates of the relationship between fiscal capacity and railroad construction in Latin America by exploiting the fact that the majority of state revenues there came from tariffs. This allows them to address the reverse causality issue arising from the fact that railroad construction encourages economic activity, expanding the tax base. However, this approach also has limited value in Germany, where

trade taxes were typically remitted to central authorities (the *Zollverein*, then the *Reich*). Furthermore, the mechanisms linking fiscal capacity to railroad construction may have been quite different in Latin America and Germany. For one, the scale of subsidization in early 19th century Germany is not well established. Subsidies certainly existed, but the role of government financing is often obscured by many states' tendencies to make direct investment into private companies through the purchase of stock.

The German context is also different because there is little evidence to suggest that a lack of access to capital was ever a serious constraint on construction, and my results provide new cliometric support for this view. Earlier scholars like Fremdling (1983) argue that demand for transportation was sufficient for private investment, and the only real constraint was the hesitancy of the states to grant concessions. Mitchell (2000) emphasizes the fact that railway construction began at a much faster rate in Germany than in France and argues that state intervention was just as likely to have spurred investment as to have constrained the private sector.[5]

My results also speak to the open question of whether public and private firms were complements or substitutes. State and private actors may have very different motivations for laying miles of track. For example, it is generally accepted that the development of state railroads in Eastern Prussia was motivated by military concerns. The state was required to enter the market here, because even with subsidies the lines would not have been profitable enough to entice private investment. On the other hand, private companies focused on constructing lines on established trade routes where high demand was easy to see. While this may have been broadly true, the fact that some states chose not to allow private construction even along high-demand lines in favor of a totally public railway system challenges the generalization that states only got involved for strategic concerns.

[5]State interventions may actually have led to an *overbuilding* of the network, as neighboring states competed to build lines that would attract traffic to routes through their own states and not neighbors. Analyzing the potential costs of this overbuilding and competition is beyond the scope of this paper.

Sections 2 discusses which German states were included the sample. Sections 3 and 4 provide background information on the historical development of the German states' railroad networks and fiscal capacity. Sections 5 and 6 discuss the empirical specification and data. Section 7 reports results and section 8 concludes.

2.2 Sample Selection

The existing literature does not provide a satisfactory explanation for the varying degrees of state involvement in the railway industry. While it is possible that such decisions were largely the result of idiosyncratic ideological differences, this view has been refuted by @millward2013 who emphasizes the role of geopolitics and the desires of states to develop their capacity and control of key industries. Millward also considers the importance of expanding the state's participation in business to raise revenues, similar to @fremdling1980, but suffers from the same problem of basing those conclusions entirely on Prussian data. Furthermore, the literature does not agree on whether the net effect of intervention was typically to facilitate or delay railroad construction.[6]

To better understand how state intervention and state capacity influenced the development of the German railroad network, it is necessary to broaden the usual scope of study, which generally focuses primarily on Prussia, and only occasionally includes Baden, Wuerttemberg, Bavaria, and Saxony (the largest and most influential of the states in the "Third Germany" that divided Prussia and Austria). However, it is also important to establish some criteria for noteworthiness; most of the 44 observed states were insignificant, and many were so

[6]Fremdling (1983) argues that there was more than enough demand for railroads, and the influence of state intervention was purely to slow construction. On the other hand, Heinze and Kill (1988) believe that, at least in the earliest years, raising funding was difficult for both state and private actors. Mitchell (2000) takes a middle position, and argues that the effect of state intervention was ambiguous and could have gone other way depending on the context. Millward (2004) takes a positive view of state intervention, arguing that it occurred in cases when the German states were impatient with the slow pace of private investment. None of these arguments are based on a robust cliometric approach.

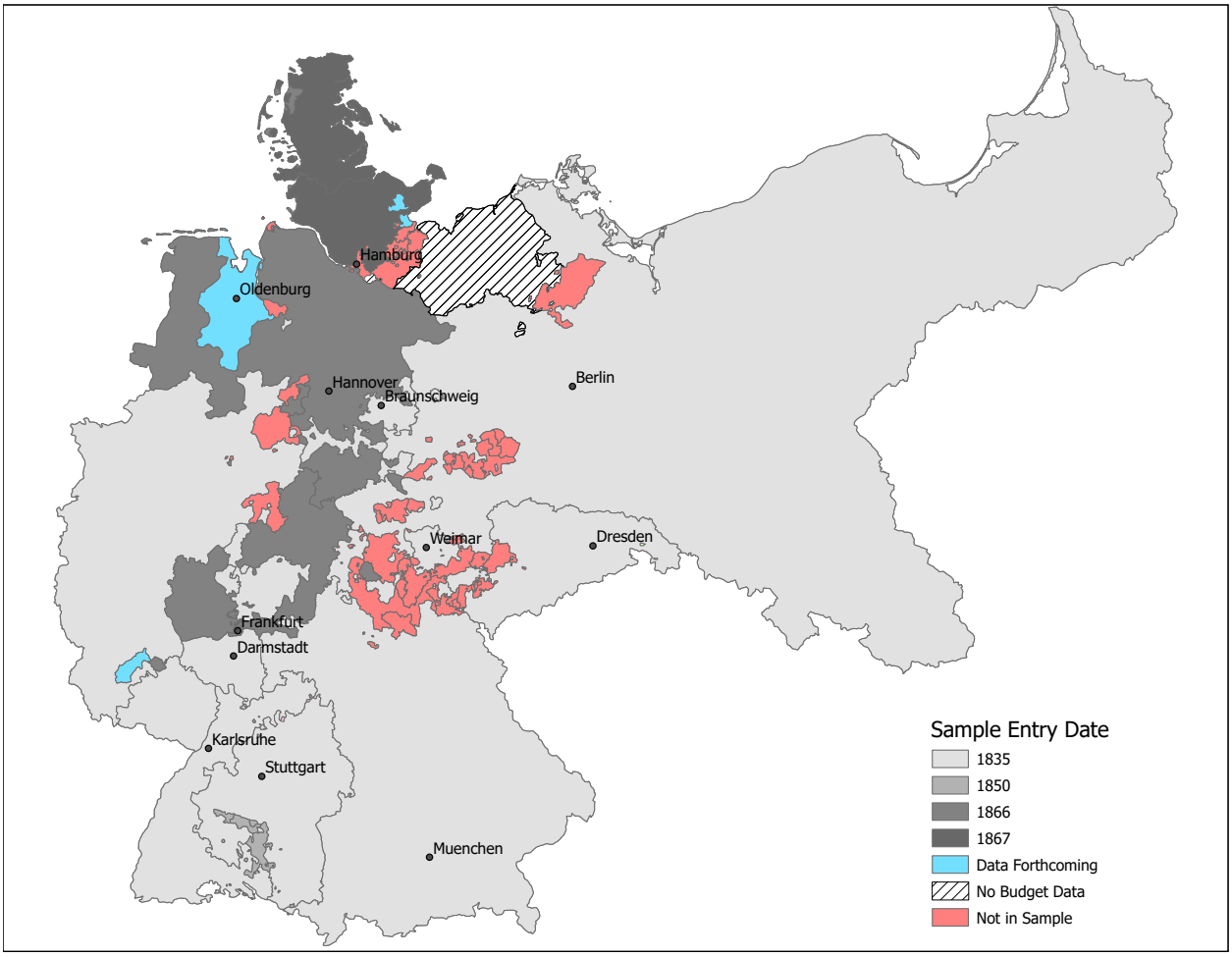


Figure 2.1: Availability of Budget Data

insignificant that they would be annexed before ever constructing a single mile of railroad. Therefore I first applied a simple cutoff: the state must construct at least 150 miles of railroad during the observation period of 1835 to 1885. This gives 11 states: Baden, Bayern, Braunschweig, Hanover, Hessen-Darmstadt, Mecklenburg-Schwerin, Oldenburg, Preussen, Sachsen, Sachsen-Weimar-Eisenach, and Wuerttemberg, whose area makes up 96.4% of the total area of Germany, as shown in Figure 2.1. Due to data constraints, results here are based on an 8 state sample that omits Oldenburg, Hanover, and Mecklinburg-Schwerin. This sample covers 84% of Germany from 1835-1865, then 94% after Hanover is annexed by Prussia.

2.3 Early German Railroad Development

2.3.1 Stylized Facts

The development of the German railroad network is illustrated in Figure 2.2. The first 3.6 miles of track were laid in 1835 between Nuremberg and Fürth by the *Ludwigs-Eisenbahn Gesellschaft*. In the next decade, over 1000 miles of track would be laid, primarily in Prussia, Baden, Saxony. Construction rapidly accelerated, with over 7000 miles built between 1846 and 1865. It has frequently been stated that the initial phase of railroad construction did not lead to any significant new connections, but simply intensified existing trade along major routes(Fremdling, 1983; Heinze and Kill, 1988). By the end of this period, we can see that the major trunk lines had been constructed, but densification had only begun in the Ruhr.

Most trunk lines that needed to cross state borders were not built until the end of this period, as the construction of East-West lines was significantly delayed by particularism and the fear of trade diversion (Mitchell, 2000; HOFFMAN, 1969; Fremdling, 1983). This was exacerbated by the challenges associated with negotiating the treaties required for a railroad to cross a border. Negotiations often took many years and most were not finalized until the 1860s and 70s (HOFFMAN, 1969). It is likely that the proliferation of private firms was in some part due to the fact that joint investment by states was a workable solution when both parties wanted to retain some degree of sovereignty and control; in fact, over 70% of border crossings were opened by private firms. These points highlight the role of federalism in the development of German railways.

Accelerating densification and a reduction in particularism following Prussia's territorial expansion in the aftermath of the Austro-Prussian War and German Unification massively increased the rate of construction in the next decade. Between 1866 and 1875, over 8000 miles were built, doubling the size of the network. In Prussia, densification of the Ruhr network

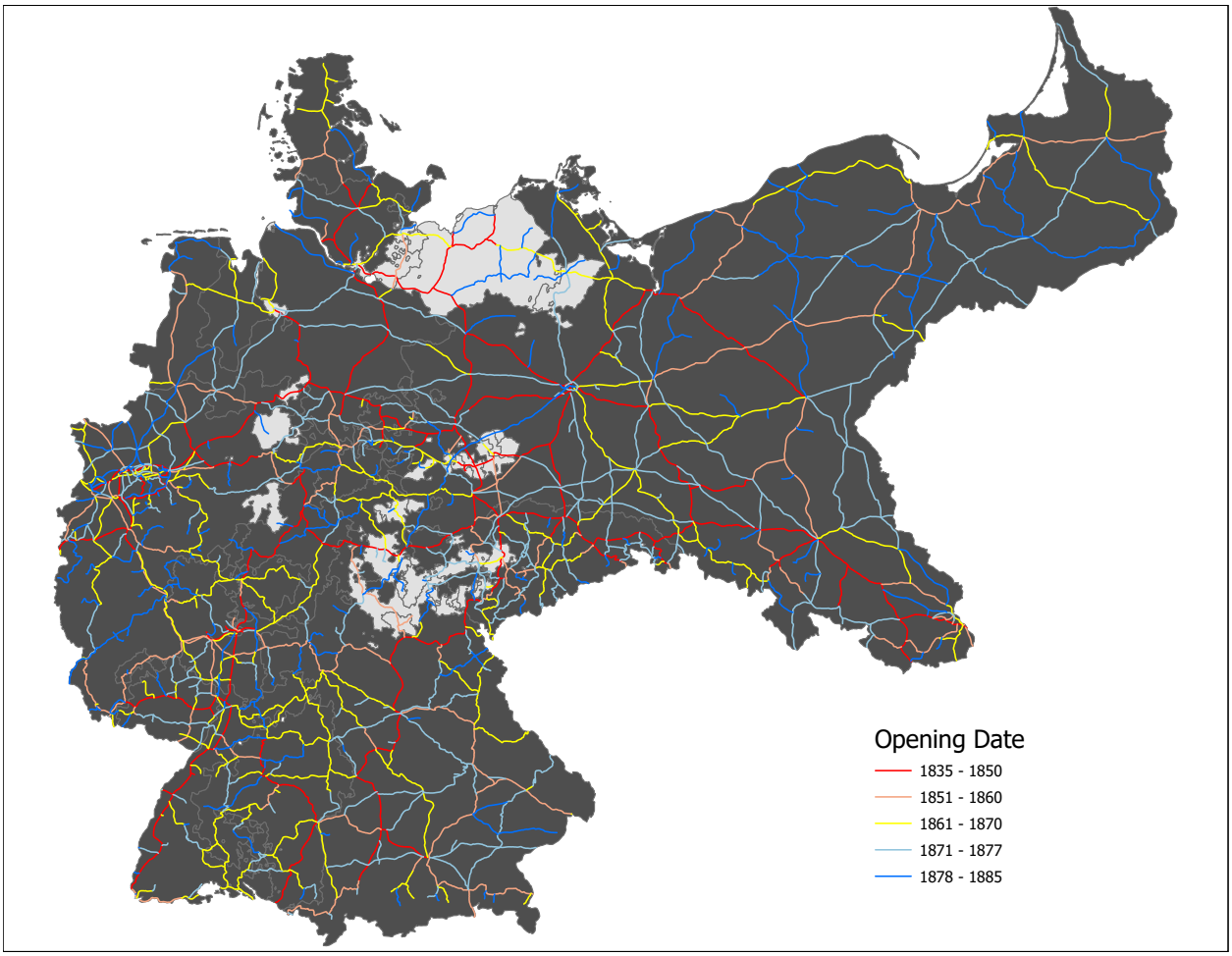


Figure 2.2: Railway Construction, 1835-1885

proceeded at pace, and massive trunk lines connected Berlin and East Prussia. Densification also began in earnest in the southern states. Another 5000 miles were built between 1876 and 1885, mostly increasing density in the remote northern and eastern regions, as well as politically fragmented Thuringia.

Table 2.1 presents the size and density of the ten largest state railway networks in 1885. One can see that, although the absolute size of the networks varies substantially, the density measures all fall within a reasonable distance of each other. This is not to say that all states achieved the same level of density; whether measuring density by area or per capita, the most dense network is roughly twice as dense as the least dense and the remainder fall relatively evenly between these two extremes.

Table 2.1: State Railroad Network Size

State	Miles	Rank	Per Capitax1000	Rank	per square mile	Rank
Prussia	12994	1	.46	8	.10	8
Bavaria	2817	2	.54	4	.10	7
Saxony	1118	3	.35	10	.19	1
Wuerttenberg	840	4	.42	9	.11	5
Baden	751	5	.47	7	.13	4
Hessen-Darmstadt	475	6	.50	6	.16	2
Mecklinburg-Schwerin	374	7	.65	1	.08	10
Oldenburg	219	9	.64	2	.09	9
Brunswick	214	10	.58	3	.15	3
Saxony-Weimar-Eisenach	156	11	.50	5	.11	6

2.3.2 Public and Private Railways

To analyze how ownership policies varied across Germany and within states it was necessary to construct a new dataset. Before describing the construction of that dataset and the stylized facts that emerge, it is important to clarify how railroad mileage is divided into different categories, and why those categories mattered. Contemporary sources such as Vereins deutscher Eisenbahn-Verwaltungen (1868) and Kühn (1883) divided firms into three

types: First were the *Staatsbahnen*, or state railroads, which would later become known as the *Länderbahnen*. These were fully incorporated into the bureaucracy of the state; administration was handled by the state and profits were treated as state revenues. Second were the *Privatbahnen*, or private railways.

It is important to emphasize that public and private railways were financed differently. State railroads were primarily financed by the issue of bonds. These bonds established railway construction funds administered by the relevant ministries in charge of regulating railways. Further bonds could be issued as desired to provide new capital injections. The interest payment on these bonds was part of the ordinary expenses of the state, and in almost all cases the profits of the state railroad system were treated as ordinary revenue (the exception being Baden, where railroad revenues were part of a separate "Special" budget). Private railroads, on the other hand, were financed by the sale of stock. This allowed private capital to supplement government debt. von Mayer (1891) writes that granting concessions to private firms to sell stock was particularly common in the early years of construction, when states were more fiscally constrained. As time went on, and states had more reliable income, but developing new lines became increasingly risky, states had two tools they could use to encourage additional private capital investment in railroads. First, they could purchase shares of stock themselves, supporting asset prices and signaling the soundness of the investment, as well as the state's desire to keep that particular line running, perhaps through favorable legislation or additional support. Second, they could offer guaranteed returns on investment, socializing all of the risk if the line turned out to be unprofitable.

States were keenly aware of the tradeoffs they faced when deciding whether to bear the burden of financing railroads themselves or seek out private capital. In the stenographic records of the Bavarian parliament from 13. July 1850 (Landtag, 1850), we can find a spirited debate over whether state investment in railroads was responsible for problematic

budget deficits. The first camp argued that state investment could not be responsible for the deficit because past investment in railroads creates income without further spending, since the railroads are profitable. As the proposed budget under discussion does not include any funds for state construction, it is instead the recurring funds that must be held in reserve for guaranteed returns to private investment that create persistent deficits. The second side retorted that the deficit would be smaller if no money was ever spent on railroads, since the state could have easily attracted foreign capital to cover all construction costs with small guarantees and instead used all of the money it spent on construction for more fruitful pursuits. This debate highlights that, in addition to being uncertain of which avenue was truly more efficient, Bavaria also faced binding short run budget constraints that it preferred not to resolve by simply issuing more debt.

A complete classification of all German railroads must also note that private railways were further subdivided into those administered by a state bureaucracy (which typically fluctuated around 10% of total railway mileage), and those which were administered privately. To my knowledge, there is no strict definition of "state administration", but we can understand through an example:

The *Bergisch-Märkische Eisenbahn-Gesellschaft* was founded in 1843 as a private railway, with a quarter of the founding capital provided by the Prussian government. In 1849, the railway failed to repay a government loan, and the Prussian government would not agree to provide further loans unless the company agreed to turn over operations to the Prussian state Grün (2001). From then on, the state handled operation of the railway, and presumably profits were distributed to shareholders through dividends; there is no evidence that profits from this firm were treated as part of the state's budget in the way that state owned firms were. Unfortunately, I have also not found any evidence to suggest whether operating expenses were borne by the state or if the state was compensated as if it were a contractor.

For my analyses, I do not retain the distinction between publicly and privately administered private railways to emphasize two points: First, granting a concession to a private company implies the state either needed or preferred for the line to be financed at least partially by private capital, even though it had the administrative capacity to run the line. Second, the budget of a private firm is at arms length from the state it is located in, even when it is publicly administered. Because there is no data on the shares of stock owned by each state, it is impossible to know precisely how the performance of private firms would have influenced state budgets.[7]

In the empirical analysis, it is important to recognize to recognize that state-owned companies also operated in other states. For simplicity, I use "State" miles to refer to *state owned* railways operating *within that state* (e.g., the Bavarian State Railway constructing miles within Bavaria). In contrast, "Other" miles refer to state railways operating in some *other* state (e.g., a Prussian state railway operating within Oldenburg). Thus, the ownership of rail lines fits into one of three categories: State, Private, and Other.

The railway GIS data used to classify mileage by type begins with shapefiles from Kunz and Zipf (2008). The railroad shape files within do not contain any qualitative data about individual line segments besides the year of opening. Thus, it is necessary to match these segments to a different source with qualitative information. The primary source is Dumjahn (1984), which notes the company responsible for construction of the segment, the length of the segment, and, if the segment was nationalized, by which state and in what year. Matching was successful in nearly all cases, and the remainder were matched using Kühn (1883). It is important to note that the methods used by Kunz and Zipf (2008) to simplify the drawing of the rail network introduce minor inaccuracies. For example, no distinction

[7]The complexity of untangling the flow of funds between governments and JSCs is highlighted by an example that can be found in Fremdling and Knieps (1993). Prussia purchased about 15% of the outstanding stock of the Cologne-Minden railway in 1843, while also guaranteeing a minimum dividend to the other investors in exchange for a share of excess profits (among other privileges). The complexity of the relationship and lack of accounting data make it impossible to determine the direction of the net flow of capital between the state and the company.

is made between different stations of most cities (with the exceptions of Berlin and some major industrial cities on the Rhine). The effect of the measured length of track is negligible, but this does ignore the important costs of trans-shipment from one company's station to another that often existed before the construction of smaller connecting lines as the number of companies fell. Furthermore, Kunz generally omits small lines connecting city centers to ports, as well as all of the narrow gauge track (roughly 300km) which was utilized primarily used for short haul industrial purposes (typically under 20km).

It is also important to note that Dumjahn does not record privatizations or mergers. Mergers or sales of lines from one private firm to another are not coded, but it is important to note privatizations. Kleeberg (1990) provides data on the timing of the privatization of the Brunswick state railways and eventual sale to the Prussian state railways. Reliable information on the privatization and renationalization of railways in Mecklenburg-Schwerin is harder to find, and not all sources agree on the timing. I follow the timing given by Fremdling and Kunz (2011). I am not aware of any other privatizations.

Figure 2.3 presents which types of firms constructed the railroads in each state. Because policies varied from state to state, an individual treatment is necessary to explain contextual factors; this is presented in Appendix B. However, some broad patterns do emerge. State construction was much more widespread in the South, which likely contributed to the delay in East-West lines connecting the southern states. These were the states whose administrations were most intensely particularistic and least willing to make any concessions that could be considered ceding sovereignty. It is harder to make generalizations about the connection between industrial regions and state construction from a purely visual inspection. While Prussian state intervention was common in the Saar and Silesia, railroads in the Ruhr were constructed almost entirely by private firms. Discontiguous territory, however, appears to be highly correlated with favoring private firms. With the exception of the Saar, and some connections with Hannover, railroad construction in the Rhineland and West Prussia is

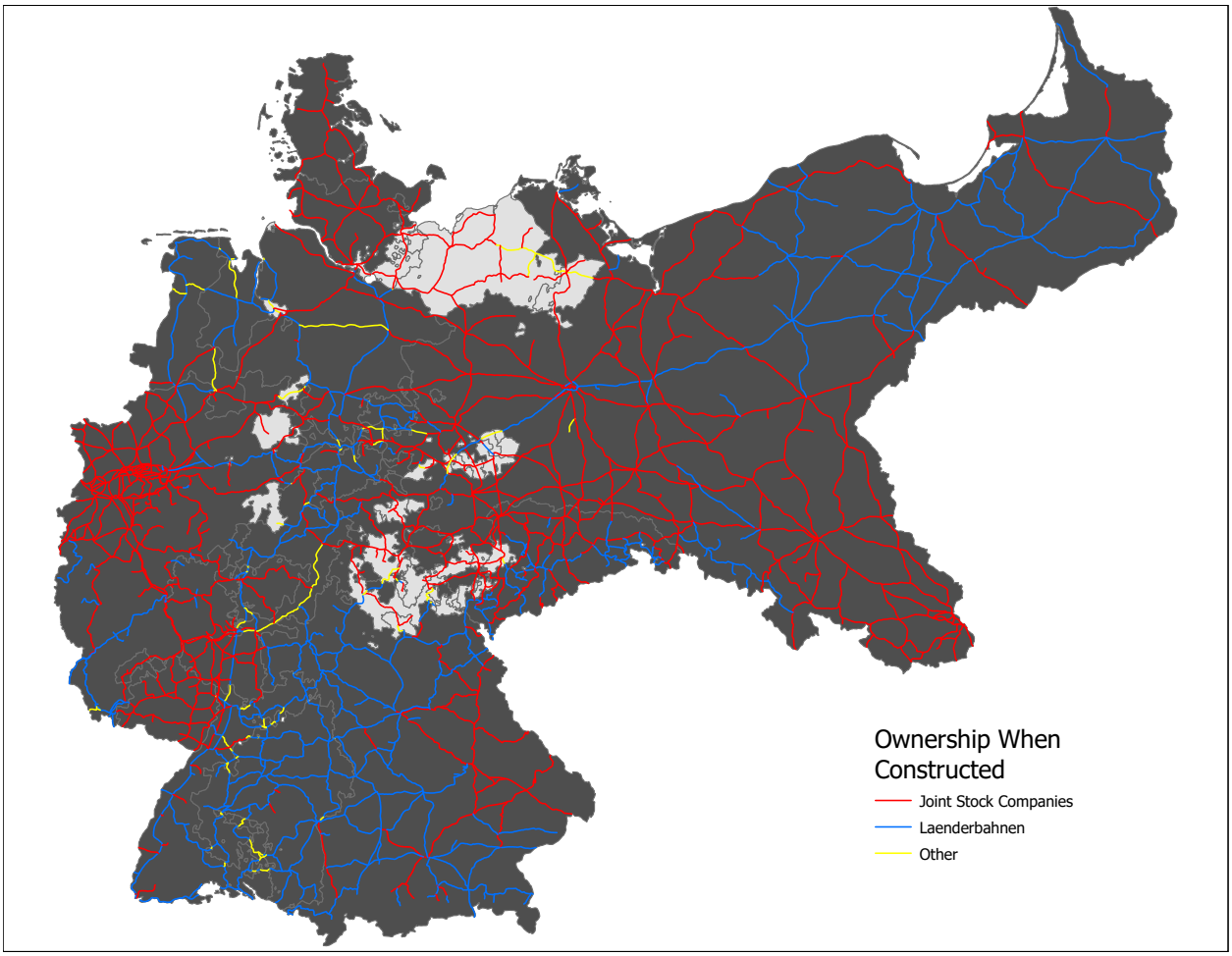


Figure 2.3: Railway Ownership at the Time of Opening

dominated by private firms. Hessen-Darmstadt's territory was also divided in two, and her network was primarily constructed by private firms as well. Bavaria and Oldenburg preferred state construction in their core territory, but left construction in their exclaves exclusively to private firms.

Despite these observable trends under specific circumstances, and in particular because a "one size fits all" explanation that state railways emerged in regions with infant industries does not match our observations, a substantial amount of variation in state railroad construction within and across states remains unexplained. Explaining how a portion of that variation is explained by fiscal capacity is the main focus of this paper.

A digression on nationalizations is also necessary, as nationalizations may play an important role in determining state railway profitability. It has been shown by Bogart (2010) that state intervention though nationalizations had differing effects from state intervention by construction. The effect of nationalization is somewhat difficult to account for in this study for two reasons. First, most nationalizations occurred close to the end of the observed period of railroad construction, if not later, as shown in Figure 2.4. The early nationalizations in Bavaria and Saxony initiated state involvement, but only led to the wider development of state firms rather than general policies of nationalization. Second, nationalizations were largely a reaction to exogenous political pressure. Prussian attempts to create a national railway administration were complete failures and led to fears among the smaller states that Prussia would create a national administration indirectly by purchasing the remaining private railways and absorbing them into the Prussian State Railways (as would occur in Brunswick). To preempt this possibility, the south German states nationalized most of the remaining private firms within their borders in 1876. In the coming years, Prussia would begin a massive nationalization campaign to bring nearly all of its existing mileage under state ownership. After a few late nationalizations, (Mecklenburg-Schwerin in 1890, Hessen-Darmstadt in 1897, and the Bavarian Palatinate in 1908), the State Railways would control

nearly all mileage in the Empire until they were finally united under the *Deutsche Reichsbahn* in 1920.

For the purposes of this paper, nationalizations are only considered to the extent that they adjust the percentage of railroad mileage owned by the state in a given year in the descriptive results. Future research may consider why the first wave of nationalizations in the 1840s and 1850s occurred instead of states simply granting financial support in the form of subsidies; this is a particularly interesting question since it may have been the catalyst for the expansion of state owned railroad systems in Prussia, Bavaria, and Saxony. The second wave of nationalizations, being exogenously determined by political factors (at least in Bavaria, Hessen, and Saxony), may prove useful as a natural experiment for studying the efficiency of state administrations.

2.4 Fiscal History

2.4.1 New Data

Though some qualitative histories about the development of public finance in Germany exist, comprehensive quantitative data has heretofore been sparse. Andic and Veverka (1963) only collected data on the expense side. The first effort to compile state income was made by Mauersberg (1988), who made no attempt to ensure the consistency of variable definitions across observations or time and simply reported the given figures in the regular budgets. Thus, Mauersberg's data is biased by changes in reporting from Net to Gross revenues, the occasional inclusion of fees and fines within direct taxation, and the definition of the "regular" budget, among other idiosyncrasies. Furthermore, Mauersberg does not discuss his choice of sources, leaving it up to the reader to determine which figures are projections and which are based on draft proposals rather than approved laws. Most importantly, Mauersberg did not

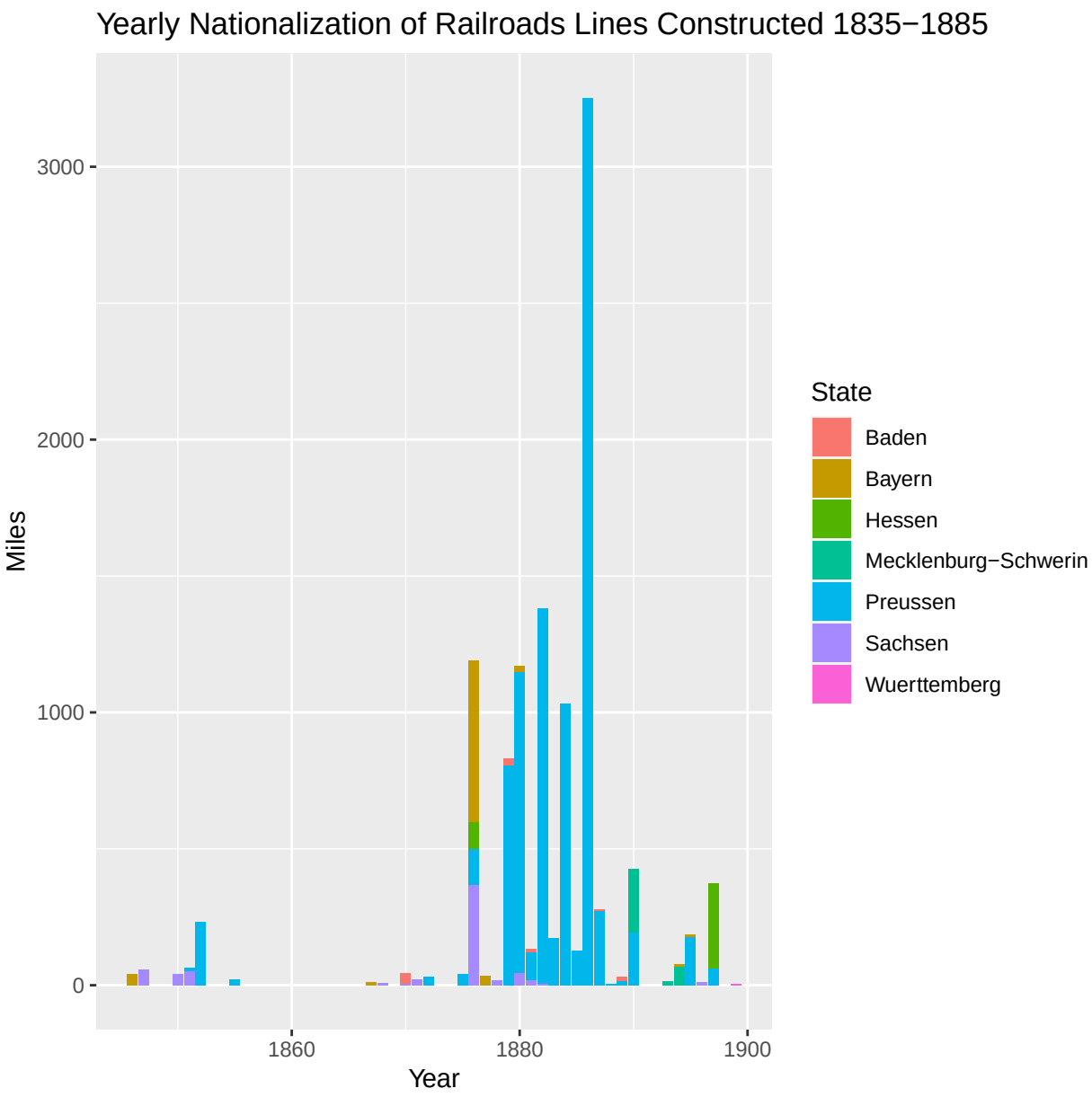


Figure 2.4: Timing of Railway Nationalizations

exhaust available sources and only included a limited sample of states and years, severely hampering the data's usefulness for a quantitative study.

In my dataset, I do my best to remedy these flaws, though as we will see, limited documentation surrounding the primary sources presents a challenge. The first problem confronting the researcher interested in compiling 19th-century budget data is one of scale. There is an over-abundance of data; the tables and appendices to a "*Finanzgesetz*" (the law establishing the budget for a financial period) frequently number in the hundreds of pages. To create a complete panel of all income and expenditure would be a herculean effort. Thus, the first step is to decide on a limited number of "high-level" categories that can be consistently measured across space and time.

For the current iteration of this project, I collect data on Total Income and three major income categories: Direct Tax, Indirect Tax, and Railroads.[8] Where possible, I also collect data on the associated operating expenses, which allows gross income figures to be converted to net and vice versa. When income is disaggregated, the remainder is made up of income from royal properties (such as forests), state enterprises and monopolies (such as salt mines and the post), administrative fees, legal fines, transfers from state coffers (i.e., liquidation of assets), and other miscellaneous sources such as reparations.

Generally, contemporary definitions of direct and indirect taxation are consistent with modern expectations. Direct taxation is primarily made up of property, poll, income, and capital taxes whereas indirect taxation is primarily made up of customs, stamp taxes, and taxes on luxury goods. While direct taxation figures can usually be taken as given, there are two main challenges to constructing the indirect taxation figures. The first is that while indirect taxation was administered by the states, they were often remitted to a central adminis-

[8] An important omitted category is debt, which is unfortunately beyond the current scope of the project. While debt servicing expenses are typically reported consistently and simply as a line item, there is little consistency in the reporting of debt *issue*. Consequently, the consideration of debt as an income source needs to be left to future research.

tration to be redistributed (first the *Zollverein*, then the *Norddeutscher Bund*, and finally the *Reich*). The reporting surrounding these redistributions is opaque and it is not always clear whether the reported figures represent the pre- or post-redistribution figures. Second, there was a tendency to report fines and penalties as indirect tax revenue. While this is arguably appropriate for customs violations, it is less clear whether fines for illegal activity fit the modern conception of indirect taxation. However, because it is generally impossible to exclusively remove civil and criminal fines only, these costs remain in indirect taxation.

Defining railroad income faces similar challenges. First, I make a judgment to include all railroad related income, exclusive of specifically labeled direct "railroad taxes". Thus, railroad income includes not just profits from state enterprise, but also other miscellaneous income generated by the ministry governing railroads. This includes income from direct investments, privatization, and other general fees. The remaining challenge regarding railroad income is related to the lack of standardized reporting for the issuance of bonds. Because bond issuance is often unreported in the regular budget, this significantly overestimates net income from railroads, as debt repayment related to those bonds is folded into the general debt servicing line item.

The first choice of primary sources for budget information are the *Gesetzsammlungen* (Law Collections) or *Gesetzblätter* (Law Gazettes). These were the official communication channels of the states to notify citizens of new laws. The budgets presented here are preferred, since they represent a fully approved law which is no longer subject to any further debate or amendment. However, it is important to note that these were still projected figures rather than an accurate accounting of real income. An additional drawback is that many states only reported abridged budgets in these public-facing documents. For example, in Bavaria's case, between 1820 and 1871, the *Gesetzblätter* only report net income, giving an incomplete picture of state fiscal capacity.

In such cases, the next choice is to examine parliamentary proceedings. Budgets submitted

for parliamentary approval typically contain far more information but are of reduced value because the figures are still subject to approval and amendments. If neither gazettes nor parliamentary proceedings are available, then secondary sources are used. Appendix C describes the sources used for each state. I convert all currency units to Marks, using the 1871 exchange rates of 1 thaler = 3 mark and 1 gulden = 12/7 mark (Bundesbank). Appendix D presents key stylized facts.

2.5 Empirical Strategy

My theoretical framework begins with the assumption that railroad construction follows a partial adjustment model.[9] In this framework, the state acts as a planner who observes the current state of the economy then decides what the optimal railway network size is. The state also has final say of how many railroad miles may be built. Private actors are able to make proposals, but ultimately the existence of all lines must be approved by the state. Thus, the evolution of the network is described by the primitive function:

$$LogRailroadMiles^*_{i,t} = R^*_{i,t} = \beta_0 + BX_{i,t} + \alpha_i + \nu_t + \epsilon_{i,t}$$

However, because proposing and constructing new lines is time consuming and furthermore there may not be sufficient capital to construct all the desired lines, the state does not instantly move to its preferred amount of railroad miles $R^*_{i,t}$. Instead, the network will partially adjust with speed δ according to the following relationship:

$$R_{i,t} - R_{i,t-1} = \delta(R^*_{i,t} - R_{i,t-1})$$

[9] My approach is similar to the model used in Bignon et al. (2015).

Substitution results in the standard estimating equation for partial adjustment models (which is typically then log transformed):

$$R_{i,t} - R_{i,t-1} = \delta\beta_0 - \delta R_{i,t-1} + \delta BX_{i,t} + \delta\alpha_i + \delta\nu_t + \delta\epsilon_{i,t}$$

Recall that so far I have classified railroad miles as (local) State, Other (state), and Private, according to their ownership and whether they were located in the state that owned them. To emphasize that states must make a decision to either construct miles through their own enterprises or grant concessions to outside actors, I group "Other" and "Private" into "Non-State". To model this decision to assign desired miles to State or Nonstate lines I approximate the log-differenced partial adjustment model with a growth rate that can be disaggregated without changing the interpretation across models[10]:

$$log(R_{i,t}) - log(R_{i,t-1}) \approx \frac{NewMiles^{Y}_{i,t}}{R^{Total}_{i,t-1}}$$

Where $NewMiles^{Y}_{i,t}$ is the number of miles constructed during year t by $Y \in \{Total, State, NonState\}$ firms. It is important to note that this measure only captures changes due to construction of new lines. Nationalizations are not considered new state miles. This is a deliberate choice, since the majority of nationalizations prior to 1885 were due to exogenous political shocks and not revenue concerns (as discussed above). Thus, we can write the baseline specification being estimated as follows:

$$\frac{NewMiles^{Y}_{i,t}}{R^{Total}_{i,t-1}} = \delta\beta_0 - \delta R_{i,t-1} + \delta\beta_1 Govt.Rev_{i,t-1} + \delta\beta_2 Population_{i,t-1} + \delta\alpha_i + \delta\nu_t + \delta\epsilon_{i,t}$$

[10]That is to say, $ln(A+B) - ln(C) \neq ln(A) - ln(C) + ln(B) - ln(C)$ whereas $\frac{a+b}{c} = \frac{a}{c} + \frac{b}{c}$.

The variable of interest across specifications is $Govt.Rev_{i,t-1}$, which is measured either as the sum of all government revenues less collection costs, or a disaggregated measure that separately estimates the effect of profits from state railroad enterprises.

To ensure that the LHS accurately approximates the log-differences and to limit the influence of outliers, we do not want to include years where $\frac{NewMiles_{i,t}^{Total}}{R_{i,t-1}^{Total}} > 1$, as relatively small amounts of construction can lead to abnormally large expansion rates in the early years of construction. We also do not want values from these years to be used as instruments, so the final sample begins with the fifth year after the last year with a growth rate greater than one. The median number of years dropped is 13 and the max is 16. Less than 8% of total mileage was constructed in the omitted years. It is also more believable that δ had a consistent value over this shortened period.

2.5.1 Identification Strategy

Because of the obvious feedback effects between railroads, the economy, and government revenues, OLS regression results are immediately suspect. To address the endogeneity problems, I adapt IV strategies from the dynamic panel modeling literature (the most common treatment of these methods can be found in Wooldridge (2002)). The identifying assumption is that the RHS variables are "sequentially exogenous", that is:

$$E(\frac{NewMiles_{i,t}^{Y}}{R_{i,t-1}^{Total}}|X_{i,t}, X_{i,t-1}, ..., X_{i,1}) = E(\frac{NewMiles_{i,t}^{Y}}{R_{i,t-1}^{Total}}|X_{i,t})$$

Where $X_{i,t}$ includes existing railroad mileage, government revenues, and economy variables. Put simply, this means that the history of the endogenous system is irrelevant and current growth rates are fully determined by the current state of the world. In my model of railroad construction, this means that the state does not care about past values of income and only

cares about projected revenues in the year that construction is occurring. This assumption is violated in years with budget surpluses, but surpluses are in fact quite rare.

Because historical values of endogenous variables do not determine current values of y, but are good predictors of the current values of endogenous variables, this makes them valid and appropriate instruments. The typical approach here is to use the so called Arellano and Bond estimator which maximizes the number of lagged values of endogenous variables. Due to the nature of my panel (N groups T years) this leads to an overidentification issue. Instead, I use only the values of endogenous variables from $t-3$ and $t-4$.[11] Wald tests show that this is enough instruments to avoid a weak instrument problem while Sargan tests show that there are few enough to avoid overidentification.

2.6 Summary Statistics

2.6.1 Railway Ownership

I construct a panel of railroad miles constructed by each company in each state, as well as a running measure of what percentage of existing mileage is owned by each set of actors in any given year. This running measure of mileage is adjusted to account for nationalizations, privatizations, and annexations so that $Miles_t - Miles_{t-1}$ can be different from $NewMiles_t$ which only measures construction. Summary statistics for the total construction and the expansion rates ($\frac{NewMiles^{Y}_{i,t}}{Miles^{Total}_{i,t-1}}$) in the unbalanced sample panel are displayed in Table 2.2. Summary statistics for individual states and all years are included in Appendix B.

[11] Lags $t-1$ and $t-2$ are not valid instruments due to Nickell bias.

Table 2.2: Summary Statistics: Railroad Construction

Statistic	N	Mean	St. Dev.	Min	Median	Max
TotalRate	298	0.06	0.09	0.00	0.02	0.65
StateRate	298	0.02	0.05	0.00	0.00	0.37
NonStateRate	298	0.03	0.08	0.00	0.000005	0.65

2.6.2 Public Finance

The public finance data panel is generally balanced, with a few exceptions. In the sample period from 1835 to 1885, only 23/357 rows are missing total net income data. Because there is generally no reason not to, I interpolate the missing values. This is most likely harmless, although there is likely some small upward bias from interpolating the often missing budgets during the crisis years of 1848-1849. Summary statistics for the final panel are presented in Table 2.3.

Table 2.3: Summary Statistics: Public Finance and Economic Activity

Statistic	N	Mean	St. Dev.	Min	Median	Max
NetIndirectTaxRev	298	16,605,610	23,197,881	297,745	7,903,325	104,063,607
NetDirectTaxRev	298	20,093,180	34,176,058	1,168,399	7,333,579	151,563,350
RailIncome	298	10,339,136	21,532,065	0	3,480,564	202,458,916
NetOtherRev	298	26,661,667	49,790,361	−252,052	8,474,962	269,761,656
NetGovt.Rev	298	73,656,569	112,958,943	3,060,476	28,700,573	617,007,982
Debt.Service	298	15,816,971	24,383,226	328,484	3,083,940	182,322,836
Population	298	4,418,730	7,136,182	262,524	1,696,430	28,318,470

Sources: Population from Kunz and Zipf (2008).
See text for Public finance statistics.
Im-Pesaran-Shin tests show that none of the variables have a unit root when measured in logs except for debt service payments. After differencing, the debt service series is also stationary.

It is also important to include controls that account for the general level of economic and commercial activity, since these influence both the supply and demand for railroads, as well as the size of the tax base. I code a dummy variable for the common shocks of war. The war dummy variable is based on Clodfelter (2017), who mentions specifically the major

participants in 19th century wars in Western Europe. The state of the economy is simply proxied by state population from Kunz and Zipf (2008).[12] Extensions use data on industrial production from the same source.

2.7 Results

2.7.1 Baseline

Table 2.4 presents the baseline results when all government revenue sources are pooled. First, note that the coefficient on $TotalMiles_{i,t-1}$, which is the δ coefficient measuring the speed of convergence, gives a very reasonable value. Each year, the network gets roughly 20% closer to its conditionally optimal size. The slight majority of this convergence is due to nonstate construction, which is appropriate since private firms constructed slightly more than half of all miles constructed between 1835 and 1885.

Population does not seem to be a consistent driver of within-state variation in railroad construction in either direction, though this is perhaps unsurprising since population growth tended to be stable across Germany in this period, except in Prussia which experienced a major population shock after annexing Hanover and other states following the 1866 Austro-Prussian War.

Interestingly, government revenues do not have a significant relationship with overall railroad network growth rates. Even more surprisingly, the IV specification suggests a slightly negative relationship. One possibility is that a major driver of railroad construction was the need to raise government revenues. It is possible that as revenues increased this demand

[12]Kunz also includes data on production of a subset of industrial goods, however, the limited sample of goods included leads to significant bias when trying to include this data, limiting its value in this application. Lack of good data on state level productivity remains a significant bottleneck for this project.

fell proportionally, since the constitutional structure of many German states tended to limit revenue generation to only what was needed to cover planned expenses.

The more interesting result appears in columns 3-6, which show how construction was allocated to state or nonstate firms. The results are highly significant and show that revenue increases led to a reduction in state construction and corresponding increase in nonstate construction. It appears that as government revenues increased, regular budget spending on state railroad firms was reduced in favor of granting subsidies or other support to private firms.

However, this is not the only explanation. Another explanation could be that as revenues increased, so did the ability of the state to raise debt. This idea is explored more in the next section. First, I consider what happens when disaggregating revenue into rail and non-rail sources.

We can see in Table 2.5 that disaggregating revenue tells a consistent but slightly more nuanced story. Nonrail incomes still seem to lead a switch from state to nonstate construction, further supporting the hypothesis that regular revenues were more easily used on subsidies or interest rate guarantees to support private construction than transferred to state firms. Rail incomes, unsurprisingly, predict increased state construction. The more profitable the state enterprise was, the more it expanded.

2.7.2 Extension: Debt

Since railroad projects were exceptionally capital intensive projects, it is unreasonable to think that marginal year-to-year increases in the regular budgets of any German state would be sufficient to fund new railroad lines. Thus, any complete study of railroad construction should also account for the issuance of railroad debt, typically in the form of bonds.

Table 2.4: Baseline Results

Dependent Variables:	TotalRate		StateRate		NonStateRate	
	OLS	IV	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$TotalMiles_{i,t-1}$	-0.1991***	-0.2346***	-0.0742**	-0.1119***	-0.1249***	-0.1227**
	(0.0420)	(0.0505)	(0.0304)	(0.0282)	(0.0390)	(0.0463)
$Population_{i,t-1}$	0.0440	0.0200	0.0339	0.0242	0.0101	-0.0042
	(0.0697)	(0.0756)	(0.0426)	(0.0500)	(0.0538)	(0.0546)
$Govt.Rev_{i,t-1}$	-0.0046	-0.0217	-0.0573***	-0.0957***	0.0527**	0.0740*
	(0.0382)	(0.0525)	(0.0206)	(0.0284)	(0.0260)	(0.0391)
War	-0.0295	-0.0380	-0.0053	-0.0120	-0.0242	-0.0260
	(0.0270)	(0.0298)	(0.0194)	(0.0190)	(0.0157)	(0.0193)
Fixed-effects						
State	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	298	298	298	298	298	298
R^2	0.43936	0.43551	0.25452	0.23810	0.51789	0.51683
Within R^2	0.14527	0.13940	0.06478	0.04419	0.10486	0.10290

Driscoll-Kraay (L=2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Note: Italicized variables have been asinh transformed.

Table 2.5: Baseline Results - Disaggregated Income

Dependent Variables:	TotalRate		StateRate		NonStateRate	
	OLS	IV	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$TotalMiles_{i,t-1}$	-0.2065***	-0.2651***	-0.0763**	-0.1408***	-0.1302***	-0.1243**
	(0.0492)	(0.0537)	(0.0285)	(0.0313)	(0.0428)	(0.0505)
$Population_{i,t-1}$	0.0365	-0.0006	0.0103	-0.0284	0.0262	0.0279
	(0.0741)	(0.0929)	(0.0351)	(0.0596)	(0.0675)	(0.0659)
$NonRailIncome_{i,t-1}$	0.0113	0.0284	-0.0336	-0.0428	0.0449*	0.0711*
	(0.0384)	(0.0679)	(0.0249)	(0.0477)	(0.0243)	(0.0412)
$RailIncome_{i,t-1}$	0.0008	0.0027	0.0004	0.0030***	0.0004	-0.0003
	(0.0016)	(0.0021)	(0.0009)	(0.0011)	(0.0014)	(0.0020)
War	-0.0234	-0.0191	-0.0076	0.0014	-0.0158	-0.0206
	(0.0241)	(0.0328)	(0.0221)	(0.0290)	(0.0179)	(0.0254)
Fixed-effects						
State	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	298	298	298	298	298	298
R^2	0.44007	0.43219	0.24596	0.21584	0.51637	0.51399
Within R^2	0.14636	0.13433	0.05405	0.01626	0.10204	0.09762

Driscoll-Kraay (L=2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Note: Italicized variables have been asinh transformed.

Unfortunately, data on the debt of German states is limited. Laws for the sale of particular bonds were idiosyncratic, which makes data difficult to collect. Furthermore, none of the states here studied published figures for outstanding debt in their budget reports (although data for some years can be found in British statistical abstracts). The only consistent time series data available is data on debt servicing payments.

Including debt service payment data leads to additional empirical issues. To remain consistent with the baseline models and identification strategy, it would be necessary to include debt service payments in levels. However, this series has a unit root. Taking first differences resolves this problem, but introduces a weak instrument problem since lagged levels of the other variables in the system do not predict debt service differences.

Consequently, the only statistically valid approach is to treat differenced debt service payments as exogenous. Fortunately, this is not as strong an assumption as it might seem at first glance. There are many arguments in the literature that the main determinant of early modern states' ability to issue debt was institutional. That is, politics and institutions constraining the state and demonstrating a commitment to repay debt may have been more important for determining the ability to issue debt than actual revenue streams. Furthermore, debt is much more dependent on the state of international financial markets. Barring any serious deficit crisis, the decision to issue new bonds might therefore be plausibly exogenous and determined more by the political will of the government to issue debt and its perceived credibility to not default. Regressions based on this assumption are presented in Tables 2.6 and 2.7.

Again, the results suggest that as government revenues increased, there was a shift away from state construction towards nonstate construction. The magnitude of these estimates is similar to those in Tables 2.4 and 2.5.

The coefficient on $\Delta Debt$ is positive and significant. If bonds were being issued faster than

they were being paid off, this predicts an increase in state railroads. Of course, since a large proportion of the increase in state debt service payments was the issuance of bonds specifically to fund railroad construction, this relationship is largely mechanical.

The results here are still preliminary, and a more detailed exploration of the relationship between government revenues and debt is required.

Table 2.6: Exogenous Debt Results

Dependent Variables:	TotalRate		StateRate		NonStateRate	
	OLS	IV	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$TotalMiles_{i,t-1}$	-0.1999***	-0.2335***	-0.0756**	-0.1114***	-0.1243***	-0.1221**
	(0.0419)	(0.0504)	(0.0303)	(0.0285)	(0.0391)	(0.0463)
$Population_{i,t-1}$	0.0439	0.0211	0.0337	0.0254	0.0102	-0.0043
	(0.0699)	(0.0758)	(0.0429)	(0.0500)	(0.0539)	(0.0549)
$Govt.Rev_{i,t-1}$	-0.0050	-0.0251	-0.0579***	-0.1003***	0.0529*	0.0752*
	(0.0378)	(0.0509)	(0.0197)	(0.0278)	(0.0264)	(0.0405)
$\Delta Debt_{i,t}$	0.0135	0.0169	0.0232	0.0273*	-0.0097	-0.0104
	(0.0174)	(0.0176)	(0.0138)	(0.0141)	(0.0197)	(0.0188)
War	-0.0303	-0.0383	-0.0066	-0.0127	-0.0237	-0.0255
	(0.0267)	(0.0294)	(0.0193)	(0.0189)	(0.0156)	(0.0194)
Fixed-effects						
State	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	298	298	298	298	298	298
R^2	0.43968	0.43599	0.25707	0.24028	0.51811	0.51697
Within R^2	0.14575	0.14014	0.06798	0.04693	0.10527	0.10315

Driscoll-Kraay (L=2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Note: Italicized variables have been asinh transformed.

Table 2.7: Exogenous Debt Results - Disaggregated Income

Dependent Variables:	TotalRate		StateRate		NonStateRate	
	OLS	IV	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$TotalMiles_{i,t-1}$	-0.2073***	-0.2622***	-0.0778***	-0.1384***	-0.1296***	-0.1239**
	(0.0490)	(0.0541)	(0.0284)	(0.0313)	(0.0429)	(0.0503)
$Population_{i,t-1}$	0.0360	-0.0002	0.0094	-0.0290	0.0266	0.0288
	(0.0738)	(0.0917)	(0.0362)	(0.0593)	(0.0678)	(0.0668)
$NonRailIncome_{i,t-1}$	0.0104	0.0241	-0.0352	-0.0502	0.0456*	0.0743*
	(0.0379)	(0.0662)	(0.0236)	(0.0452)	(0.0248)	(0.0425)
$RailIncome_{i,t-1}$	0.0008	0.0026	0.0004	0.0029***	0.0004	-0.0003
	(0.0016)	(0.0021)	(0.0009)	(0.0011)	(0.0014)	(0.0020)
$\Delta Debt_{i,t}$	0.0131	0.0163	0.0242*	0.0297*	-0.0111	-0.0134
	(0.0169)	(0.0172)	(0.0144)	(0.0152)	(0.0207)	(0.0188)
War	-0.0241	-0.0204	-0.0089	-0.0007	-0.0152	-0.0197
	(0.0239)	(0.0325)	(0.0223)	(0.0294)	(0.0181)	(0.0255)
Fixed-effects						
State	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	298	298	298	298	298	298
R^2	0.44037	0.43348	0.24873	0.22080	0.51666	0.51401
Within R^2	0.14681	0.13630	0.05752	0.02249	0.10258	0.09765

Driscoll-Kraay (L=2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Note: Italicized variables have been asinh transformed.

2.8 Conclusion

Regular government revenues did not seem to be an important factor in determining the growth rate of railroads in German states (except perhaps through their effect on states' ability to borrow, which requires more study). However, revenue shocks do have the effect of making contemporaneous construction more private. I hypothesize that this is because revenue shocks were not large enough to fund new state owned railroad projects, but did allow the states to increase subsidies enticing private investment. Further study should explore how much private capital a subsidy can attract compared to the amount of debt service an equally sized revenue stream could support.

Chapter 3

Railroads and Rural Displacement in Bavaria: 1861-1910

3.1 Introduction

The 19th century was a period of radical social and economic change across all of Europe, and Bavaria was no exception. Initially, Bavaria appeared to handle these changes particularly well. Bavaria emerged from the Napoleonic wars substantially enlarged, with new Swabian lands centered on Augsburg, Franconian territory centered on the Nürnberg-Fürth metropolitan area, and an exclave in the more densely populated Palatinate. Although the incorporation of new territories with different dialects and greater religious diversity posed a challenge to social cohesion, the Bavarian efforts to craft a modern state were so successful that by 1865 Bismarck would remark that it was the only German state that had actually succeeded in creating a genuine "national feeling" (Segal, 2019). Bavaria was an early adopter of new technologies as well: the first railway tracks in Germany were laid connecting Nuremberg and Fürth in 1835, and the first factories of the *Badische Anilin und Sodafabrik*, the leaders of Germany's chemical revolution, were built in Bavaria in the 1860s.

As the political and economic integration of Germany continued, Bavaria appeared poised to enter the 20th century as a major modernization success story and an important counterweight within the new German Empire, balancing south German interests against Prussian hegemony. It is surprising then, that a 1912 report would find that the population growth rate in Bavaria was lagging significantly behind the rest of Germany because of mass emigration and above average infant mortality rates (Königlich Statistischen Landesamt, 1912).[1] Although it always remained the second largest economy in Germany, Bavaria largely failed to industrialize on the scale of the other German states until after the second world war,[2] and there were other signs suggesting that she experienced significant social and economic upheaval in the latter half of the 19th century. The number of children born out of wedlock

[1]For modern studies covering the issues of infant mortality and emigration, see (Gehrmann (2012) and Segal (2019) respectively.

[2]Tipton (1976) documents relatively low levels of industrial employment, and Schremmer (1976) makes a convincing argument that Bavaria's rural land ownership structure prevented the introduction of factories.

was growing rapidly (Lee, 1977), and Catholic peasants agitated by Bismarck's *Kulturkampf* and simultaneously feeling threatened by economic disruption were flocking to new political organizations (Farr, 1986). This is not to say that Bavaria did not continue to innovate and grow in the second half of the 19th century. On the contrary, absolute levels of employment in both the industrial and services sectors nearly doubled between 1882 and 1907.

Because it clearly experienced both massive growth as well as acute growing pains, Bavaria has the potential to be a particularly illustrative case study for understanding how technological revolutions can create winners and losers in an industrializing economy. In this paper, I explore how one innovation in particular reshaped economic geography in Bavaria: the railway. There is little doubt that 19th century investment in railways had positive macroeconomic effects. Railways generated value through social savings and increased market access,[3] created agglomeration economies by accelerating urbanization[4], spurred the development of financial markets because of their large capital requirements,[5] and (particularly importantly for Germany) were leading industries.[6] However, many questions about the effects of railways still remain: Which of these channels were most important? Was public ownership a key variable?[7] Do aggregate benefits hide regional losers?[8] One paper cannot effectively answer all of these questions, but the unique features of the Bavarian context will offer new insights, particularly into the issues faced by those most vulnerable to displacement when railways connected Bavaria to a wider, more competitive, economy.

There is no question that railways were responsible for some degree of rural displacement.

[3]See the seminal work by Donaldson and Hornbeck (2016), which also includes a comprehensive review of earlier social savings literature.

[4]See Braun and Franke (2022); Hornung (2015) for studies linking railways to city growth in other German states. See Alvarez-Palau et al. (2025); Atack et al. (2010); Berger and Enflo (2017) for studies covering the UK, USA, and Sweden, respectively.

[5]For econometric analyses, see Atack et al. (2014). For more qualitative documentation, see Beyer (1978); Millward (2013, 2004); Mitchell (2000).

[6]See Fremdling (1985, 1977).

[7]See Bogart (2010, 2009), as well as Chapter 2 of this dissertation.

[8]The strand of literature documenting "agglomeration shadows" touches on one aspect of this issue. See Büchel and Kyburz (2018); Hodgson (2018).

In the aforementioned 1912 report on emigration, lack of access to railways was cited as a primary reason for depopulation in 25% of municipalities that experienced a significant population decline. However, this does not necessarily mean that access to railways was always desirable. Railways may have also been indirectly responsible for some of the other mentioned causes, such as shortages of agricultural wage laborers or imports of agricultural machinery. Falling transportation costs and greater exposure to foreign goods may also have made many small farms uneconomical. This issue of rural displacement was widespread but concentrated in the north and west of the kingdom. More than one in three Bavarian municipalities experienced population decline from 1861 to 1910; in Franconia, the rate was near 50%. The uneven distribution of population decline is visible in Figure 3.1. In this paper, I will go beyond a simple analysis of railway access on population growth and try to explore why the introduction of railways in Bavaria had such heterogeneous effects.

In the following section (Section 3.2), I provide a brief history of the Bavarian Economy in the 19th century. Then (Sections 3.3 and 3.4), I describe the empirical strategy and new population and transportation network data that was digitized for this study. Section 3.5 presents results, and Section 3.6 concludes.

3.2 A Brief History of the Bavarian Economy

For a comprehensive discussion of the Bavarian economy at the turn of the 19th century, the reader is directed to Schremmer (1970) and Spindler (2003). This section provides a brief overview of the most relevant points.

Prior to its territorial expansion following the Napoleonic wars, Bavaria was a compact principality centered around Munich. This core territory of *Altbayern* benefited from a wide mix of resource endowments thanks to its geography. The alps provided important mineral

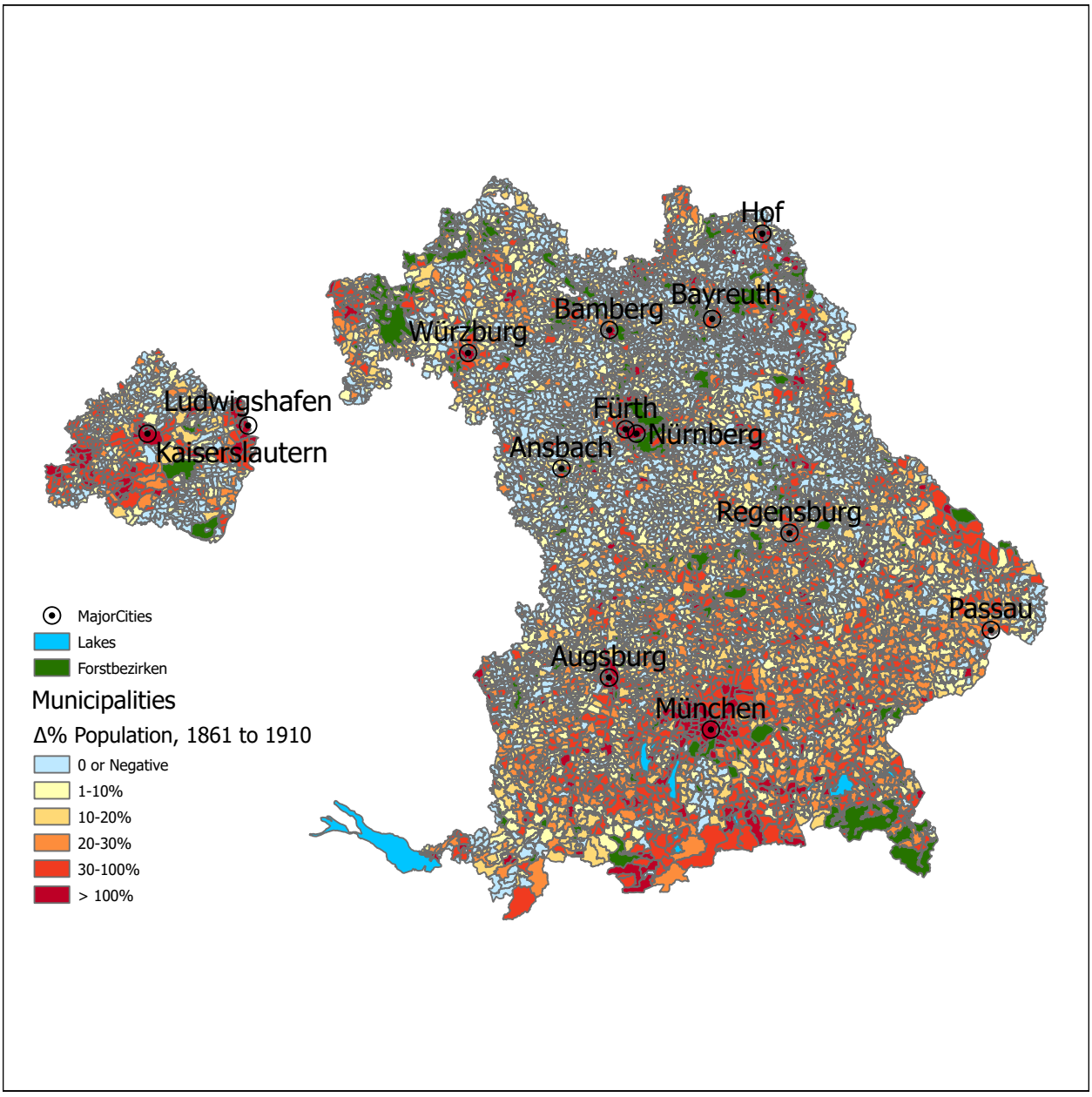


Figure 3.1: Population Change in Late 19th Century Bavaria

reserves—the iron and salt from Berchtesgaden being particularly important to Munich's early prosperity—as well as excellent conditions for dairy industry. The runoff from alpine lakes and rivers also provided an easy source of water-power that was not accessible further north, after those rivers fed into the Danube. Of course, the Danube itself also contributed to Bavaria's early economy, because of its importance for trade, and because of the fertile soil in its valley where hops thrived.[9] In regions that could not support cash crops, rye and oats were most the most common crops (Bauer, 1930).

The size and scope of this economy increased greatly during the early 19th century. First, because of territorial expansion. In 1803, the *Reichsdeputationshauptschluss* incorporated Passau and portions of Swabia into Bavaria. Swabian lands included Augsburg, the financial and commercial capital of central Europe, as well as the fertile region around Lindau on Lake Constance, which had the additional benefit of connecting Bavaria to Switzerland. Passau was important center for trade on the Danube. Then, in 1806, along with the dissolution of the Holy Roman Empire and the formation of the Confederation of the Rhine, Bavaria was expanded yet again with the territories of Franconia. This region had previously included many small but prosperous principalities such as the bishopric of Würzburg, the Margraviate of Brandenburg-Kulmbach, and the Imperial City of Nuremberg. Munich remained the largest city in Bavaria, but was no longer the only urban center. Along with these important cities, Franconia also had important mineral deposits in the *Frankenjura*, and large swaths of land best suited for growing wheat and potatoes.

The final territorial expansion of Bavaria occurred officially in 1816, when the Treaty of Munich officially established the Palatine territories west of the Rhine as part of the Kingdom of Bavaria. These territories, which were never contiguous with the rest of Bavaria, had at

[9] A difficult to verify claim that is often repeated online claims that 1/3 of the world's hops are grown here in the *Hallertau* region. Back of the envelope calculations suggest it is likely somewhat exaggerated, since in 2023 Germany produced roughly 34% of the world's hops (IHGC Economic Commission, 2023) and in 1925 Bavaria as a whole (that is, also including the major hop growing regions around Nuremberg) produced 81% percent of Germany's hops (Bauer, 1930).

various times been independent, in personal union with Bavaria, occupied by France and occupied by Baden, and in many ways remained culturally, politically, and economically distinct from the Bavarian *hauptstadt* (Ruppert, 2017).

The second set of major shocks to the Bavarian economy occurred in the 1830s, when the nearly simultaneous development of railways and formation of the *Zollverein* (German Customs Union) obliterated trade barriers within and between the German states. But to understand how falling trade costs affected Bavarian society and economics, first we must describe in a bit more detail the basic structure of Bavarian peasant agriculture. Unfortunately, we are forced to paint with a rather broad brush here, because a qualitative history describing local trends in agricultural production would be too large an undertaking and quantitative sources such as the land-use censuses have not yet been digitized. Schremmer (1976) documents some of the most important stylized facts: In Bavaria, agriculture was dominated by smallholders who needed to supplement their agricultural production with income from putting out, typically in the textile industry. Furthermore, the proportion of smallholdings was growing over time from 32% in 1691 to 46% in the 1750s and 48% in 1949.[10] This system also located proto-industrial production firmly in the countryside. In 1794, 75% of artisans lived in rural areas, suggesting an advanced "territorialization" of production following the weakening of guilds and medieval urban monopolies on manufacturing. Because of this decentralized socioeconomic system and a generous process of peasant emancipation that did not saddle free farmers with onerous debt, "a new landless class was not created". This conclusion of Schremmer's needs to be qualified. While the data suggests that there was no such new class that could efficiently supply labor to expanded Bavarian factories, it does not rule out the possibility that many smallholders left their land seeking opportunities in America or the Ruhr.

[10]Schremmer did not include the data for 1882 from Königlich Statistischen Bureau and Rasp (1887). At that time, 49.2% of Bavarian farms earned supplemental income. This percentage was lower in *Altbayern* (even falling below 40% in Niederbayern) and Swabia, slightly higher in Franconia, and nearly 60% in the Palatinate.

Even if the socioeconomic structure of Bavaria was kind to Bavarian peasants, they were still vulnerable to the effects of increased competition in a world of rapidly falling trade costs. Smallholders were in a particularly precarious position, since the products they produced were most vulnerable to replacement by cheap manufactures produced by factories abroad. On the other hand, as farmers they benefited from increasing demand for agricultural commodities, so long as world prices remained sufficiently high. The literature has not yet established which of these effects dominated, largely due to data constraints and a lack of reliable trade statistics for units smaller than the *Zollverein* or Reich.[11] This brings us to the subject of this paper, which will look at how the railway shock to trade costs affected economic activity in Bavaria at a local level, benefiting some regions at the expense of others.

To inform the hypotheses this paper seeks to test, i.e., that increasing market access may have had differing effects depending on the region's resource endowments and comparative advantages, I conclude this section with a brief discussion of the work of Dumke (1977), who finds statistics that are consistent with the qualitative history described above. Dumke finds a rich inter-regional trade in manufactures across South Germany (i.e., between Baden, Wüttemberg, and Bavaria) but that trade with North Germany (Prussia, Saxony, Hesse) is characterized by agricultural exports and manufactured inputs. Dumke contrasts the relatively well performing South German agrarian economy with the stagnant East Prussian one in two ways: First, by noting that the increased demand for southern agriculture supported the southern proto-industry that supported it. Second, by noting that East Prussian agriculture was dominated by large scale wheat farms. Although Dumke considers the income distribution a more important factor for limiting Prussian growth than the specific crop, we should be aware that the wheat growing regions of Bavaria had the smallest farms and thus were in more direct competition with these (and potentially other eastern European) large producers. Dumke concludes that the Zollverein provided modest benefits to south German

[11] Dedinger (2015) describes the poor state of German trade data. Recent work by Hungerland and Wolf (2022) suggests trying to build a dataset with statistics from any earlier than 1880 is hopeless.

incomes since they were able to purchase northern manufactures more cheaply following the reduction of tariffs but declines to speculate on whether the long run effects of increased northern industrialization led to a decline in southern proto-industry.

Taking these facts into consideration, we can begin to explore the possibility that market access had heterogeneous effects in Bavaria. As a whole, the Bavarian economy was growing, and that growth accelerated in the 19th century following the Zollverein and railway shocks. At the same time, rural depopulation was extremely uneven and much more common in the north and west of the state. While some of it can be explained by urbanization and the German industrial revolution, the data suggests that many Bavarians emigrated instead of relocating to their local metropolis. In the subsequent analysis, I use new data to explore how key socioeconomic variables related to agricultural production—such as soil type and farm size—interacted with market access to reshape Bavarian economic geography.

3.3 Empirical Strategy

3.3.1 The Estimating Equation

To estimate the relationship between market access and changing economic geography, I focus on the relationship between railways and municipality level population growth. It is necessary to conduct this analysis at the most granular level possible because the effect of railways is extremely localized, with a single kilometer of additional difference from the network often having significant economic effects. In Bavaria, this means using the *gemeinde* as the unit of analysis.[12] I will emphasize the role of railways when discussing shocks to market access, because the railway shock is most easily disentangled from other contemporaneous

[12]These are the smallest administrative divisions with a legal status, although sometimes they were constructed from multiple villages or suburbs with distinct identities. Their legal status was established in the 1818 constitution Bavarikon.

shocks. Furthermore, the literature on railways has an established toolkit for dealing with endogeneity issues. In this paper, I will use the widely accepted "incidental connection" logic[13] to generate an instrumental variable that uses the completion of the Bavarian trunk lines in 1860 to identify the effects of market access on population growth.

One caveat to this approach is that we are limited to estimating a cross-sectional growth equation[14] with 1860 as the base year. Thankfully, the population variation observed between 1861 and 1910 is exactly the variation we are trying to explain, and while confounding shocks such as the formation of the empire or changes to tariff policy following Bismarck's ministry could bias an OLS specification, since their effects on trade costs are difficult to measure, the IV specification will allow us to be more confident in our results. Population is the main outcome of interest because it is widely accepted as a proxy for economic activity throughout the long 19th century and captures the aggregate effects of both natural growth and migration. Unfortunately, the extant data for migration and agricultural output is not sufficiently granular to be included in a municipality level study. The baseline equation being estimated is:

$$Log(Pop_i^{1910}) - Log(Pop_i^{1860}) = \beta_0 + \beta_1 MA_i^{1860} + B\chi + \epsilon_i$$

Where the dependent variable is the log difference in the population of municipality i and the variable of interest MA_i^{1860} is market access in 1860. Market access is defined using the equation $MA_i^{1860} = \sum_d = \tau_{od}^{-\theta} Y_d$, where $\tau_{od}^{-\theta}$ is the iceberg trade cost between origin o and destination d.[15] In some specifications, market access is instrumented by the distance to the least-cost-paths that connect the major urban areas of Bavaria (the calculation of

[13] See Berger (2019); Berger and Enflo (2017); Bogart et al. (2022); Büchel and Kyburz (2018); Hornung (2015).

[14] See Duranton and Puga (2014) for a discussion of the advantages and shortcomings of this specification.

[15] To get an iceberg cost, the transportation cost in currency units is divided by the value of a representative shipment. Since the average value of railway shipments is not known, I use the 1869 value of a ton of wheat from Mayr (1870).

this instrument is discussed in more detail below). The equations also include a vector of controls related to geography and some characteristics of the local railway line. To study the heterogeneity of market access effects, we modify the equation as follows:

$$Log(Pop_i^{1910}) - Log(Pop_i^{1860}) = \beta_0 + \beta_1 MA_i^{1860} + A(\chi * MA_i^{1860}) + B\chi\epsilon_i$$

The term $A(\chi * MA_i^{1860})$ allows the slope of the market access effect to vary with X, a variable that potentially causes market access to have heterogeneous effects. X can be continuous or a vector of dummy variables. IV specifications include the instrument(s) $X * LCPDistance_i$.

3.3.2 Constructing the Instrument

It is often repeated in the histories of railway construction in Germany that the first lines were constructed primarily to reduce costs along well-established trade routes rather than to make any connections. This raises a significant endogeneity concern: access to railways is selected based on an entrenched economic geographic and historical prosperity. However, it is also true that lines were generally constructed based on the characteristics of their endpoints. As a consequence, the cities between these selected endpoints were only *incidentally* connected. Therefore, proximity to the least-cost-path (using elevation change as the cost) between cities early planners wanted to connect functions as an effective instrumental variable. It is relevant, since it strongly predicts proximity to actual railway lines, and it is excluded because the location of villages was established long before anyone had the notion of rail travel.

In the absence of documents discussing the planning process of the Bavarian railway network, I define the selected endpoints of the network as the cities which were ever one of the ten largest cities in Bavaria. Because railway planners also would be interested in con-

necting to neighboring German states, I also include the nearest city to the North with a population greater than 5000[16] that is located on a paved road connected to Bavaria. Important connections to Austria and Switzerland are located at Lindau, Salzburg, and Passau. After calculating the elevation-weighted least-cost-paths between each of these points, the distances of each municipality to those lines is calculated, generating the main instrumental variable. These paths, cities, and the distribution of the instrument are visible in Figure 3.2. The actual lines connecting these major cities were completed in 1860, which is when I consider the trunk network completed.

It is important to be clear about what this instrument captures. Proximity to the least-cost-path is correlated with the likelihood of being connected to the trunk network, i.e., the likelihood of having a high level of market access in 1860. It does not necessarily reflect a high change in market access, since railways constructed parallel to rivers had much smaller effects on market access. High market access in 1860 is also heavily correlated with market access going forward, since the closure of unproductive only began in the 1930s Heinze and Kill (1988). On the other hand, low market access in 1860 does not necessarily mean that market access remained low over the next 50 years. This only becomes a concern if market access changes from the feeder lines constructed later had a different effect on population growth than market access changes from trunk lines. This is unlikely, since in both cases we are considering the effects of a similar shock on similar rural communities. However, because the endogeneity problem cannot be resolved in the same way to estimate the effect of feeder lines, we cannot rule out the possibility.

[16]Population data from Buringh (2021)

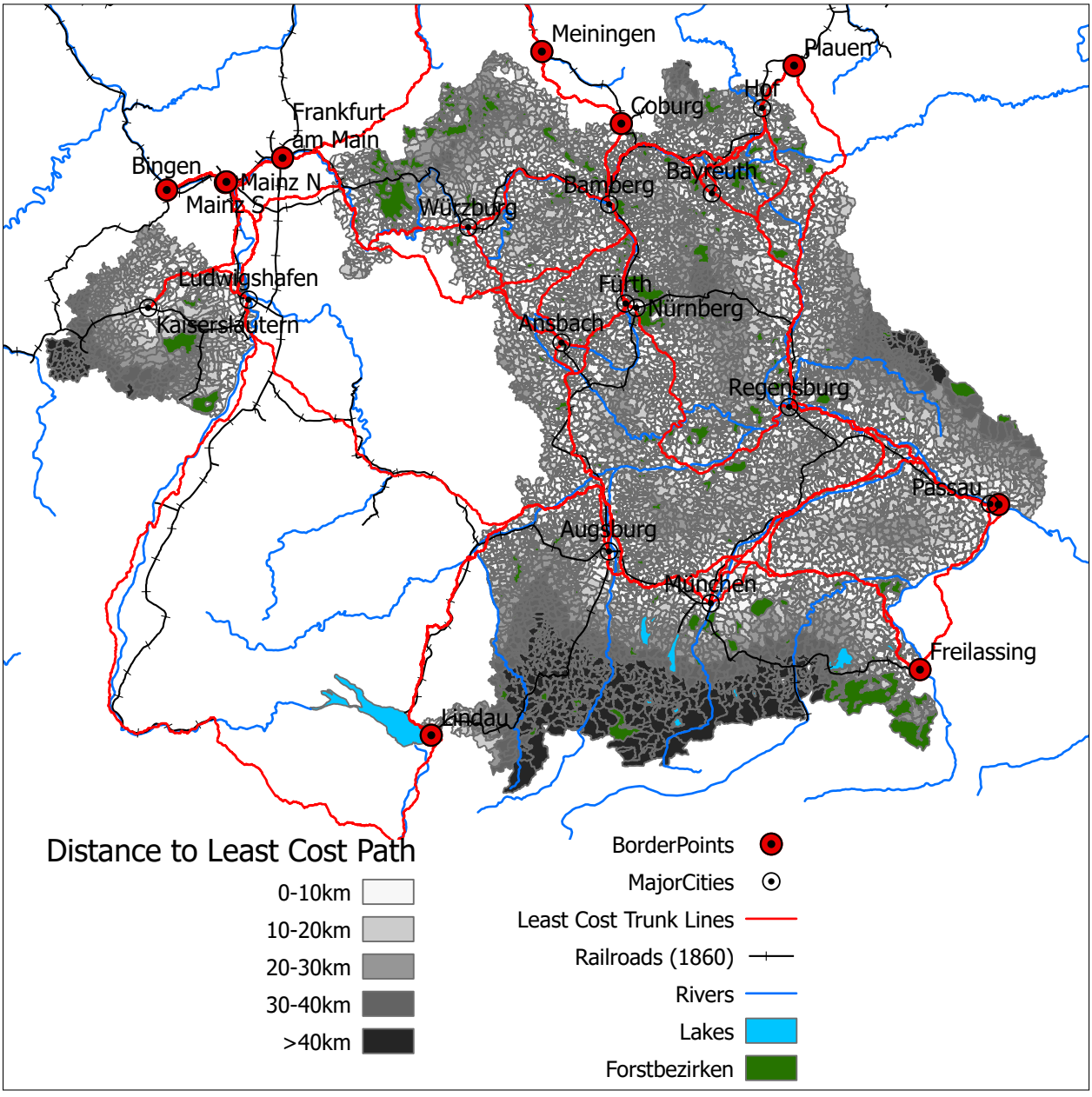


Figure 3.2: Least Cost Paths between Major and Border Cities

3.4 Data Sources and Visualizations

Estimating the equations described above requires the following data: municipality populations in 1861 and 1910, contemporary municipal borders/point locations and a transportation network with contemporary costs. Additionally, whatever controls we would like to include. In this section, I describe the various sources and digitization methodologies employed to create the novel dataset used for this paper's analysis. I also present relevant summary statistics and descriptive visualizations.

3.4.1 Municipal Population

The primary source used for population data is the Bayerischen Statistischen Landesamt (1953) or GVB. This publication reported *gemeinde* level population statistics for each of the 21 Bavarian censuses that took place between 1840 and 1952. Crucially, it includes notes that make it possible to track each municipality over time even if the name changed. It also notes the creation of new municipalities occurring from mergers of old municipalities, as well as the annexation of smaller municipalities into larger ones. This allows me to aggregate according to the universe of municipalities as they were organized in 1910.

Figure 3.3 shows the scale of population growth in Bavaria throughout the second half of the long 19th century. Despite above average rates of emigration and below average rates of natural population growth, the population of Bavaria still increased by 65% (almost 3 million individuals) between 1840 and 1910. Nearly all of this growth occurred in urban areas: The number of Bavarians living in the top 10 most populous cities nearly quintupled and the number living in the remaining cities with more than 5000 occupants more than quadrupled. Meanwhile, the share living in municipalities with between 1000 and 5000 inhabitants only grew by 70%, while the population living in villages of less than 1000 grew by less than one

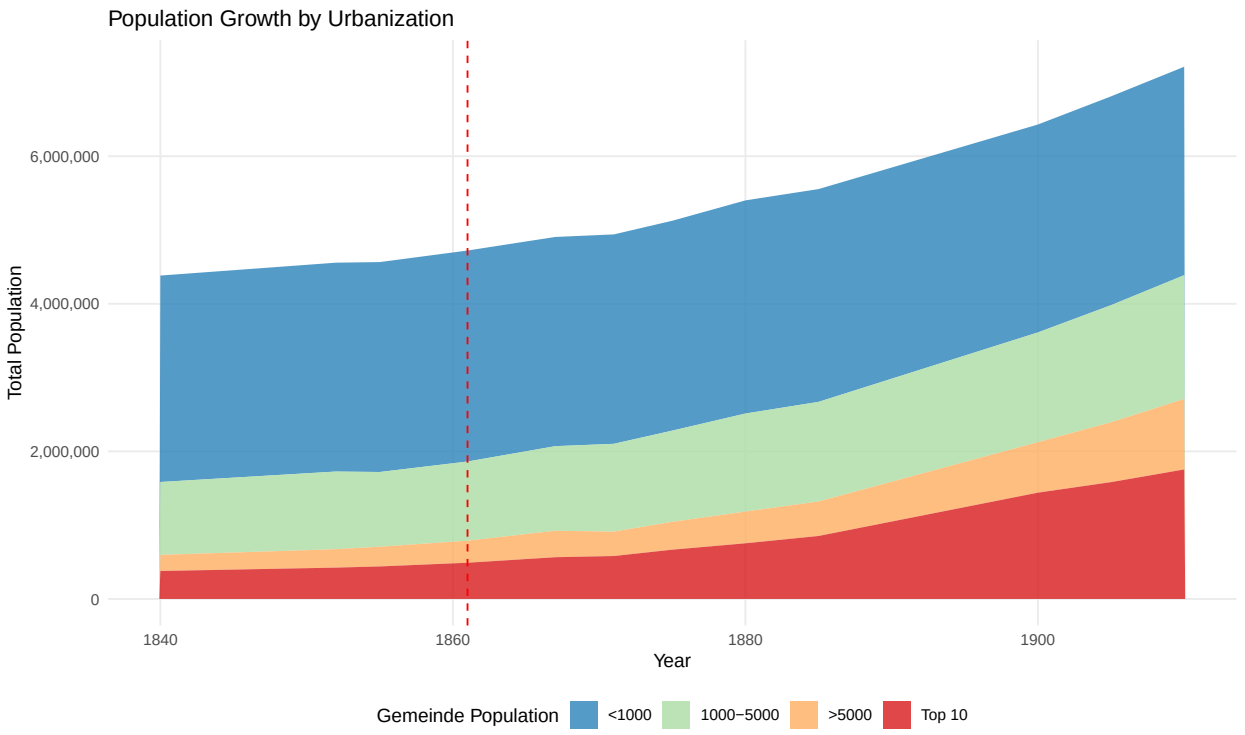


Figure 3.3: Population Growth by Municipality Size

percent. The natural rate of population growth in rural Bavaria may have been relatively low, but it remained well above zero, so this can only be explained by rural out-migration.

3.4.2 Geocoded Municipalities

Digitized population records are helpful, but of limited value until they are geocoded. Geocoding historical locations poses a variety of challenges, particularly in the German context, where village names are often duplicated and the geographical qualifiers that uniquely identify them are not always included in the census records. German orthography was also reformed in 1996, further complicating name-based matching. Even if matching is successful, most geocoding services only provide a point location, and the calculation of controls based on geography requires polygons. Aggregation to contemporary polygons also involves a significant loss of resolution, since by 2020 the number of municipalities fell by 70% from

8015 to 2337.

To address these issues I digitized a contemporary map: K.B. Brandversicherungs-Kammer (1892). The main benefit of this map is that it includes a point location for each municipality as well as their municipal borders. This gives us a more accurate point location than a centroid for measuring key distances like the distance of a municipality to a river or railway, while also allowing us to calculate statistics related to land quality of the entire municipality like the terrain ruggedness index. Using the notes included in the GVB, I aggregated the polygons according to their 1910 borders.

My code for matching the digitized records of the GVB to the new shapefile is extremely successful, with only nine municipalities failing to match after dropping municipalities that were not always Bavarian, such as those that were annexed by Prussia in 1866. These nine failures municipalities had a combined population less than 4000, representing less than .01% of the overall population. There is little concern that this failure introduces any significant bias.

3.4.3 The Multimodal Transport Network and Market Access

The last component needed to calculate market access is an estimate of trade costs between every pair of origins and destinations. Before discussing the estimation of those costs, we need to address some shortcomings of the market access approach that relate to the feasible set of destinations. The population data from the GVB only allows us to calculate the intra-Bavarian market access. Expanding the set of destinations to include locations outside of Bavaria is fraught with both methodological challenges and data constraints. Commonly used historical European population datasets such as Bairoch et al. (1988) or Buringh (2021) only include urban populations above a certain threshold, omitting large segments of the market. It may be reasonable to say that access to urban centers captures all the relevant

variation, but then we are faced with a lack of publicly available shapefiles for transport infrastructure across Europe, as well as difficulties finding sources with comprehensive data for trade costs between countries.

These data constraints regarding transportation limit us to looking at market access within Germany, but expanding beyond a single state also has challenges. Local population data across the entirety of Germany is only available after 1871 and uses the universe of municipalities as they were in 2019, rather than the contemporary set (Roesel, 2022). Calculating market access using pan-German urban centers as potential trading partners is feasible, but it is difficult to predict what kind of biases this introduces. This seems particularly inappropriate for Bavaria, where trade costs to Vienna or Switzerland were presumably more relevant than those to Hamburg. Rather than stretching the market access approach beyond its standard application, I only calculate the intra-Bavarian market access.[17] To estimate trade costs within Bavaria, I construct a multi-modal transportation network for Bavaria which includes country roads, paved roads, railways, and canals. The sources for shapefiles and price data are presented in Table 3.1. The spatial distribution of market access in 1860 is visible in Figure 3.4, with the distribution over time in figure 3.5. One can see clearly that rivers and railroads determined market access, and that while the construction of Trunk Lines greatly increased market access between 1839 and 1860 for all of Bavaria, the general structure of market access was not changed nearly as much compared to the creation of branch lines in the following years.

3.4.4 Geography and Agriculture

Unfortunately, there is no municipal level data on historical soil quality or agricultural productivity. The first official Bavarian publication to include data on agricultural land usage

[17]It is possible to proxy for increasing exposure to foreign markets by measuring the transportation costs to key border cities, but this metric is too closely correlated with Bavarian market access to be included in the same regression. It is also difficult to conceptualize a distinct instrumental variable that could be used.

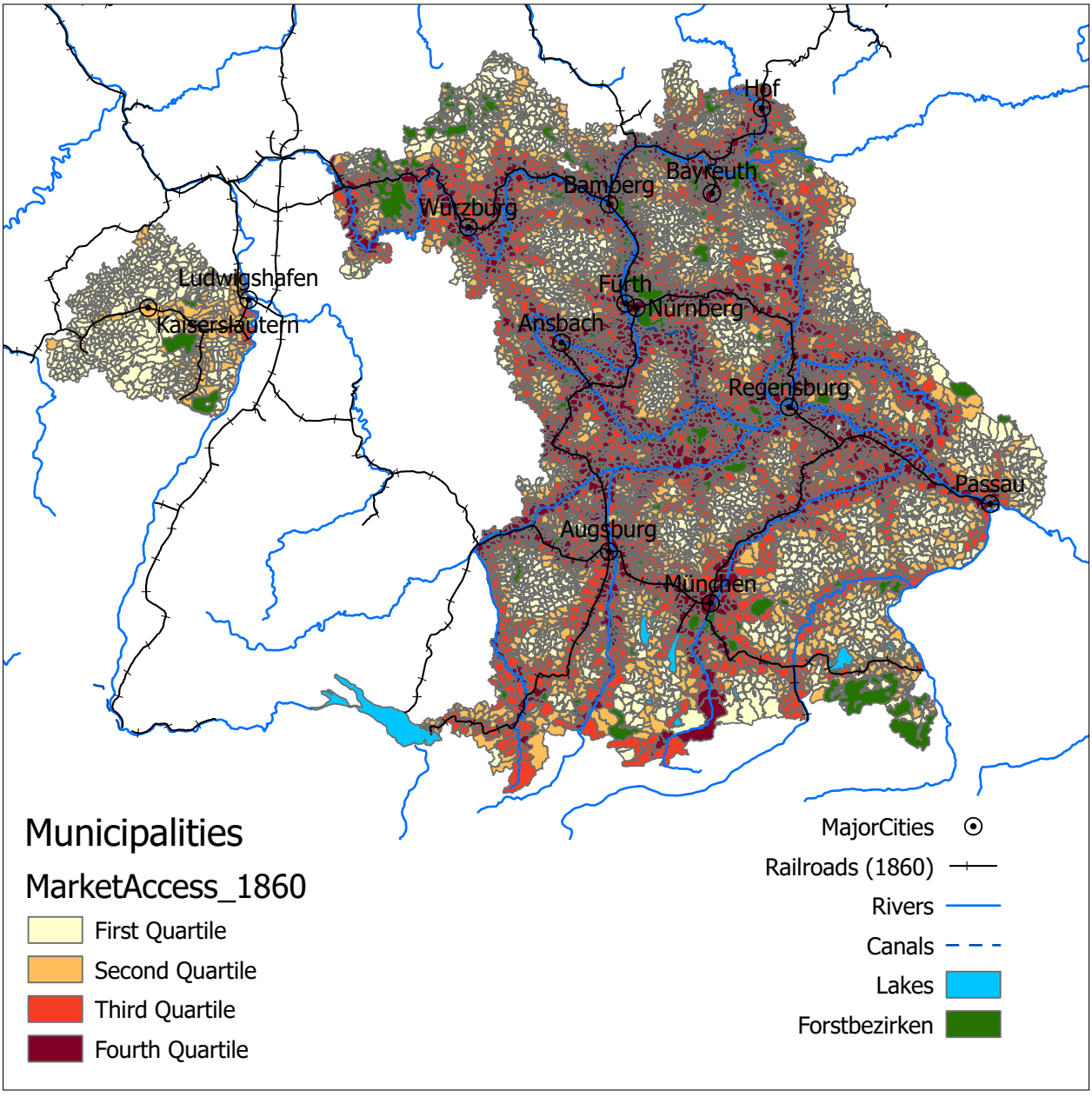


Figure 3.4: Bavarian Market Access in 1860

Connection	GIS Source	Cost (Pfennig/TKM)	Cost Source
Country Road	Author calculated elevation weighted least-cost paths between neighboring municipalities.	120	Huning and Wolf (2016)
Paved Road	Kunz and Zipf (2008)	30	Huning and Wolf (2016)
River (Downstream)	Kunz and Zipf (2008)	0.7	Huning and Wolf (2016)
River (Upstream)	Kunz and Zipf (2008)	1.8	Huning and Wolf (2016)
Canal	Kunz and Zipf (2008)	1.774	Voelker (1970)
Railway	Kunz and Zipf (2008)	7.9	Fremdling (1985)

Table 3.1: Transportation Network Costs and Sources

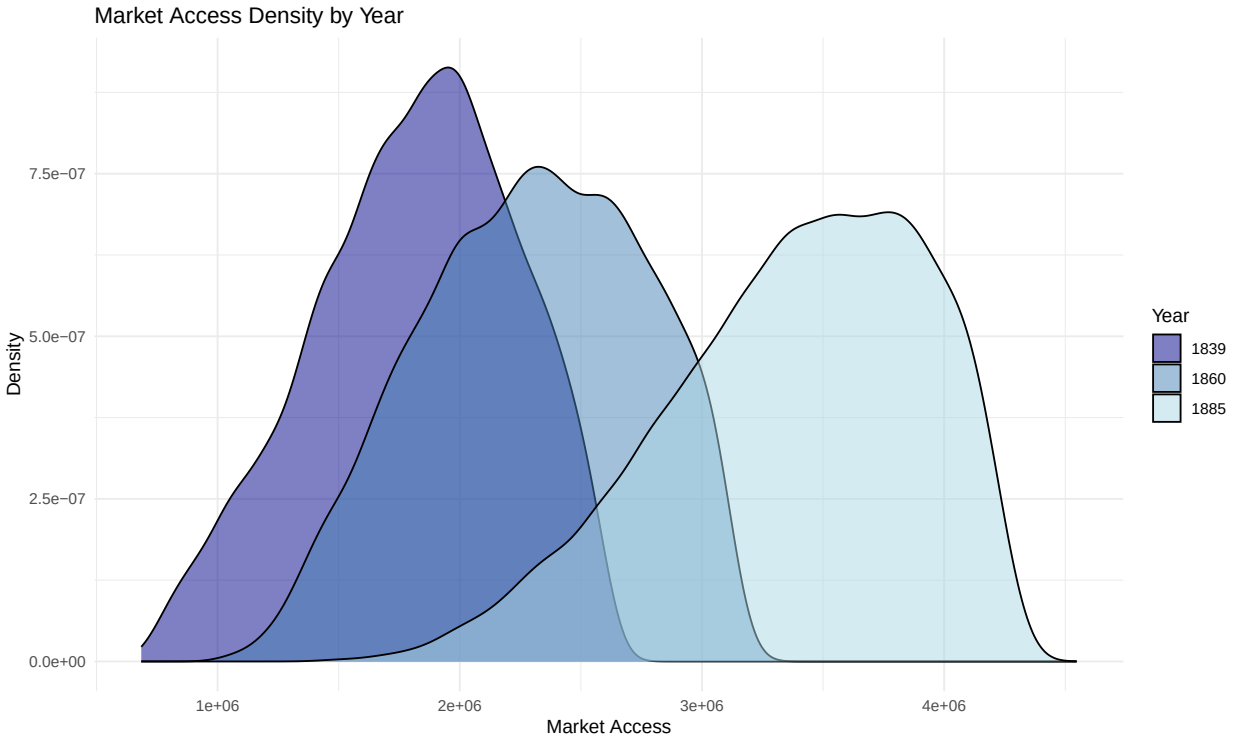


Figure 3.5: The Evolution of Bavarian Market Access during the 19th Century

was Königlich Statistischen Bureau and Rasp (1887). The report was mostly based on data from an 1882 land use survey but also refers to aggregated data from 1863 and 1878. The source of this earlier data is either lost to time or remains undiscovered in the archives. The 1882 data that is available at the *gemeinde* level tells us the area and share of the municipality's land that was engaged in productive agriculture, the number of landowners, the share of those landowners who live within the community, average plot size, the range of plot sizes, and the share of farms and agriculture land that were smallholdings. Data on the share of land dedicated to each crop, as well as data on actual output, is only available at higher levels of aggregation. Digitization of this data is an ongoing; preliminary results using aggregated are included as an exercise but, in addition to be less useful than disaggregated data, may suffer from significant endogeneity bias since the measurement occurred midway through the period of interest.

Without local data on the size of farms or the share of farms engaged in putting out, the best proxy is data related to the quality of the land. Among other potential relationships, Huning and Wahl (2021) argue that geographic constraints on can influence inheritance law by bounding the minimum efficient size of farms. To control for these relationships, I use elevation data from European Space Agency and Airbus (2022) to calculate the terrain ruggedness index, visible in Figure 3.6. Geographic controls also include the size of the municipality and the average elevation.

Measuring the quality of the soil itself can become complicated because there are so many dimensions to soil quality. The practice of measuring soil quality is also relatively recent, and it is possible that local soil quality has changed significantly in the last 150 years. For simplicity, I restrict my analysis to two dimensions. First, I code municipalities according to which "*Boden-Klima-Raum*" (BKR), or "soil-climate-area", they belong to, following the definitions of Roßberg et al. (2007). This groups regions according to their suitability for different types of agricultural, but does not directly give any ordinal information about

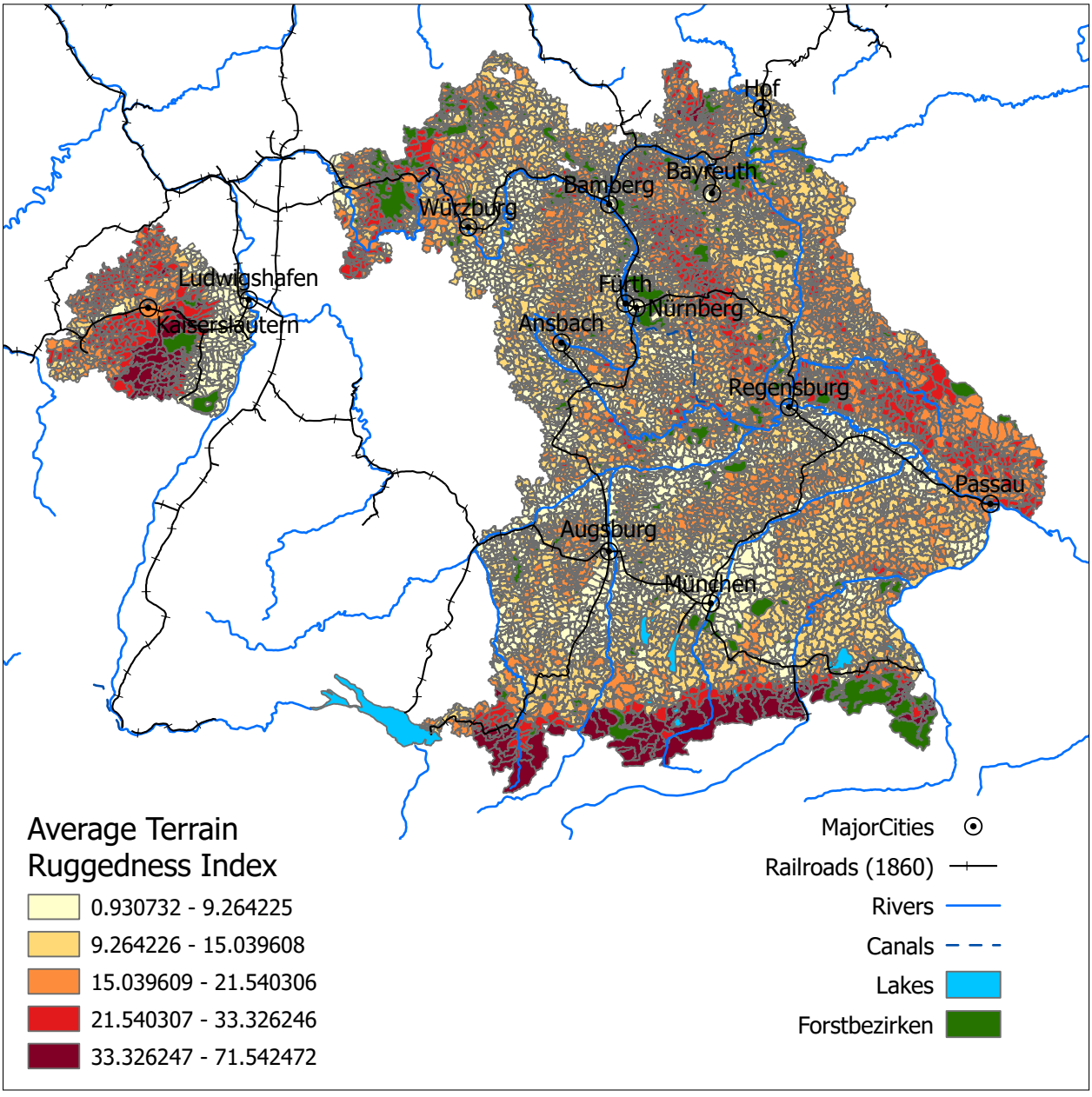


Figure 3.6: Average Terrain Ruggedness Index

quality. Graduated quality can either be weighted by suitability for a grouping of different crops or measure the suitability for a single crop. For simplicity, I use the FAO & IIASA (2025) measure of soil suitability for growing wheat, under conditions of rainfall watering and low management, as this best represents the baseline potential of the soil. The calculated average suitability by municipality is visible in figure 3.7. It is important to note that this is a measure of underlying soil quality, rather than actual decisions to plant wheat. In reality, the regions in Figure 3.7 with the highest soil suitability mostly grew rye, oats, and hops instead of wheat.

3.5 Results

The results are organized as follows: First, I take a baseline estimate of the effect of proximity to a rail line. This functions as both as simple sanity check and as point of comparison to similar studies. Then, I present the baseline model estimating the effects of market access on rural population growth. Finally, I explore whether the effects of market access are contingent on the agricultural or socioeconomic qualities of different regions. So that all specifications are consistent with the "incidental connections" logic, the 140 cities with a population greater than 2000 in 1840 are dropped from the analysis, as these were more likely to be deliberately chosen as stops when connecting the major cities.

3.5.1 Proximity to the Railway

To estimate the effect of proximity to a railway, I measured the distance between each municipality's point location and the nearest railway line. Unfortunately, data on the location of stations is unavailable, so this measure does include some noise. Then, I group the municipalities into 5 kilometer bins based on that distance, and include these bins as dummy

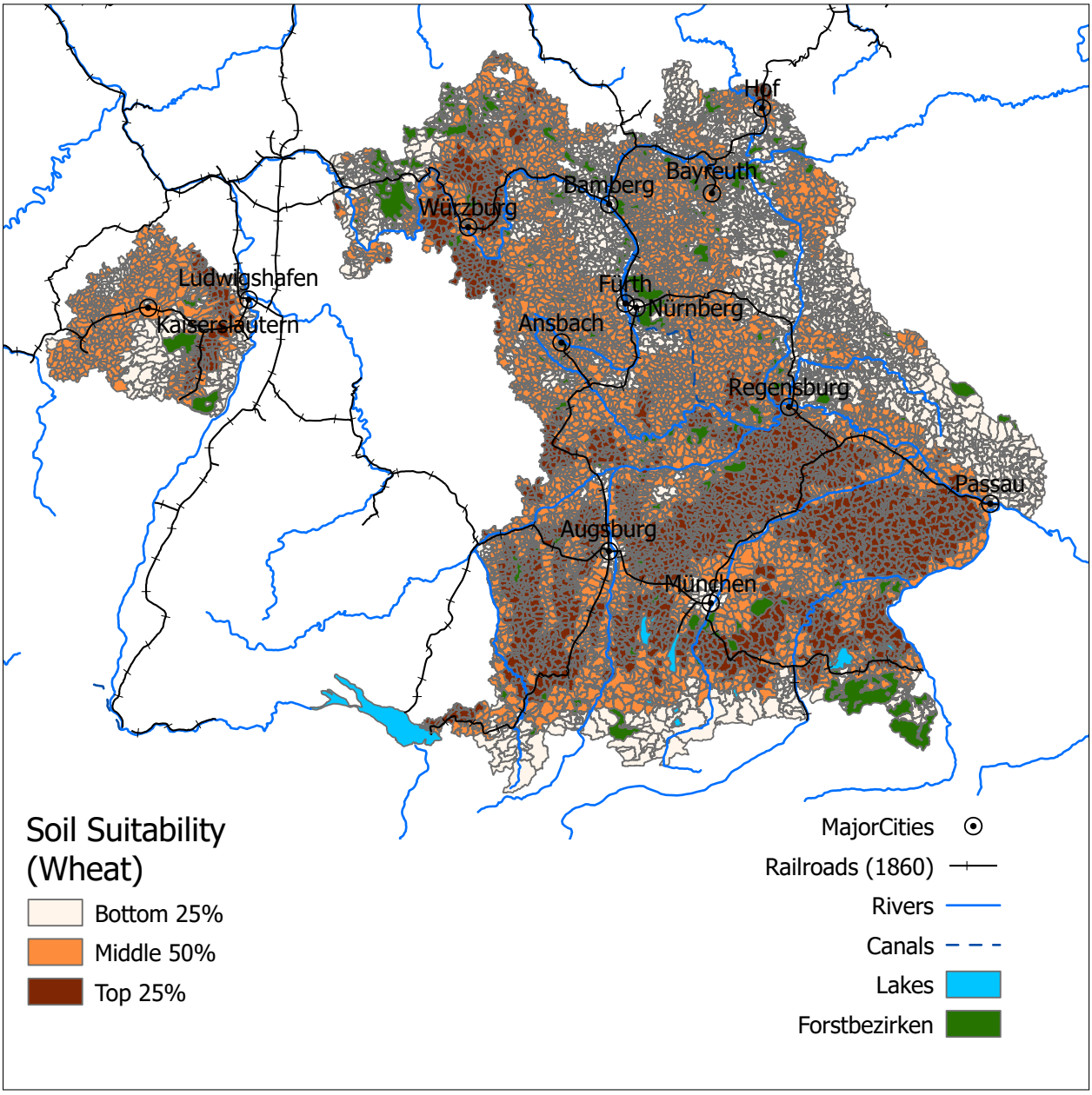


Figure 3.7: Soil Suitability for Growing Wheat

variables, with a distance of greater than 30 kilometers as the reference level. Table 3.2 shows the results of this regression. Column 1 does not include any covariates.

Column 2 includes dummy variables for a "Region". These regions are *Altbayern*, Franconia, Swabia, and the Palatinate. The dummy variable captures the unobservable cultural differences between these areas, as well as the differences in laws and institutions that persisted even after the territories were all united in the Kingdom, such as the differences in inheritance traditions (Freiherrn von Gagern, 1891). Column 3 introduces controls based on geography: average elevation, average ruggedness (measured using the Terrain Ruggedness Index approach or TRI), and distance to the nearest river. These variables are all essentially time-invariant.

In the next columns, I introduce controls that are not completely time-invariant. Since these were measured decades (or more) later, this may introduce measurement error. However, they are slow to adjust, and therefore even delayed measurements should function as reasonable proxies for earlier conditions. Column 4 introduces controls based on soil or climactic qualities: dummy variables for BKR, the wheat suitability index, and a dummy variable for the main crop grown in the region according to (Bauer, 1930). Finally, column 5 includes aggregated data at the bezirksamt (approximately equivalent to a county) level on the organization of agriculture: Average farm size and the share of farms with *nebenerwerb*, or supplemental income. The precise meaning of this term, and how it should be interpreted, is discussed in more detail below.

There are two main takeaways from the results in Table 2. First, the coefficients on the distance bins show that railways had positive local effects but may also have created agglomeration shadows. Municipalities that were within 5km of a railway line experienced about 10% more population growth than those more than 30km away in all specifications. In specifications 2 and 3 this is the only effect, and the negative coefficients for a 15-25km distance are insignificant. In columns 4 and 5 there is a negative and significant agglomer-

ation shadow occurring from 10-25 away from the rail line. Municipalities in this shadow grow 2-3% slower than communities further away.

The second main takeaway is that this was a period of significant population divergence, as shown from the coefficient on 1861 population. Even after omitting urban areas with a population greater than 2000, population growth rates were higher the larger the community, suggesting stagnation in small communities or migration to areas with a higher population concentration. This result is consistent across all models and specifications in the remainder of this paper: If the population of a village in 1860 was doubled, we would expect its growth rate over the next 50 years to have been 6% higher.

3.5.2 Baseline Results

Finding evidence for the existence of agglomeration shadows caused by railways is interesting, but we would like to take the analysis a step further and understand the mechanisms driving this result. One key mechanism could be market access: Railways, by lowering trade costs, greatly increased market access in Bavaria, particularly when they passed through regions with poor access to rivers. Furthermore, understanding the effects of market access will allow us to make more general conclusions about the effects of other shocks like the creation of the *Zollverein* or other policies reducing trade barriers. Rather than concentrating the analysis on the effects of railways, in this section railways are used as part of an "incidental connections" identification strategy to understand the effects of market access.

Table 3.3 reports the baseline OLS estimates of regressing late 19th century population growth on market access in 1860. The results are positive, significant, and relatively large in magnitude. Having 10% higher market access in 1860 is associated with having slightly more than 1% higher population growth over the next 50 years. Since the coefficients on controls are generally consistent across specifications, I will briefly discuss those results here.

Table 3.2: Binned Proximity to the Railway Regressions

Dependent Variable:	Pop_Logdif_1910_1861				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
RailDistance_bin00_05	0.059***	0.113***	0.108***	0.075***	0.075***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
RailDistance_bin05_10	-0.042***	0.011	0.013	-0.014	-0.014
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
RailDistance_bin10_15	-0.045***	0.006	0.010	-0.019*	-0.018
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
RailDistance_bin15_20	-0.055***	-0.007	-0.003	-0.032***	-0.031***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
RailDistance_bin20_25	-0.052***	-0.014	-0.011	-0.024**	-0.025**
	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
RailDistance_bin25_30	-0.019	0.008	0.012	0.010	0.008
	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)
Log Population_{1861}	0.068***	0.062***	0.062***	0.058***	0.059***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Region	No	Yes	Yes	Yes	Yes
Geography	No	No	Yes	Yes	Yes
Soil	No	No	No	Yes	Yes
Agricultural Organization	No	No	No	No	Yes
Fit statistics					
Observations	7,825	7,825	7,825	7,825	7,825
R^2	0.05745	0.10802	0.12236	0.20525	0.20902
Adjusted R^2	0.05661	0.10688	0.12090	0.20157	0.20516

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Relative to *Altbayern*, growth rates in Franconia and Swabia were significantly lower. These results are robust to the inclusion of additional covariates, suggesting that there was something beyond market access, geography, soil quality, or agricultural organization that was driving these results. More research perhaps warranted to understand what difference could be driving this result. The coefficients on the Palatinate dummy are less consistent, but also typically negative, especially in IV specifications. Elevation appears to suffer from similar omitted variable bias in the more sparse specifications. While consistently positive in column 3, it is either null or negative in columns 4 and 5. Ruggedness, however, is consistently negative. The soil variables are not enumerated because BKR is divided into too many categories to meaningfully report and because wheat suitability is an ordinal variable that is difficult to interpret. The Bauer agricultural regions can be summarized as follows: relative to the areas that grow rye, oats, and hops, the dummy variables for dairy are sometimes significantly positive, for potatoes are sometimes significantly negative, and for the mixed area which grows both rye and wheat are always significantly negative.

Because the coefficients on market access in Table 3.3 potentially suffer from endogeneity bias, I repeat these strategies using likelihood to be on a railway line as an instrument. This is measured by a municipality's distance to the least-cost-paths that would serve as trunk lines connecting all major cities in Bavaria to each other and to major border crossings. For small municipalities, proximity to this path is incidental; they were not selected to receive a railroad but were incidentally treated because it was cost-efficient. The results of this approach are reported in Table 3.4.

A surprising result emerges from this exercise: The coefficients on market access have become negative, although only significantly so in columns 3 and 5. There are three possibilities for this outcome: First, the instrument could be inappropriate. Since the first stage F statistics are all above 30, a weak-instrument problem is unlikely. A violation of the exclusion restriction cannot be ruled out without more information about the process of deciding line

Table 3.3: Baseline Regressions - Market Access and Growth

Dependent Variable:	Pop_Logdif_1910_1861				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
log(MarketAccess_1860)	0.087***	0.116***	0.087***	0.151***	0.150***
	(0.014)	(0.015)	(0.019)	(0.019)	(0.019)
log Population_{1861}	0.075***	0.068***	0.068***	0.062***	0.062***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
RegionFranconia		-0.133***	-0.104***	-0.034***	-0.026**
		(0.006)	(0.007)	(0.011)	(0.012)
RegionPfalz		-0.027**	0.049***	-0.103	-0.080
		(0.012)	(0.015)	(0.113)	(0.113)
RegionSwabia		-0.063***	-0.089***	-0.137***	-0.114***
		(0.009)	(0.009)	(0.010)	(0.011)
log(Avg_Elv)			0.103***	-0.006	-0.047**
			(0.013)	(0.017)	(0.019)
log(Avg_TRI)			-0.066***	-0.078***	-0.068***
			(0.006)	(0.006)	(0.007)
log(Dist_NearRiver)			-0.006*	0.003	0.004
			(0.003)	(0.003)	(0.003)
Avg_FarmSize					0.078***
					(0.015)
Nebenerwerb%					0.002***
					(0.000)
Soil	No	No	No	Yes	Yes
Fit statistics					
Observations	7,825	7,825	7,825	7,825	7,825
R^2	0.03608	0.08599	0.10143	0.19337	0.19696
Adjusted R^2	0.03584	0.08540	0.10051	0.19016	0.19356

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

locations, but should not introduce a large bias unless some of the selected cities were extreme outliers, since very few were likely candidates for economic reasons given the documented lack of factories outside major urban areas.

The second possibility is simply that the IV results have exposed an abnormally large endogeneity bias. However, I hypothesize that the third possibility is most likely: I hypothesize that the effect of market access is, in fact, heterogeneous. This is likely given the historical context: falling trade costs and increasing exposure to foreign markets can affect different producers differently, depending on their comparative advantages. In this case, the difference between the OLS and IV results reflects is not the difference between a biased and unbiased result, but because of more complicated interactions that need to be modeled separately. Given data constraints, the factors determining regional comparative advantage dynamics are hard to measure precisely. In the following section, I present results based on a handful of variables that are likely candidates for driving heterogeneity, or at least useful proxy measures.[18]

3.5.3 Heterogeneity Analysis

In this section, market access is interacted with a variable that may cause some regions to benefit from increased market access while others suffer. These hypotheses are motivated by a comparative advantage logic. For example, prior to increased trade with more industrial nations in North Germany, Bavarian proto-industrialists did not face much competition for their manufactures. As trade barriers fell, and factory production became increasingly cheaper, smallholders who relied on supplemental income from putting out were more likely to see negative consequences than farmers with large plots of land who only benefited from

[18] An additional strategy used trade costs to foreign markets directly, under the assumption that nations to the north could produce grain, potatoes, and manufactures more cheaply than Bavaria, harming these sectors, while nations to the South simply wanted to buy cereals. However, that analysis is omitted, as there is no way to separately instrument for the two distances in one model, making the model incomplete and likely biased. Modeling them separately finds results consistent with the other analyses in the paper.

Table 3.4: Baseline Results - IV Estimates

Dependent Variable:	Pop_Logdif_1910_1861				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
log(MarketAccess_1860)	-0.063*	0.066	-0.491**	-0.459	-0.637**
	(0.037)	(0.041)	(0.235)	(0.298)	(0.307)
log Population_{1861}	0.072***	0.068***	0.059***	0.060***	0.061***
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
RegionFranconia		-0.132***	-0.110***	-0.047***	-0.060***
		(0.007)	(0.008)	(0.013)	(0.019)
RegionPfalz		-0.042**	-0.075	-0.242*	-0.292*
		(0.016)	(0.053)	(0.138)	(0.150)
RegionSwabia		-0.062***	-0.080***	-0.133***	-0.115***
		(0.009)	(0.011)	(0.011)	(0.012)
log(Avg_Elv)			0.059***	-0.045*	-0.095***
			(0.022)	(0.027)	(0.028)
log(Avg_TRI)			-0.096***	-0.107***	-0.107***
			(0.014)	(0.016)	(0.017)
log(Dist_NearRiver)			-0.060***	-0.051*	-0.066**
			(0.022)	(0.027)	(0.027)
Avg_FarmSize					0.054***
					(0.019)
Nebenerwerb%					0.003***
					(0.001)
Soil	No	No	No	Yes	Yes
Fit statistics					
Observations	7,822	7,822	7,822	7,822	7,822
F-test (1st stage)	1,284.2	1,144.1	57.621	37.227	37.588

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

falling prices for manufactured goods. By similar logic, farmers who produced cereals that were produced more cheaply and in larger quantities in eastern Europe, like wheat and potatoes, were more likely to be negatively affected by falling trade barriers than those who produced specialized crops that did not grow well outside of Bavaria, like hops. The four variables I consider here are ruggedness, farm size, share of farms with supplemental income, and soil quality.

It is important to take the results in this specification with a grain of salt for two closely related reasons. The first is that all of the above variables were measured at some point after 1860. This may or may not be an issue, since all of these variables are relatively slow to change. Terrain ruggedness, although measured recently by satellites, is largely unchanged since the 19th century due to the slow nature of geological change. On the other hand the size of farms and whether they earned supplemental income was measured in 1882, and it is entirely possible that trade shocks had already restructured farm organization in Bavaria by that time. However, this bias would likely lead to underestimation, since pressures on small farms leading to consolidation would cause convergence towards the southern style of organization. Since significant differences between north and south remained, this process was still ongoing, if it was occurring at all, in 1882, and the ordinal ranking of these variables was likely unchanged. Soil suitability for growing wheat, which was measured in the 1980s, may have been influenced by climate change, over-farming, or other factors, but again we can relatively safely assume that the ordinal relationships remain the same.

The second issue regards whether these variables are exogenous, since one must be interacted with the least-cost-path instrument to act as an instrument for the interaction term between itself and market-access. Again, this relies on the assumption that these variables are slow to react, if they can react at all. This is almost certainly not a concern for the ruggedness and soil quality variables, but does require us to be cautious about the estimates based on the prevalent characteristics of farms in an area.

Finally, to be more concise, I only use the models corresponding to columns 3-5 in the tables above. Both OLS and IV results are presented for comparison.

Ruggedness

As a candidate for explaining heterogeneity, ruggedness has a distinct benefit: It functions as a sort of proxy for farm organization, while remaining plausibly exogenous. Ruggedness caps the viable size of farms directly, as well as indirectly, since ruggedness is correlated with poor soil. Although the data suggests only a weak correlation between ruggedness and farm size in Bavaria, there is a significant correlation between ruggedness and the share of farms that earn supplemental income. It is plausibly exogenous because ruggedness does not necessarily affect the likelihood of a municipality to receive a railway; all a railway needs is a single valley, and a visual inspection of the location of railway lines shows that many did pass through many high ruggedness areas as well as low.

The results of this heterogeneity analysis are presented in Table 3.5. Here, the coefficient on Market Access is positive, although only significant in the OLS specifications. The interaction with ruggedness, however, is significantly negative. This suggests that market access had positive effects on flatter areas with larger farms, better soil, and farmers who could farm enough to subsist without putting out. On the other hand, areas where the output capacity of farms was more limited were affected more negatively. This is consistent with either farmers whose small farms could not compete with larger scale agriculture or those who could not scale up their putting out to compete with factories being pushed out of the market, leading to higher out-migration from those areas.

It is important to note that the coefficient on ruggedness is significantly positive throughout. This is somewhat surprising, considering it is associated with poor soil and putting out, and may be indicative of an issue with this specification. It is particularly concerning since the

coefficient is so large that the positive effect of increasing ruggedness will dominate even when market access and the negative interaction is high.

Table 3.5: Heterogeneity Analysis - Ruggedness

Dependent Variable:	Pop_Logdif_1910_1861					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
log(MarketAccess_1860) x log(Avg_TRI)	-0.187***	-0.092***	-0.113***	-0.320***	-0.206***	-0.208***
	(0.025)	(0.025)	(0.026)	(0.065)	(0.072)	(0.073)
log(MarketAccess_1860)	0.582***	0.394***	0.449***	0.277	0.278	0.166
	(0.068)	(0.070)	(0.071)	(0.208)	(0.265)	(0.268)
log(Avg_TRI)	2.681***	1.268***	1.589***	4.593***	2.924***	2.970***
	(0.362)	(0.375)	(0.377)	(0.953)	(1.055)	(1.077)
log Population_{1861}	0.068***	0.062***	0.062***	0.059***	0.061***	0.062***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Region	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Yes	Yes	Yes	Yes	Yes	Yes
Soil	No	Yes	Yes	No	Yes	Yes
Agricultural Organization	No	No	Yes	No	No	Yes
Fit statistics						
Observations	7,825	7,825	7,825	7,822	7,822	7,822
F-test, $\log(\text{MarketAccess}_{1860})$				135.89	70.601	70.510
F-test, $\log(\text{MarketAccess}_{1860})xlog(Avg\_TRI)$				185.78	129.34	126.44

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Farm Size and *Nebenerwerb*

First, it is important to reiterate that unlike the rest of the data in this paper, the variables for farm organization are only available at a level of aggregation above the municipality. If data on farm size and the share of farms with a *nebenerwerb* (lit. side income) were available at a granular level prior to 1861, these would be ideal variables to use. They are direct measures of agricultural organization, and how different types of farms would be able to compete in a globalizing world. Furthermore, they would also be effective as Bartik instruments, which would open up more modeling choices beyond the cross-sectional growth regressions used here. Hopefully, the municipality level measurements for these variables can be recovered in the future.

Table 3.6, when interacts market access with average farm size, again suggests that the effects of market access are positive, not significant after controlling for endogeneity, and negative when interacted with a key covariate. However, this result is somewhat puzzling, as generally we would expect larger farms to benefit more from increased trade.

Table 3.6: Heterogeneity Analysis: Farm Size

Dependent Variable:	Pop_Logdif_1910_1861					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
log(MarketAccess_1860) x log(Avg_FarmSize)	-0.132***	-0.226***	-0.219***	-0.277**	0.301**	0.373***
	(0.037)	(0.038)	(0.038)	(0.125)	(0.131)	(0.135)
log(MarketAccess_1860)	0.274***	0.469***	0.455***	-0.013	-0.583**	-0.742***
	(0.054)	(0.056)	(0.056)	(0.235)	(0.256)	(0.271)
Avg_FarmSize	2.001***	3.369***	3.279***	4.090**	-4.344**	-5.398***
	(0.540)	(0.556)	(0.556)	(1.826)	(1.914)	(1.971)
log Population_{1861}	0.065***	0.059***	0.060***	0.056***	0.064***	0.066***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Region	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Yes	Yes	Yes	Yes	Yes	Yes
Soil	No	Yes	Yes	No	Yes	Yes
Agricultural Organization	No	No	Yes	No	No	Yes
Fit statistics						
Observations	7,825	7,825	7,825	7,822	7,822	7,822
F-test $\log(\text{MarketAccess}_{1860})$				130.75	69.072	65.968
F-test $\log(\text{MarketAccess}_{1860})xlog(Avg\_FarmSize)$				219.25	122.42	123.99

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

It is important to add a caveat to the *nebenerwerb* variable before discussing it. Though Schremmer seems to equate it with smallholding, it is not necessarily equivalent. Not every farm that received a side income was dependent on that side income to remain viable. It is likely the case that some farms simply engaged in putting out as a supplemental stream. This is especially likely since many of the largest farms also had a *nebenerwerb*. Generally speaking, it was much more common for smaller farms, but this suggests that we should not treat this variable as equivalent to the share of farms in a region that are smallholds. That being said, it is still likely highly correlated with the share of smallholds, and I interpret it accordingly.

The results in Table 3.7 are somewhat consistent with expectations but are also sufficiently puzzling and inconsistent across specifications that it is difficult to find them credible. Columns 5 and 6, which are in theory the preferred specifications, find that market access has positive but insignificant effects that are mitigated by a negative effect on regions where farms were more likely to have secondary income streams. However, this is the first specification which finds a negative coefficient on market access in the OLS results, and a highly significant one at that. The fact that almost all coefficients change signs between the OLS and IV results is also suspect, and suggests a more complicated relationship than can accurately be modeled. It is possible that not all forms of supplemental income were made equal, but the data cannot capture every degree of heterogeneity.

Table 3.7: Heterogeneity Analysis: *Nebenerwerb*

Dependent Variable:	Pop_Logdif_1910_1861					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
log(MarketAccess_1860) x Nebenerwerb%	0.006***	0.009***	0.010***	-0.007	-0.009*	-0.010**
	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)
log(MarketAccess_1860)	-0.205**	-0.273***	-0.325***	-0.211	0.201	0.210
	(0.088)	(0.089)	(0.089)	(0.222)	(0.231)	(0.232)
Nebenerwerb%	-0.086***	-0.126***	-0.143***	0.099	0.136*	0.154**
	(0.026)	(0.026)	(0.026)	(0.078)	(0.076)	(0.074)
log Population$_{1861}$	0.068***	0.061***	0.061***	0.061***	0.063***	0.063***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Region	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Yes	Yes	Yes	Yes	Yes	Yes
Soil	No	Yes	Yes	No	Yes	Yes
Agricultural Organization	No	No	Yes	No	No	Yes
Fit statistics						
Observations	7,825	7,825	7,825	7,822	7,822	7,822
F-test, log(MarketAccess$_{1860}$)				133.74	65.992	68.573
F-test, log(MarketAccess$_{1860}$)$xNebenerwerb\%$				95.487	53.768	59.562

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Soil Quality

Finally, I report in the Table 3.8 the results modeling heterogeneity that is dependent on soil quality. Because of the somewhat low resolution of the data, the actual measurements mapped to each municipality are difficult to interpret. Instead, I use ordinal bins corresponding to the quintiles, with the lowest 20% of values as the reference category. Using this method, we again find IV results that are directly in contradiction with the OLS results. The OLS results find a substantially positive effect of market access, although one that decreases as soil quality improves. On the other hand, the IV results are consistent with the preferred interpretation of the other specifications: Market Access had a negative effect in regions with low soil quality, since they could not compete. As soil quality increased, so did the benefits of market access. However, these results are largely insignificant.

Because of the number of categories is quite large, and because the number of observations within each category is quite unbalanced, heterogeneity based on BKR is not practical and not reported. Heterogeneity based on Bauer's classifications is also omitted, because it would require omitting the Palatinate, and also due to concerns with the resolution of the data and its possible endogeneity.

Table 3.8: Heterogeneity Analysis: Soil Quality

Dependent Variable:	Pop_Logdif_1910_1861					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
log(MarketAccess_1860) x SoilQualityQ2	-0.028	0.037	0.048	-0.212	0.003	-0.053
	(0.039)	(0.038)	(0.038)	(0.136)	(0.138)	(0.146)
log(MarketAccess_1860) x SoilQualityQ3	-0.040	-0.018	-0.008	-0.209*	0.066	0.048
	(0.044)	(0.043)	(0.043)	(0.112)	(0.119)	(0.121)
log(MarketAccess_1860) x SoilQualityQ4	-0.090**	-0.140***	-0.117***	-0.096	0.055	0.083
	(0.043)	(0.042)	(0.043)	(0.122)	(0.125)	(0.127)
log(MarketAccess_1860) x SoilQualityQ5	-0.119***	-0.116***	-0.096**	-0.040	0.107	0.110
	(0.045)	(0.045)	(0.045)	(0.125)	(0.133)	(0.135)
log(MarketAccess_1860)	0.143***	0.202***	0.189***	-0.247	-0.267	-0.382*
	(0.031)	(0.030)	(0.030)	(0.156)	(0.209)	(0.216)
log Population_{1861}	0.065***	0.061***	0.062***	0.059***	0.061***	0.062***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Region	Yes	Yes	Yes	Yes	Yes	Yes
Geography	Yes	Yes	Yes	Yes	Yes	Yes
Soil	No	Yes	Yes	No	Yes	Yes
Agricultural Organization	No	No	Yes	No	No	Yes
Fit statistics						
Observations	7,825	7,825	7,825	7,822	7,822	7,822
F-test, log(MarketAccess_1860)				71.484	40.503	41.184
F-test, log(MarketAccess_1860) x SoilQualityQ2				141.39	145.30	142.21
F-test, log(MarketAccess_1860) x SoilQualityQ3				339.72	300.04	300.41
F-test, log(MarketAccess_1860) x SoilQualityQ4				403.52	366.57	365.10
F-test, log(MarketAccess_1860) x SoilQualityQ5				353.40	315.93	322.33

IID standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

3.6 Conclusion

In four of five models, OLS results suggested market access in 1860 had a positive effect on municipal population growth in the following 50 years. However, these results suffer from significant endogeneity bias: market access was largely determined by investments in transportation infrastructure, and the location of those investments will be determined by economic activity. IV results using an incidental connections logic suggests this bias was quite large, sufficient enough to flip the sign of the coefficient on market access in all specifications.

I hypothesized that that this is because some regions were better suited to perform well when exposed to trade, either because they produced more specialized agricultural products that had little competition when trade barriers fell, organized farmland in ways that were more suited to production for export, or were not reliant on supplemental income from a putting out system that could no longer compete in an age of industrialization.

The results of the heterogeneity analysis are consistent with this hypothesis. IV specifications find that market access had negative effects in regions that were more rugged, had smaller farms that were more likely to have a supplemental income, and in regions with poorer soil quality that were more likely to produce wheat and potatoes.

Bibliography

Natural Earth. URL https://www.naturalearthdata.com/about/terms-of-use/.

Scott F Abramson. The Economic Origins of the Territorial State. *International Organization*, 71(1):97–130, 2017. ISSN 0020-8183, 1531-5088. doi: 10.1017/S0020818316000308. URL https://www.cambridge.org/core/product/identifier/S0020818316000308/type/journal_article.

Daron Acemoglu. Oligarchic Versus Democratic Societies. *Journal of the European Economic Association*, 6(1):1–44, March 2008. ISSN 1542-4766, 1542-4774. doi: 10.1162/JEEA.2008.6.1.1. URL https://academic.oup.com/jeea/article-lookup/doi/10.1162/JEEA.2008.6.1.1.

Daron Acemoglu, Suresh Naidu, Pascual Restrepo, and James A Robinson. Democracy Does Cause Growth. *journal of political economy*, page 54, 2019.

Eduard J Alvarez-Palau, Dan Bogart, Max Satchell, and Leigh Shaw-Taylor. Transport and urban growth in the First Industrial Revolution. *The Economic Journal*, 135(668):1191–1228, May 2025. ISSN 0013-0133. doi: 10.1093/ej/ueae111. URL https://doi.org/10.1093/ej/ueae111.

Suphan Andic and Jindřich Veverka. The Growth of Government Expenditure in Germany since the Unification. *FinanzArchiv / Public Finance Analysis*, 23(2):169–278, 1963. ISSN 0015-2218. URL https://www.jstor.org/stable/40909774. Publisher: Mohr Siebeck GmbH & Co. KG.

Manuel Arellano and Stephen Bond. Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations. *The Review of Economic Studies*, 58(2):277–297, 1991. ISSN 0034-6527. doi: 10.2307/2297968. URL https://www.jstor.org/stable/2297968. Publisher: [Oxford University Press, Review of Economic Studies, Ltd.].

Jeremy Atack, Fred Bateman, Michael Haines, and Robert A. Margo. Did Railroads Induce or Follow Economic Growth?: Urbanization and Population Growth in the American Midwest, 1850–1860. *Social Science History*, 34(2):171–197, 2010. ISSN 0145-5532, 1527-8034. doi: 10.1017/S0145553200011202. URL https://www.cambridge.org/core/product/identifier/S0145553200011202/type/journal_article.

Jeremy Atack, Matthew Jaremski, and Peter L. Rousseau. American Banking and the Transportation Revolution before the Civil War. *The Journal of Economic History*, 74(04):

943–986, December 2014. ISSN 0022-0507, 1471-6372. doi: 10.1017/S0022050714000825. URL http://www.journals.cambridge.org/abstract_S0022050714000825.

Paul Bairoch, Jean Batou, and Pierre Chevre. *Population des villes européennes de 800 à 1850: banque de données et analyse sommaire des résultats (la)*. Librairie Droz, 1988. URL https://github.com/JakeRuss/bairoch-1988. original-date: 2015-11-10T20:34:37Z.

Hubert A. Bauer. Economic Adjustments in Bavaria. *Economic Geography*, 6(3):257, July 1930. ISSN 00130095. doi: 10.2307/140386. URL https://www.jstor.org/stable/140386?origin=crossref.

Bavarikon. Decree on the Future Constitution and Administration of Local Authorities 1818. URL https://www.bavarikon.de/.

Bayerischen Statistischen Landesamt. *Historisches Gemeindeverzeichnis, die Einwohnerzahlen der Gemeinden Bayerns in der Zeit von 1840 bis 1952*. Number 192 in Beiträge zur Statistik Bayerns. Bayerischen Statistischen Landesamt, München, 1953.

Sascha O. Becker, Steven Pfaff, and Jared Rubin. Causes and consequences of the Protestant Reformation. *Explorations in Economic History*, 62:1–25, October 2016. ISSN 00144983. doi: 10.1016/j.eeh.2016.07.007. URL https://linkinghub.elsevier.com/retrieve/pii/S0014498316301085.

Thor Berger. Railroads and Rural Industrialization: evidence from a Historical Policy Experiment. *Explorations in Economic History*, 74:101277, October 2019. ISSN 0014-4983. doi: 10.1016/j.eeh.2019.06.002. URL https://www.sciencedirect.com/science/article/pii/S0014498318302080.

Thor Berger and Kerstin Enflo. Locomotives of local growth: The short- and long-term impact of railroads in Sweden. *Journal of Urban Economics*, 98:124–138, March 2017. ISSN 0094-1190. doi: 10.1016/j.jue.2015.09.001. URL https://www.sciencedirect.com/science/article/pii/S0094119015000595.

Timothy Besley and Torsten Persson. *Pillars of prosperity: the political economics of development clusters*. The Yrjö Jahnsson lectures. Princeton University Press, Princeton [N.J.], 2011. ISBN 978-0-691-15268-4 978-0-691-15815-0. OCLC: ocn719714687.

Timothy Besley, Ethan Ilzetzki, and Torsten Persson. Weak States and Steady States: The Dynamics of Fiscal Capacity. *American Economic Journal: Macroeconomics*, 5(4):205–235, 2013. ISSN 1945-7707. URL https://www.jstor.org/stable/43189566. Publisher: American Economic Association.

Peter Beyer. *Leipzig und die Anfänge des deutschen Eisenbahnbaus*. Böhlau, Weimar, 1978.

Vincent Bignon, Rui Esteves, and Alfonso Herranz-Loncán. Big push or big grab? Railways, government activism, and export growth in Latin America, 1865-1913: Railways and Governments in Latin America. *The Economic History Review*, 68(4):1277–1305, November 2015. ISSN 00130117. doi: 10.1111/ehr.12094. URL https://onlinelibrary.wiley.com/doi/10.1111/ehr.12094.

Max Boelcke. *Die entwicklung der finanzen im grossherzogtum Sachsen-Weimar von 1851 biz zur gegenwart*. Gustav Fischer, Jena, 1906.

Dan Bogart. Nationalizations and the Development of Transport Systems: Cross-Country Evidence from Railroad Networks, 1860–1912. *The Journal of Economic History*, 69(1):202–237, March 2009. ISSN 0022-0507, 1471-6372. doi: 10.1017/S0022050709000382. URL https://www.cambridge.org/core/product/identifier/S0022050709000382/type/journal_article.

Dan Bogart. A global perspective on railway inefficiency and the rise of state ownership, 1880–1912. *Explorations in Economic History*, 47(2):158–178, April 2010. ISSN 00144983. doi: 10.1016/j.eeh.2009.09.001. URL https://linkinghub.elsevier.com/retrieve/pii/S0014498309000485.

Dan Bogart and Latika Chaudhary. Railways in Colonial India: An Economic Achievement? *SSRN Electronic Journal*, 2012. ISSN 1556-5068. doi: 10.2139/ssrn.2073256. URL http://www.ssrn.com/abstract=2073256.

Dan Bogart, Xuesheng You, Eduard J. Alvarez-Palau, Max Satchell, and Leigh Shaw-Taylor. Railways, divergence, and structural change in 19th century England and Wales. *Journal of Urban Economics*, 128:103390, March 2022. ISSN 00941190. doi: 10.1016/j.jue.2021.103390. URL https://linkinghub.elsevier.com/retrieve/pii/S0094119021000723.

Edyta P Bogucka, Davide Cantoni, and Matthias Weigand. Princes and Townspeople: A Collection of Historical Statistics on German Territories and Cities. 1: City Locations and Border Maps, 2019. URL https://doi.org/10.7910/DVN/ZGSJED.

Thomas A. Brady. *Turning Swiss: cities and empire, 1450-1550*. Cambridge studies in early modern history. Cambridge University Press, Cambridge [Cambridgeshire] ; New York, 1985. ISBN 978-0-521-30525-9.

Sebastian Till Braun and Richard Franke. Railways, Growth, and Industrialization in a Developing German Economy, 1829–1910. *The Journal of Economic History*, 82(4):1183–1221, December 2022. ISSN 0022-0507, 1471-6372. doi: 10.1017/S0022050722000377. URL https://www.cambridge.org/core/journals/journal-of-economic-history/article/railways-growth-and-industrialization-in-a-developing-german-economy-18291910/0A37B4DAC9EEDBF8FF82836A706CDE2E.

Deutsche Bundesbank. Purchasing power comparisons of historical amounts of money. URL https://www.bundesbank.de/en/statistics/economic-activity-and-prices/producer-and-consumer-prices/purchasing-power-comparisons-of-historical-amounts-of-money-795290.

Eltjo Buringh. The Population of European Cities from 700 to 2000. September 2021. doi: 10.1163/24523666-06010003. URL https://brill.com/view/journals/rdj/6/1/article-p1_3.xml. Publisher: Brill.

Konstantin Büchel and Stephan Kyburz. Fast track to growth? Railway access, population growth and local displacement in 19th century Switzerland. *Journal of Economic Geography*, December 2018. ISSN 1468-2702, 1468-2710. doi: 10.1093/jeg/lby046. URL https://academic.oup.com/joeg/advance-article/doi/10.1093/jeg/lby046/5250421.

Peter Bühner. *Die Freien und Reichsstädte des Heiligen Römischen Reiches: kleines Repertorium*. Number Band 38 in Schriftenreihe der Friedrich-Christian-Lesser-Stiftung. Michael Imhof Verlag, Friedrich-Christian-Lesser-Stiftung, Petersberg : [Nordhausen], 2019. ISBN 978-3-7319-0664-3 978-3-930558-34-6. OCLC: on1083590973.

Davide Cantoni. The Economic Effects of the Protestant Reformation: Testing the Weber Hypothesis in the German Lands. *Journal of the European Economic Association*, 13 (4):561–598, August 2015. ISSN 1542-4766, 1542-4774. doi: 10.1111/jeea.12117. URL https://academic.oup.com/jeea/article/13/4/561/2319778.

Davide Cantoni. Princes and Townspeople: A Collection of Historical Statistics on German Territories and Cities. 5: Construction Activity, 2020. URL https://doi.org/10.7910/DVN/JIAC0S.

Davide Cantoni, Jeremiah Dittmar, and Noam Yuchtman. Religious Competition and Reallocation: the Political Economy of Secularization in the Protestant Reformation*. *The Quarterly Journal of Economics*, 133(4):2037–2096, November 2018. ISSN 0033-5533, 1531-4650. doi: 10.1093/qje/qjy011. URL https://academic.oup.com/qje/article/133/4/2037/5033707.

Davide Cantoni, Cathrin Mohr, and Matthias Weigand. Princes and Townspeople: A Collection of Historical Statistics on German Territories and Cities. 2L Territorial Histories, 2019. URL https://doi.org/10.7910/DVN/GZPPVE.

Davide Cantoni, Cathrin Mohr, and Matthias Weigand. The Rise of Fiscal Capacity: Administration and State Consolidation in the Holy Roman Empire. *Econometrica*, 92(5):1439–1472, 2024. ISSN 1468-0262. doi: 10.3982/ECTA20612. URL https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA20612. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA20612.

Micheal Clodfelter. *Warfare and armed conflicts: a statistical encyclopedia of casualty and other figures, 1492-2015*. McFarland & Company, Inc., Publishers, Jefferson, North Carolina, fourth edition edition, 2017. ISBN 978-0-7864-7470-7.

J. Bradford De Long and Andrei Shleifer. Princes and Merchants: European City Growth Before the Industrial Revolution. *The Journal of Law and Economics*, 36(2), 1993.

Béatrice Dedinger. Trade statistics of the Zollverein, 1834-1871. *Revue de l'OFCE*, 140(4): 67, 2015. ISSN 1265-9576, 1777-5647. doi: 10.3917/reof.140.0067. URL http://www.cairn.info/revue-de-l-ofce-2015-4-page-67.htm.

Mark Dincecco. *Political Transformations and Public Finances: Europe, 1650–1913*. Cambridge University Press, Cambridge, 2011. ISBN 978-1-139-01334-5. doi: 10.1017/CBO9781139013345. URL http://ebooks.cambridge.org/ref/id/CBO9781139013345.

Mark Dincecco and Gabriel Katz. State Capacity and Long-Run Economic Performance. *The Economic Journal*, 126(590):189–218, February 2016. ISSN 0013-0133, 1468-0297. doi: 10.1111/ecoj.12161. URL https://academic.oup.com/ej/article/126/590/189/5077805.

Mark Dincecco and Mauricio Prado. Warfare, fiscal capacity, and performance. *Journal of Economic Growth*, 17(3):171–203, September 2012. ISSN 1381-4338, 1573-7020. doi: 10.1007/s10887-012-9079-4. URL http://link.springer.com/10.1007/s10887-012-9079-4.

Jeremiah E Dittmar and Ralf R Meisenzahl. Public Goods Institutions, Human Capital, and Growth: Evidence from German History. *The Review of Economic Studies*, February 2019. ISSN 0034-6527, 1467-937X. doi: 10.1093/restud/rdz002. URL https://academic.oup.com/restud/advance-article/doi/10.1093/restud/rdz002/5306344.

Dave Donaldson and Richard Hornbeck. Railroads and American Economic Growth: A "Market Access" Approach*. *The Quarterly Journal of Economics*, 131(2):799–858, May 2016. ISSN 0033-5533, 1531-4650. doi: 10.1093/qje/qjw002. URL https://academic.oup.com/qje/article/131/2/799/2606976.

Horst-Werner Dumjahn. *Handbuch der deutschen Eisenbahnstrecken: Eröffnungsdaten 1835 - 1935, Streckenlängen, Konzessionen, Eigentumsverhältnisse = Die deutschen Eisenbahnen in ihrer Entwicklung 1835 - 1935*. Number 29 in Dokumente zur Eisenbahngeschichte. Dumjahn, Mainz, vollst. unveränd. nachdr. berlin, 1935 edition, 1984. ISBN 978-3-921426-29-6.

Rolf H. Dumke. *The Political Economy of German Economic Unification: Tariffs, Trade and Politics of the Zollverein Era*. PhD thesis, University of Wisconsin-Madison, 1976.

Rolf Horst Dumke. Intra-German Trade in 1837 and Regional Economic Development. *VSWG: Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 64(4):468–496, 1977. ISSN 0340-8728. URL https://www.jstor.org/stable/20733074. Publisher: Franz Steiner Verlag.

Gilles Duranton and Diego Puga. The Growth of Cities. In *Handbook of Economic Growth*, volume 2, pages 781–853. Elsevier, 2014. ISBN 978-0-444-53546-7. doi: 10.1016/B978-0-444-53540-5.00005-7. URL https://linkinghub.elsevier.com/retrieve/pii/B9780444535405000057.

European Space Agency and Airbus. Copernicus DEM, 2022. URL https://dataspace.copernicus.eu/explore-data/data-collections/copernicus-contributing-missions/collections-description/COP-DEM. Institution: European Space Agency.

FAO & IIASA. Global Agro-ecological Zoning version 5 (GAEZ v5) Model Documentation, 2025. URL https://github.com/un-fao/gaezv5/wiki.

Ian Farr. Peasant Protest in the Empire - The Bavarian Example. In Robert G. Moeller, editor, *Peasants and lords in modern Germany: recent studies in agricultural history*. Allen & Unwin, Boston, 1986. ISBN 978-0-04-943037-2.

Hans Erich Feine. *Die Besetzung der Reichsbistümer vom Westfälischen Frieden bis zur Säkularisation 1648-1803*. P. Schippers., Amsterdam, 1921.

J. S. Foreman-Peck. Natural Monopoly and Railway Policy in the Nineteenth Century. *Oxford Economic Papers*, 39(4):699–718, 1987. ISSN 0030-7653. URL https://www.jstor.org/stable/2663102. Publisher: Oxford University Press.

Alfons Freiherrn von Gagern. *Die Succession in Bauerngüter und landwirtschaftliche Erbgüter nach den in Bayern geltenden Rechten*. Aug. Vollrath's k. b. Hofbuchdruckerei, Erlangen, 1891.

Rainer Fremdling. Railroads and German Economic Growth: A Leading Sector Analysis with a Comparison to the United States and Great Britain. *The Journal of Economic History*, 37(3):583–604, September 1977. ISSN 0022-0507, 1471-6372. doi: 10.1017/S0022050700095395. URL https://www.cambridge.org/core/product/identifier/S0022050700095395/type/journal_article.

Rainer Fremdling. Freight Rates and State Budget: the Role of the National Prussian Railways 1880-1913. *The Journal of European economic history*, 9, 1980.

Rainer Fremdling. Germany. In Patrick O'Brien, editor, *Railways and the Economic Development of Western Europe, 1830–1914*, pages 121–147. Palgrave Macmillan UK, London, 1983. ISBN 978-1-349-06324-6. doi: 10.1007/978-1-349-06324-6_4. URL https://doi.org/10.1007/978-1-349-06324-6_4.

Rainer Fremdling. *Eisenbahnen und deutsches Wirtschaftswachstum 1840 - 1879: ein Beitrag zur Entwicklungstheorie und zur Theorie der Infrastruktur*. Number 2 in Untersuchungen zur Wirtschafts-, Sozial- und Technikgeschichte. Ges. für Westfälische Wirtschaftsgeschichte, Dortmund, 2., erw. aufl edition, 1985. ISBN 978-3-925227-25-7.

Rainer Fremdling and Günter Knieps. Competition, regulation and nationalization: The Prussian railway system in the nineteenth century. *Scandinavian Economic History Review*, 41(2):129–154, May 1993. ISSN 0358-5522, 1750-2837. doi: 10.1080/03585522.1993.10415864. URL http://www.tandfonline.com/doi/abs/10.1080/03585522.1993.10415864.

Rainer Fremdling and Andreas Kunz. Germany's statistics of railways 1835 to 1989., 2011.

Rainer Fremdling, Ruth Federspiel, Andreas Kunz, Dietlind Hüchtker, and Angelus H. Johansen, editors. *Statistik der Eisenbahnen in Deutschland 1835-1989*. Number Bd. 17 in Quellen und Forschungen zur historischen Statistik von Deutschland. Scripta Mercaturae, St. Katharinen, 1995. ISBN 978-3-89590-004-4.

Bruno S. Frey and Reiner Eichenberger. *The new democratic federalism for Europe: functional, overlapping, and competing jurisdictions*. Studies in fiscal federalism and state-local finance. E. Elgar, Cheltenham, UK ; Northampton, MA, USA, 1999. ISBN 978-1-84064-004-5.

Rolf Gehrmann. German census-taking before 1871. Working paper WP-2009-023, Max Planck Institute for Demographic Research, Rostock, Germany, 2009. URL https://EconPapers.repec.org/RePEc:dem:wpaper:wp-2009-023.

Rolf Gehrmann. Infant Mortality in Germany in the 19th Century. *Comparative Population Studies*, 36(4), November 2012. ISSN 1869-8999, 1869-8980. doi: 10.12765/CPoS-2011-22. URL https://www.comparativepopulationstudies.de/index.php/CPoS/article/view/95.

Andrew Goodman-Bacon. Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277, December 2021. ISSN 03044076. doi: 10.1016/j.jeconom.2021.03.014. URL https://linkinghub.elsevier.com/retrieve/pii/S0304407621001445.

Wolf-Dieter Grün. Der Bau der Ruhr-Sieg-Strecke vor 150 Jahren, Teil 1. In *An Bigge, Lenne und Fretter*, number 13 in Heimatkundliche Beiträge aus der Gemeinde Finnentrop. 2001.

C. Hefeker. The agony of central power: Fiscal federalism in the German Reich. *European Review of Economic History*, 5(1):119–142, April 2001. ISSN 1361-4916, 1474-0044. doi: 10.1017/S1361491601000053. URL https://academic.oup.com/ereh/article-lookup/doi/10.1017/S1361491601000053.

G. Wolfgang Heinze and Heinrich H. Kill. The Development of the German Railroad System. In Renate Mayntz and Thomas P. Hughes, editors, *The Development of Large Technical Systems*, pages 105–134. Routledge, 1 edition, 1988. ISBN 978-0-429-30999-1. doi: 10.4324/9780429309991-4. URL https://www.taylorfrancis.com/books/9781000243994/chapters/10.4324/9780429309991-4.

Charles Hodgson. The effect of transport infrastructure on the location of economic activity: Railroads and post offices in the American West. *Journal of Urban Economics*, 104:59–76, March 2018. ISSN 0094-1190. doi: 10.1016/j.jue.2018.01.005. URL https://www.sciencedirect.com/science/article/pii/S0094119018300068.

DONALD STONE HOFFMAN. *Railways and Railway Politics in South Germany: Defensive Particularism at the State Level, 1835-1870*. Ph.D., University of Delaware, United States – Delaware, 1969. URL https://www.proquest.com/docview/302358650/citation/A08649C1AD1F4754PQ/1. ISBN: 9798657814439.

Erik Hornung. Railroads and Growth in Prussia. *Journal of the European Economic Association*, 13(4):699–736, August 2015. ISSN 15424766. doi: 10.1111/jeea.12123. URL https://academic.oup.com/jeea/article-lookup/doi/10.1111/jeea.12123.

Wolf-Fabian Hungerland and Nikolaus Wolf. The panopticon of Germany's foreign trade, 1880–1913: New facts on the first globalization. *European Review of Economic History*, 26(4):479–507, November 2022. ISSN 1361-4916, 1474-0044. doi: 10.1093/ereh/heac001. URL https://academic.oup.com/ereh/article/26/4/479/6529230.

Thilo R. Huning and Fabian Wahl. The origins of agricultural inheritance traditions. *Journal of Comparative Economics*, 49(3):660–674, September 2021. ISSN 0147-5967.

doi: 10.1016/j.jce.2021.01.004. URL https://www.sciencedirect.com/science/article/pii/S0147596721000044.

Thilo R. Huning and Nikolaus Wolf. How England Unified Germany: Geography and the Rise of Prussia After 1815. 2016. URL https://www.econstor.eu/bitstream/10419/145725/1/VfS_2016_pid_6734.pdf.

IHGC Economic Commission. *Summary Reports*. International Hop Growers' Convention, Nuremberg, 2023.

Marcus Jacob. Long-Term Persistence: The Free and Imperial City Experience in Germany. *SSRN Electronic Journal*, 2010. ISSN 1556-5068. doi: 10.2139/ssrn.1616973. URL http://www.ssrn.com/abstract=1616973.

Noel D. Johnson and Mark Koyama. States and economic growth: Capacity and constraints. *Explorations in Economic History*, 64:1–20, April 2017. ISSN 00144983. doi: 10.1016/j.eeh.2016.11.002. URL https://linkinghub.elsevier.com/retrieve/pii/S0014498316301966.

K.B. Brandversicherungs-Kammer. Königreich Bayern nach Gemeinde-Bezirken, 1892. URL https://www.digitale-sammlungen.de/en/details/bsb00152502.

John M. Kleeberg. The Privatisation of the Brunswick State Railways in 1869–70. *The Journal of Transport History*, 11(1):12–28, March 1990. ISSN 0022-5266. doi: 10.1177/002252669001100103. URL https://doi.org/10.1177/002252669001100103. Publisher: SAGE Publications Ltd.

Andreas Kunz and Alexander Zipf. HGIS Germany: Historical Information System of the German States, 1820 to 1914, 2008.

Königlich Statistischen Bureau and Carl Rasp. *Die Landwirtschaftliche Bodenbenutzing in Bayern nach der Erhebung des Jahres 1883 und die landwirthschaftlichen Betriebe in Bayern*. Number LI. in Beiträge der Statistik des Königreichs Bayern. Commissionsverlag Lindauer'sche Buchhandlung, München, 1887.

Königlich Statistischen Landesamt. *Bayern und seine Gemeinden unter dem Einfluß der Wanderungen während der letzten 50 Jahre*. Number 69 in Beiträge zur Statistik des Königreichs Bayern. J. Lindauersche Buchhandlung (Schöpping), München, 1912.

Ernst Kühn. *Die historische Entwicklung des deutschen u. deutsch-österreichischen Eisenbahn-Netzes vom Jahre 1838 - 1881*. Königlich Preussischen Statistichen Bureau, Berlin, 1883.

Bayerischer Landtag. *Verhandlungen der Kammer der Abgeordneten des zweiten bayerischen Landtages im Jahre 1849*, volume 5 of *Stenographische Berichte*. Bayerischer Landtag, Augsburg, 1850.

W. R. Lee. Bastardy and the Socioeconomic Structure of South Germany. *The Journal of Interdisciplinary History*, 7(3):403–425, 1977. ISSN 0022-1953. doi: 10.2307/202573. URL https://www.jstor.org/stable/202573. Publisher: [The MIT Press, Massachusetts Institute of Technology and the editors of The Journal of Interdisciplinary History].

Henry E Lowood, T Fringsmyr, and R Rider. The calculating forester: quantification, cameral science, and the emergence of scientific forestry management in germany, 1990.

Ernst Löbe. *Der Staatshaushalt des Königreichs Sachsen in seinen verfassungsrechtlichen Beziehungen und finanziellen Leistungen*. Verlag von Veit & Comp., Leipzig, 1889.

Hans Mauersberg. *Finanzstrukturen deutscher Bundesstaaten zwischen 1820 und 1944*. Scripta Mercaturae, St. Katharinen, 1988. ISBN 978-3-922661-42-9.

Georg Mayr. *Zeitschrift des Koeniglich Bayerischen Statistischen Bureau*, volume 2. E. A. Fleischmann's Buchhandlung, München, 1870.

Charlotte Methuen. The German Catholic Dioceses on the Eve of the Reformation. In *Oxford Research Encyclopedia of Religion*. Oxford University Press, March 2017. ISBN 978-0-19-934037-8. doi: 10.1093/acrefore/9780199340378.013.275. URL http://religion.oxfordre.com/view/10.1093/acrefore/9780199340378.001.0001/acrefore-9780199340378-e-275.

Robert Millward. European governments and the infrastructure industries, c.1840-1914. *European Review of Economic History*, 8(1):3–28, April 2004. ISSN 1361-4916, 1474-0044. doi: 10.1017/S1361491604001030. URL https://academic.oup.com/ereh/article-lookup/doi/10.1017/S1361491604001030.

Robert Millward. *The state and business in the major powers: an economic history, 1815-1939*. Routledge, London, 2013. ISBN 978-0-415-62790-0.

Chris Minns, Clare H Crowston, Raoul De Kerf, Bert De Munck, Marcel J Hoogenboom, Christopher M Kissane, Maarten Prak, and Patrick H Wallis. The extent of citizenship in pre-industrial England, Germany, and the Low Countries. *European Review of Economic History*, 24(3):601–625, August 2020. ISSN 1361-4916, 1474-0044. doi: 10.1093/ereh/hez005. URL https://academic.oup.com/ereh/article/24/3/601/5480915.

Allan Mitchell. *The Great Train Race: Railways and the Franco-German rivalry, 1815-1914*. Berghahn Books, New York, 2000. ISBN 978-1-57181-166-0.

Stephen Nickell. Biases in Dynamic Models with Fixed Effects. *Econometrica*, 49(6):1417–1426, 1981. ISSN 0012-9682. doi: 10.2307/1911408. URL https://www.jstor.org/stable/1911408. Publisher: [Wiley, Econometric Society].

Peter Oestmann. *The Law of the Holy Roman Empire of the German Nation*, volume 1. Oxford University Press, August 2018. doi: 10.1093/oxfordhb/9780198785521.013.31. URL http://oxfordhandbooks.com/view/10.1093/oxfordhb/9780198785521.001.0001/oxfordhb-9780198785521-e-31.

Sheilagh Ogilvie and A.W. Carus. Institutions and Economic Growth in Historical Perspective. In *Handbook of Economic Growth*, volume 2, pages 403–513. Elsevier, 2014. ISBN 978-0-444-53546-7. doi: 10.1016/B978-0-444-53538-2.00008-3. URL https://linkinghub.elsevier.com/retrieve/pii/B9780444535382000083.

Sheilagh C. Ogilvie. Germany and the Seventeenth-Century Crisis. *The Historical Journal*, 35(2):417–441, 1992. ISSN 0018-246X. URL https://www.jstor.org/stable/2639676. Publisher: Cambridge University Press.

Mancur Olson. *The rise and decline of nations: economic growth, stagflation, and social rigidities*. Yale Univ. Press, New Haven, 1982. ISBN 978-0-300-03079-2 978-0-300-02307-7. OCLC: 230337475.

Mancur Olson. Dictatorship, Democracy, and Development. *The American Political Science Review*, 87(3):567–576, 1993. ISSN 0003-0554. doi: 10.2307/2938736. URL https://www.jstor.org/stable/2938736. Publisher: [American Political Science Association, Cambridge University Press].

W. Mark Ormrod, Margaret Bonney, and Richard Bonney. *Crises, revolutions and self-sustained growth: essays in European fiscal history, 1130-1830*. Shaun Tyas, Stamford, 1999. ISBN 978-1-871615-93-7.

Elias Papaioannou and Gregorios Siourounis. Democratisation and Growth. *The Economic Journal*, 118(532):1520–1551, October 2008. ISSN 0013-0133, 1468-0297. doi: 10.1111/j.1468-0297.2008.02189.x. URL https://academic.oup.com/ej/article/118/532/1520-1551/5088814.

Florian Ploeckl. The Zollverein and the Formation of a Customs Union. *University of Oxford Discussion Papers in Economic and Social History*, 84, 2010.

Felix Roesel. German Local Population Database (GPOP) (Version 1.0). August 2022. doi: 10.24355/dbbs.084-202207181844-0. URL https://leopard.tu-braunschweig.de/receive/dbbs_mods_00071017.

Dietmar Roßberg, Volker Michel, Rudolf Graf, and Ralf Neukampf. Definition von Boden-Klima-Räumen für die Bundesrepublik Deutschland. *Heft 7*, 59(7):155–161, 2007. URL https://www.openagrar.de/receive/openagrar_mods_00056830.

Karsten Ruppert. *Die Pfalz im Königreich Bayern: Geschichte, Kultur und Identität*. Verlag W. Kohlhammer, Stuttgart, 1. auflage edition, 2017. ISBN 978-3-17-032401-5.

Eckart Schremmer. *Die Wirtschaft Bayerns: vom hohen Mittelalter bis zum Beginn der Industrialisierung ; Bergbau, Gewerbe, Handel*. Beck, München, 1970. ISBN 978-3-406-02227-2.

Eckart Schremmer. The Textile Industry in South Germany 1750 to 1850: Some Causes for the Technological Backwardness during the Industrial Revolution; Investment Approach and Structure Approach. *Textile History*, 7(1):60–89, October 1976. ISSN 0040-4969, 1743-2952. doi: 10.1179/004049676793691748. URL https://www.tandfonline.com/doi/full/10.1179/004049676793691748.

Peter Schröder. The Constitution of the Holy Roman Empire after 1648: Samuel Pufendorf's Assessment in His Monzambano. *The Historical Journal*, 42(4):961–983, 1999. ISSN

0018-246X. URL https://www.jstor.org/stable/3020932. Publisher: Cambridge University Press.

Joseph Alois Schumpeter, 1883-1950. *Die Krise des Steuerstaats*. Leuschner & Lubensky, 1918, Austria, 1918.

Zef M. Segal. *The Political Fragmentation of Germany: Formation of German states by Infrastructures, Maps, and Movement, 1815–1866*. Springer International Publishing, Cham, 2019. ISBN 978-3-030-19826-8 978-3-030-19827-5. doi: 10.1007/978-3-030-19827-5. URL https://link.springer.com/10.1007/978-3-030-19827-5.

Max Spindler. *Handbuch der bayerischen Geschichte Bd. IV,1: Das Neue Bayern: Von 1800 bis zur Gegenwart. Erster Teilband: Staat und Politik*. Verlag C.H.BECK Literatur - Sachbuch - Wissenschaft, München, 2 edition, 2003. ISBN 978-3-406-50451-8 978-3-406-70455-0. doi: 10.17104/9783406704550.

Mark Spoerer. *Steuerlast, Steuerinzidenz und Steuerwettbewerb. Verteilungswirkungen der Besteuerung in Preußen und Württemberg (1815-1913)*. December 2004. ISBN 978-3-05-004088-2. doi: 10.1515/9783050084152.

Mark Spoerer. The Revenue Structures of Brandenburg-Prussia, Saxony and Bavaria (Fifteenth to Nineteetnth Centuries): Are They Compatible with the Bonney-Ormrod Model? In Simonetta Cavaciocchi, editor, *La fiscalità nell'economia Europea secc. XII-XVIII =: Fiscal systems in the European economy from the 13th to the 18th centuries: atti della "Trentanovesima settimana di studi, 22-26 aprile 2007*, number 39 in Serie II. Atti delle "settimane di studi" e altri convegni. Firenze University Press, Firenze, 2008. ISBN 978-88-8453-702-7 978-88-8453-703-4. OCLC: ocn229431981.

Mark Spoerer. The Evolution of Public Finances in Nineteenth-Century Germany. In José Luís Cardoso and Pedro Lains, editors, *Paying for the Liberal State*, pages 103–131. Cambridge University Press, 1 edition, February 2010. ISBN 978-0-521-51852-9 978-0-511-84510-9 978-1-107-68648-9. doi: 10.1017/CBO9780511845109.006. URL https://www.cambridge.org/core/product/identifier/CBO9780511845109A013/type/book_part.

David Stasavage. Was Weber Right? The Role of Urban Autonomy in Europe's Rise. *American Political Science Review*, 108(2):337–354, May 2014. ISSN 0003-0554, 1537-5943. doi: 10.1017/S0003055414000173. URL https://www.cambridge.org/core/product/identifier/S0003055414000173/type/journal_article.

Barbara Stollberg-Rilinger and Yair Mintzker. *The Holy Roman Empire: A Short History*. 2019. ISBN 978-1-4008-9026-2. URL https://www.degruyter.com/doi/book/10.23943/9781400890262. OCLC: 1104893475.

Gerald Strauss. The Holy Roman Empire Revisited. *Central European History*, 11(3):290–301, September 1978. ISSN 0008-9389, 1569-1616. doi: 10.1017/S0008938900018732. URL https://www.cambridge.org/core/product/identifier/S0008938900018732/type/journal_article.

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199, December 2021. ISSN 03044076. doi: 10.1016/j.jeconom.2020.09.006. URL https://linkinghub.elsevier.com/retrieve/pii/S030440762030378X.

Andreas Thier. Steuergesetzgebung und Staatsfinanzen in Preußen 1871-1893. In Gerhard Lingelbach, editor, *Staatsfinanzen, Staatsverschuldung, Staatsbankrotte in der europäischen Staaten- und Rechtsgeschichte*. Böhlau, Köln, 2000.

Frank B. Tipton. *Regional variations in the economic development of Germany during the nineteenth century*. Wesleyan University Press, Middletown, Conn, 1st ed edition, 1976. ISBN 978-0-8195-4096-6.

Jan Luiten Van Zanden, Eltjo Buringh, and Maarten Bosker. The rise and decline of European parliaments, 1188-1789: EUROPEAN PARLIAMENTS. *The Economic History Review*, 65(3):835–861, August 2012. ISSN 00130117. doi: 10.1111/j.1468-0289.2011.00612.x. URL http://doi.wiley.com/10.1111/j.1468-0289.2011.00612.x.

Vereins deutscher Eisenbahn-Verwaltungen. *Deutsche Eisenbahn Statistik für das Betriebs-Jahr 1866*. Rauck'schen Buchdruckerei, 1868.

Gottfried Eberhard Voelker. *The Impact of Railroads on the German Economy in the Late Nineteenth Century*. Ph.D., The University of Nebraska - Lincoln, United States – Nebraska, 1970. URL https://www.proquest.com/docview/302430633/citation/6F1B045829924848PQ/1. ISBN: 9798659572467.

Oliver Volckart. No Utopia: Government without Territorial Monopoly in Medieval Central Europe. *Journal of Institutional and Theoretical Economics*, 158(2):325, 2002. ISSN 0932-4569. doi: 10.1628/0932456022975411. URL https://www.mohrsiebeck.com/10.1628/0932456022975411.

Oliver Volckart. Rules, Discretion or Reputation? Monetary Policies and the Efficiency of Financial Markets in Germany, 14th to 16th Centuries. Technical Report 2007,007, Humboldt University of Berlin, Collaborative Research Center 649: Economic Risk, Berlin, 2007.

Arthur von Mayer. *Geschichte und Geographie der deutschen Eisenbahnen. I. Band, Theil I und II umfassend.* Wilhelm Baensch Verlagshandlung, Berlin, 1891.

Friedrich Carl von Moser. *Ueber die Regierung der geistlichen Staaten in Deutschland*. Frankfurt und Leipzig, 1787.

Fabian Wahl. Political participation and economic development. Evidence from the rise of participative political institutions in the late medieval German Lands. *European Review of Economic History*, 23(2):193–213, May 2019. ISSN 1361-4916, 1474-0044. doi: 10.1093/ereh/hey009. URL https://academic.oup.com/ereh/article/23/2/193/4995798.

Max Weber. *Die protestantische Ethik und der "Geist" des Kapitalismus*. Springer Fachmedien Wiesbaden, Wiesbaden, 2016. ISBN 978-3-658-07431-9 978-3-658-07432-6. doi: 10.1007/978-3-658-07432-6. URL http://link.springer.com/10.1007/978-3-658-07432-6.

Max Weber. *Economy and Society: A New Translation*. Harvard University Press, Cambridge, Massachusetts, 2019. ISBN 978-0-674-91654-8.

Joachim Whaley. *Germany and the Holy Roman EmpireVolume I: Maximilian I to the Peace of Westphalia, 1493-1648*. Oxford University Press, November 2011a. ISBN 978-0-19-873101-6. doi: 10.1093/acprof:oso/9780198731016.001.0001. URL https://oxford.universitypressscholarship.com/view/10.1093/acprof:oso/9780198731016.001.0001/acprof-9780198731016.

Joachim Whaley. *Germany and the Holy Roman EmpireVolume II: The Peace of Westphalia to the Dissolution of the Reich, 1648-1806*. Oxford University Press, November 2011b. ISBN 978-0-19-969307-8. doi: 10.1093/acprof:oso/9780199693078.001.0001. URL https://oxford.universitypressscholarship.com/view/10.1093/acprof:oso/9780199693078.001.0001/acprof-9780199693078.

Jeffrey M. Wooldridge. *Econometric analysis of cross section and panel data*. MIT Press, Cambridge, Mass, 2002. ISBN 978-0-262-23219-7.

Appendix A

Public and Private Railway Performance

The firms for which there is at least one year of data in Fremdling and Kunz (2011) were responsible for building 75% of the railway mileage constructed between 1835 and 1885. The missing performance data is mostly in the band of Prussia between Schleswig-Holstein and Silesia. There are also significant sections of Saxony and the Bavarian Palatinate missing.

First, this appendix points to evidence that the return on invested capital (ROIC) of state railways was significantly lower than private ones. The general trends for ROIC are visible in Figure A.1 Table A.1 shows that for each mark of capital invested in a state railway, two fewer marks of revenues would have been generated than by the same investment in a private railway. Note that ROIC says more about operational efficiency than on return on investment, since no data is available on subsidies or dividends.

Efficiency can also be measured by looking at shipping revenues per ton-kilometer and passenger revenues per passenger-kilometer (Tables A.2 and A.3. These measures also tell us the average prices customers would have faced when using the railways. Shipping revenues

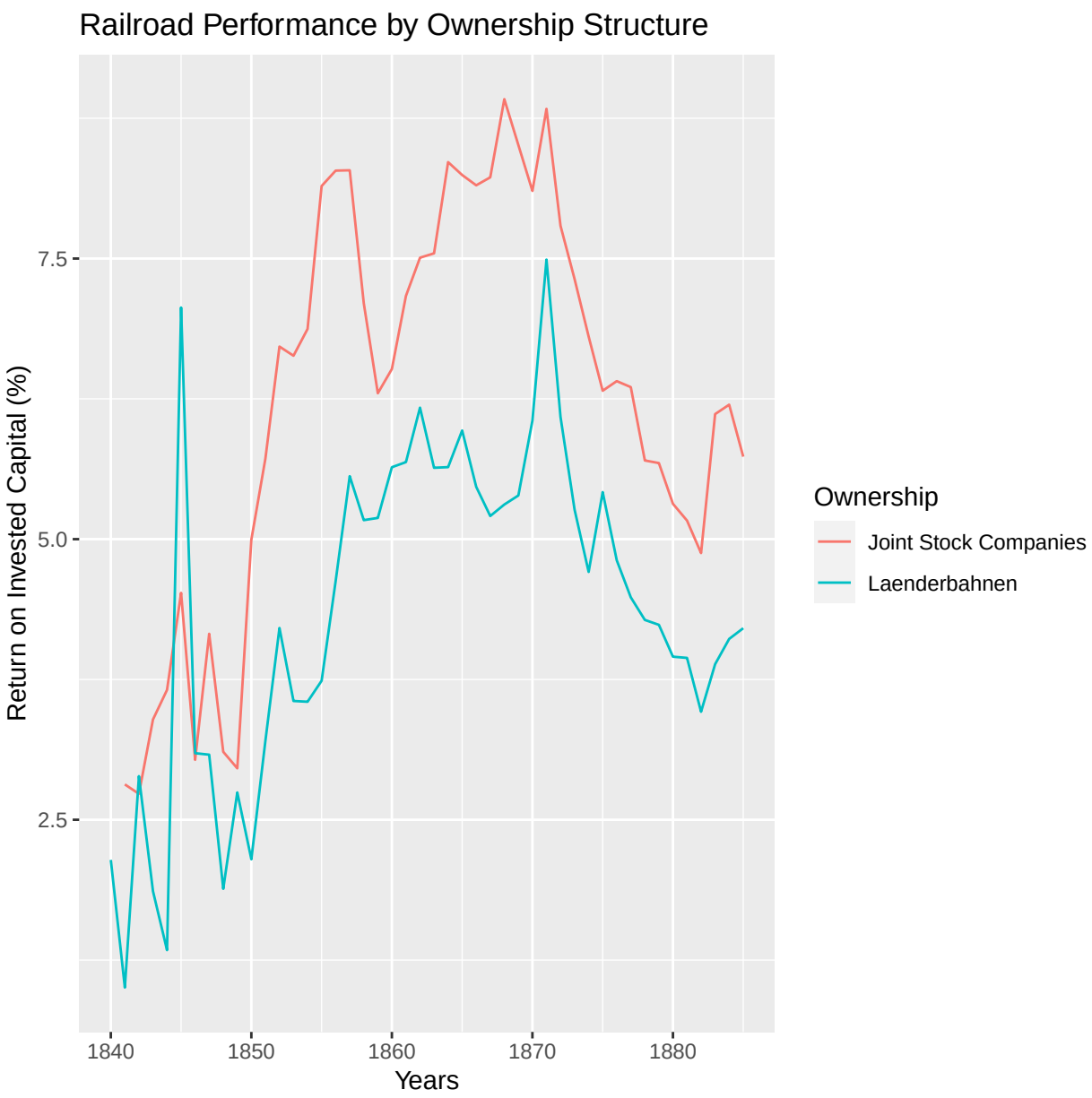


Figure A.1: Railway Firm Performance by Ownership

Table A.1: Firm Performance by Ownership

Dependent Variable:	ROIC
Model:	(1)
Variables	
OwnershipLaenderbahnen	-1.972**
	(0.8650)
Fixed-effects	
Year	Yes
Fit statistics	
Observations	1,813
R^2	0.35332
Within R^2	0.02545

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

for state and private firms track each other closely, both falling steadily until the early 1870s, when the measure begins to increase private firms. This divergence increases as nationalizations occur, so the higher revenues may be due to the location of the few remaining private firms.

Gaps for passenger revenues are largest initially, but the absolute size of the gap is small: only about 5 marks per passenger-kilometer. By the 1860s this gap has vanished. This suggests that passenger travel rates were more competitive, or perhaps simply more similar across regions.

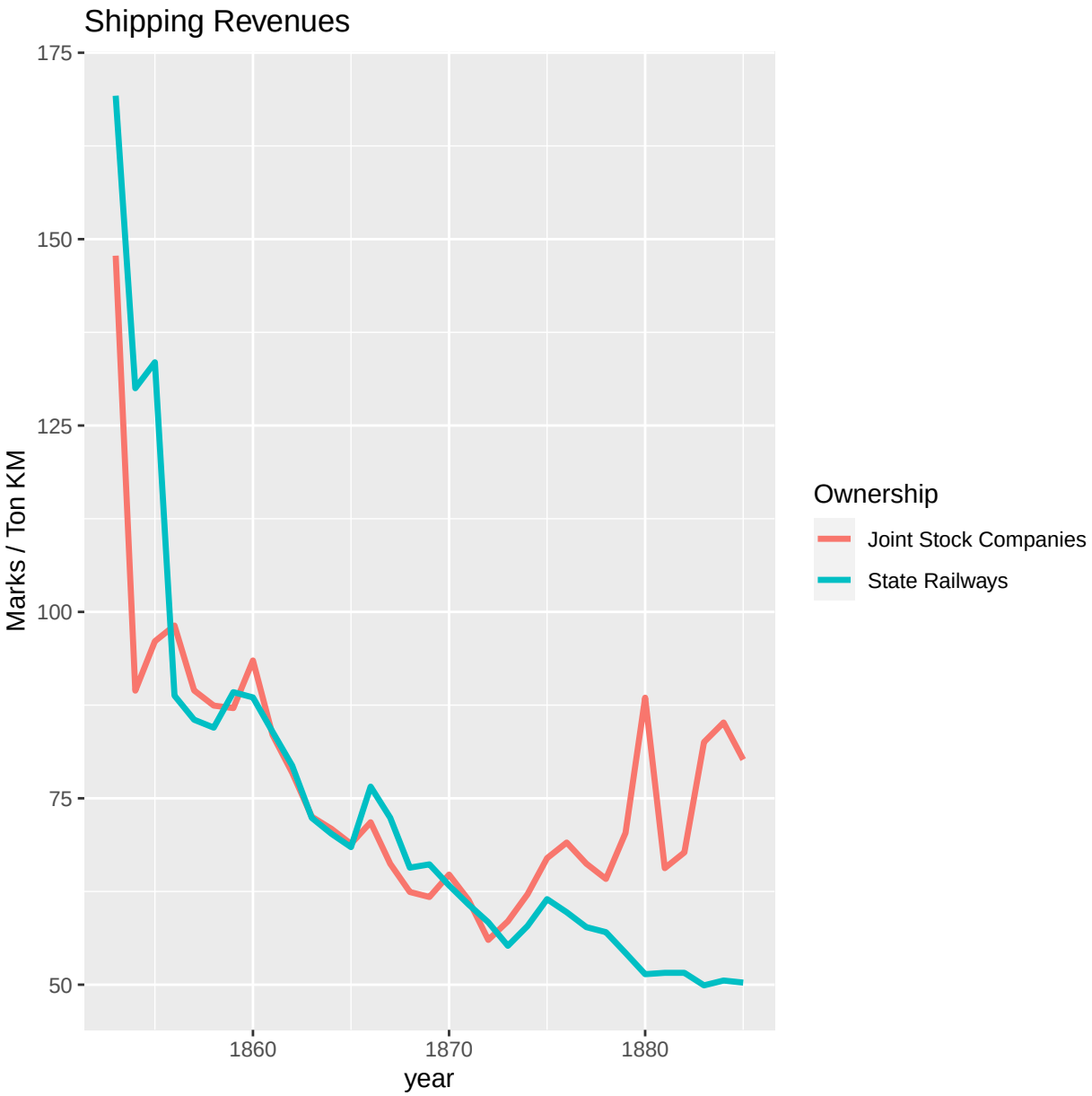


Figure A.2: Revenues for Shipping Goods

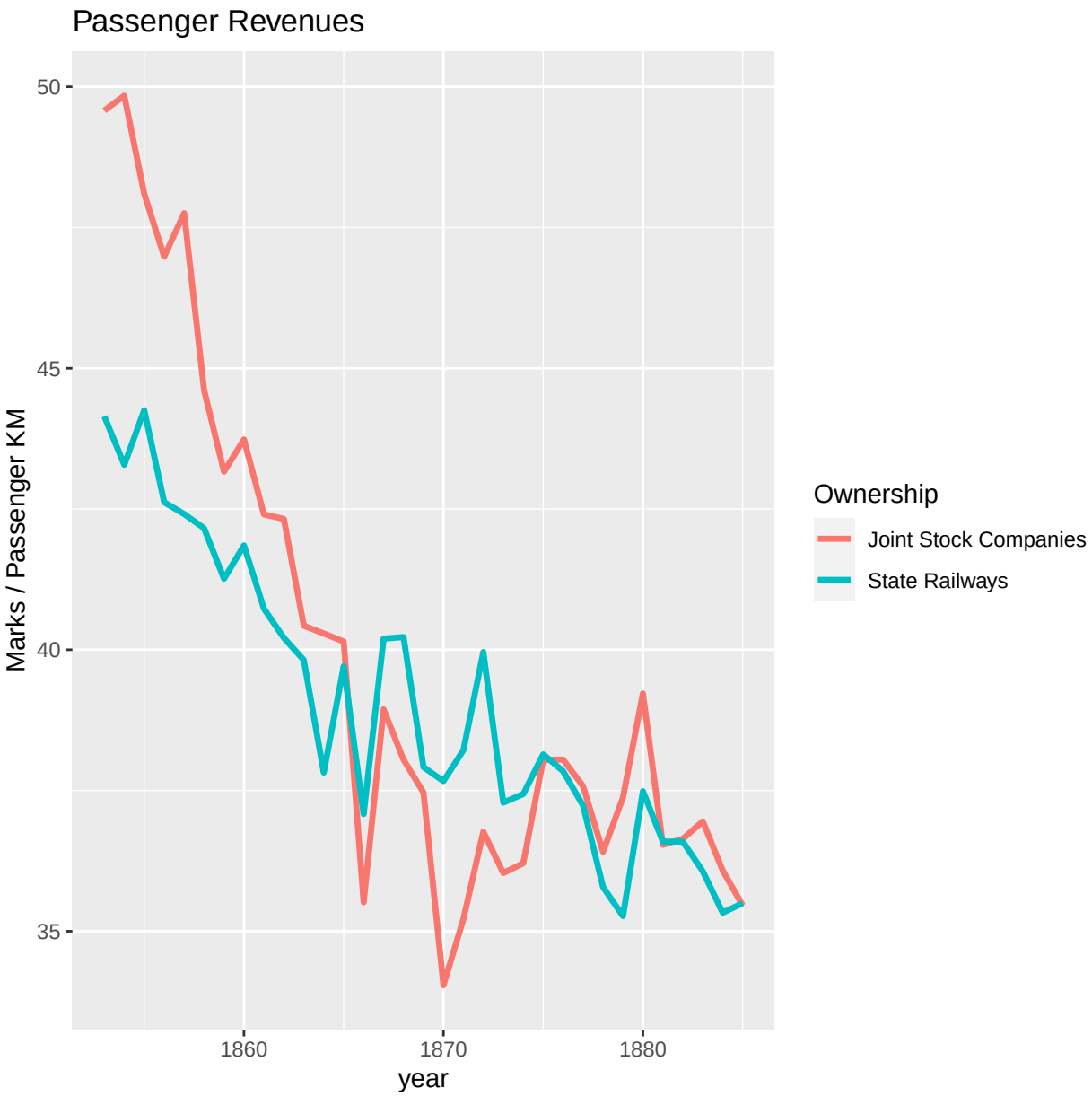


Figure A.3: Revenues for Shipping Passengers

Appendix B

State by State Railway Construction Trajectories

B.1 Bavaria

The Nuremberg-Fürth railway, the first German railroad, was built by (and remained for a long time) a private company. After 10 more years of mainly private construction, nearly all construction between 1845 and 1860 would be done by the state. From the late 1850s to 1875, there would be a resurgence in private construction in the eastern borderlands, the Palatinate, and Franconia while public construction remained dominant along the borders with Wuerttemberg. That the approach towards Franconia was similar to the approach to the Palatinate, despite its geographic proximity and contiguity with Bavaria, is interesting, and highlights the difficulties faced in integrating these regions highlighted by @segal2019. 1875 would see the nationalization of all rail in Bavaria except the Palatine, which would remain the largest Private railway until 1909 when it was also nationalized.

Independent railway management was perhaps more important to Bavaria than any other state. They refused to sign the constitution of the German Empire unless guaranteed "special priviliges", and one of the demands was the continued sovereignty of the Bavarian State Railway.

Table B.1: Expansion Rates: Bavaria

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	41	0.09	0.11	0.00	0.45
StateRate	41	0.06	0.10	0.00	0.45
NonStateRate	41	0.03	0.05	0.00	0.20

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

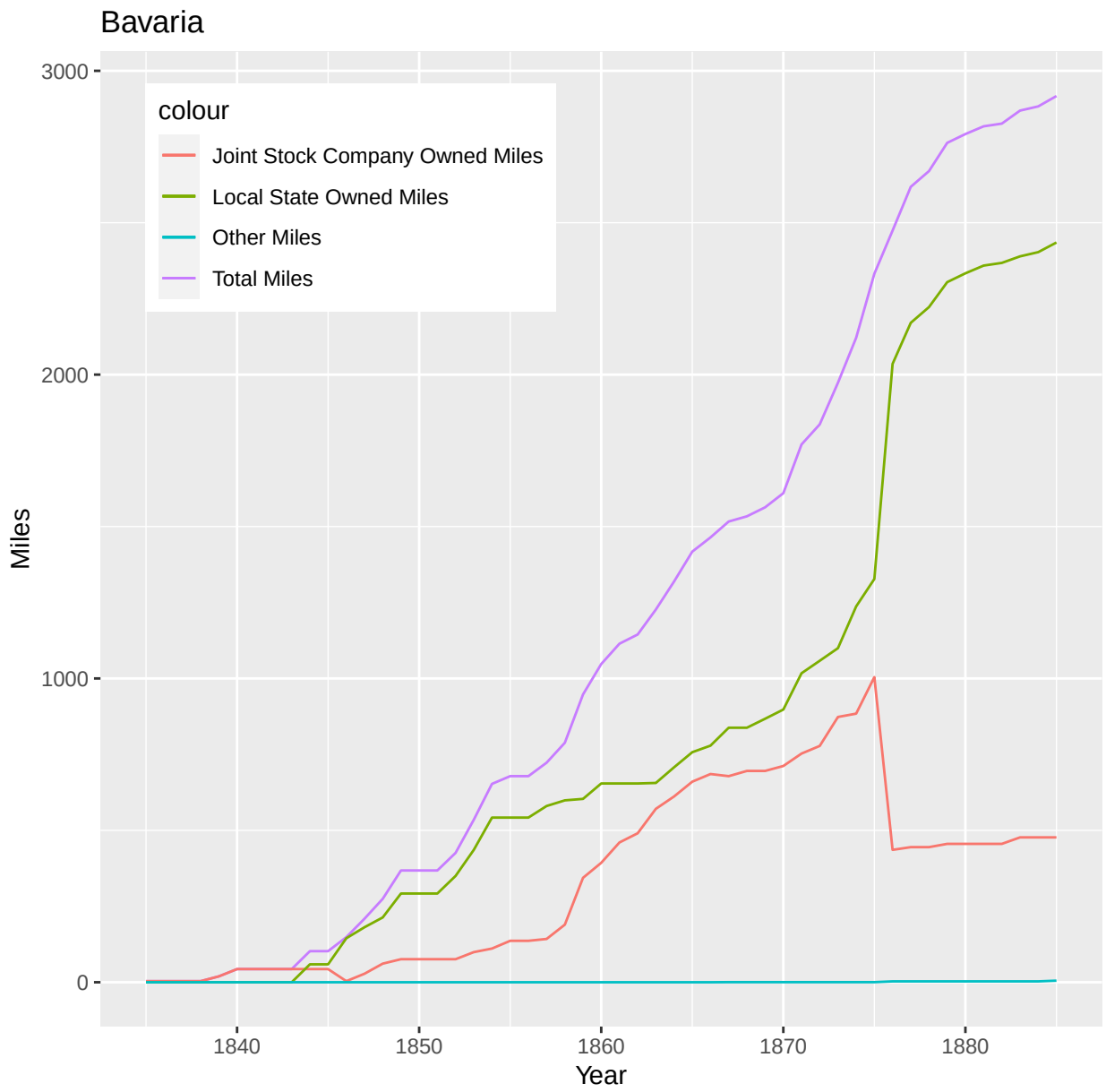


Figure B.1: Bavarian Railroad Construction

B.2 Saxony

After 10 years of exclusively private construction, Saxony nationalized the Saxon-Bavarian and Saxon-Silesian railway companies due to their financial insolvency. Public and private construction would continue at similar rates until total nationalization in 1876.

The Saxon case is of particular interest, because even though the first rail was built in Bavaria, Saxon planners and financiers were heavily involved in most early railroad plans (Beyer, 1978).

Table B.2: Expansion Rates: Saxony

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	47	0.07	0.09	0.00	0.34
StateRate	47	0.02	0.04	0.00	0.16
NonStateRate	47	0.05	0.09	0.00	0.34

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

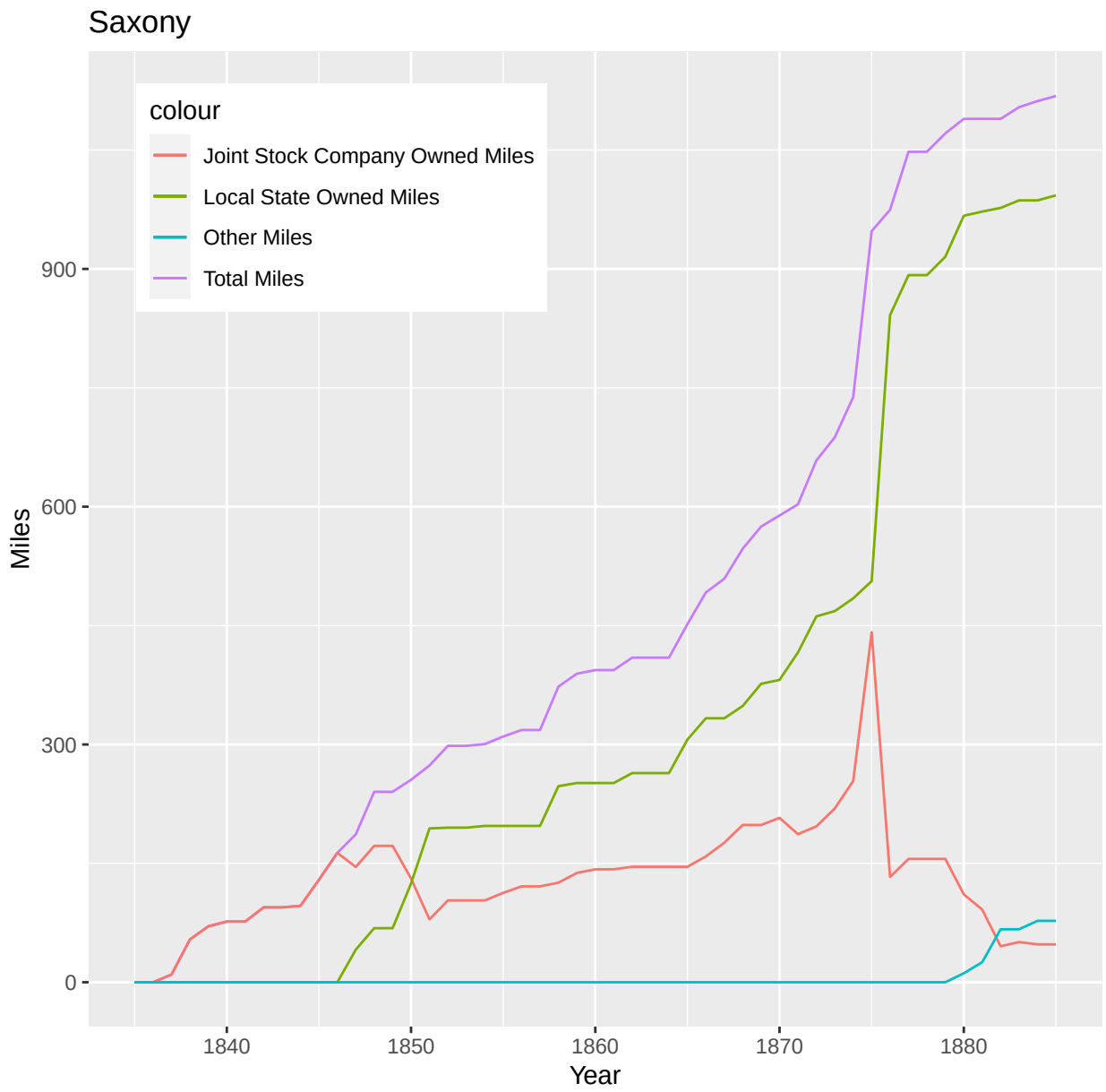


Figure B.2: Saxon Railroad Construction

B.3 Prussia

Mitchell (2000) emphasizes that Prussia was much more committed to liberalism than any of the other German states, and this is why the first 1000 miles of railroad would all be by private firms. There were no public railroads until 1850, when Prussia stepped in to manage railroads in the Saarland and the connections to the Palatinate. In 1851, public railroad construction would begin at a much larger scale in East Prussia. The first nationalization would be in 1852, of the Lower Silesian railway.

The pattern that emerges is that public railroads initially emerged in areas with significant strategic concern, as well as significant exports. Interestingly, even in the Rhineland, where JSCs dominated, the state railways managed the connections into Hesse and Hannover. This is a contrast to the connections through smaller states in central Germany and Thuringia, which tended to be handled by JSCs based in Prussia. Despite this increase in public construction, JSCs still dominated and their rate of growth was faster into the 1870s.

Nationalizations begin in 1875, and by 1879 JSCs are being nationalized faster than they can expand. Nationalization continues to accelerate until 1886, when nearly all remaining lines are nationalized; over 3000 miles were nationalized in one year.

Table B.3: Expansion Rates: Prussia

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	44	0.10	0.14	0.00	0.65
StateRate	44	0.01	0.02	0.00	0.09
NonStateRate	44	0.09	0.15	0.00	0.65

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

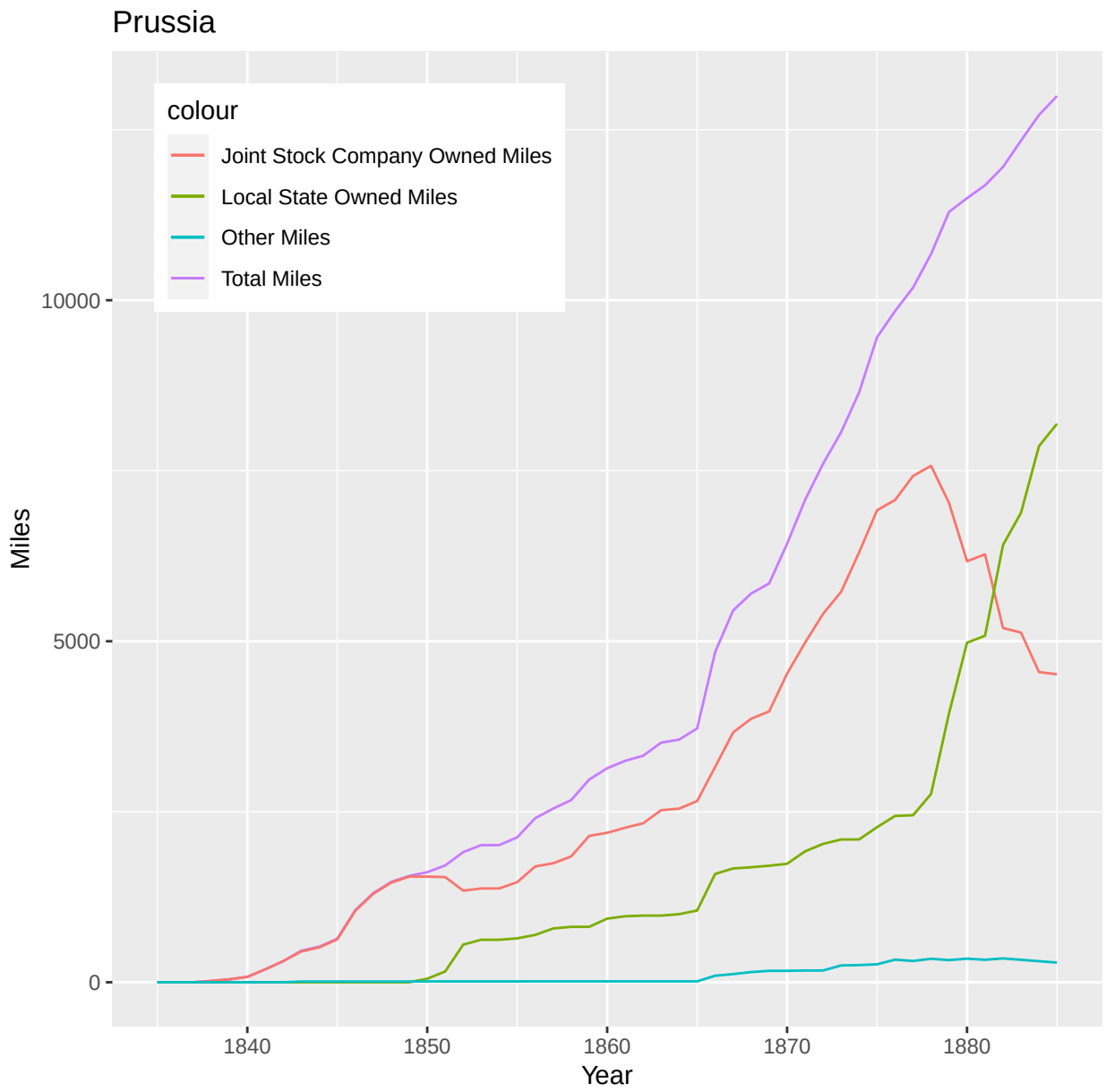


Figure B.3: Prussian Railroad Construction

B.4 Brunswick

100% state operated until the 1850s, when a small portion of mileage owned and operated by Hannover is allowed. All remaining construction is by the Brunswick State railway until the late 1860s, when some JSC activity occurs.

The Brunswick State Railway was privatized in 1870 (see Kleeberg (1990) for reasoning), then purchased by Prussia in 1884.

Table B.4: Expansion Rates: Brunswick

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	42	0.04	0.09	0.00	0.37
StateRate	42	0.03	0.08	0.00	0.37
NonStateRate	42	0.01	0.04	0.00	0.19

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

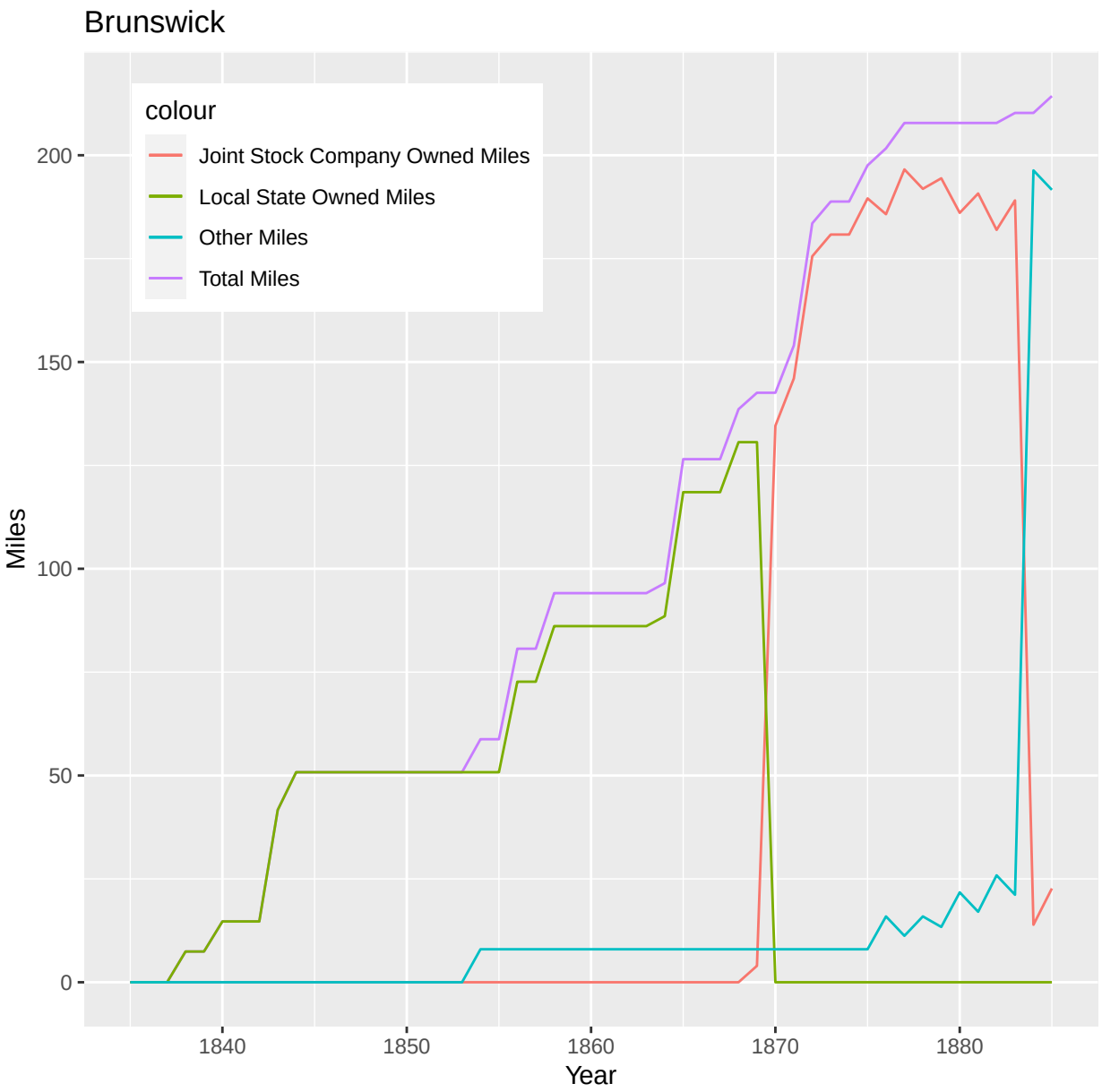


Figure B.4: Brunswick's Railroad Construction

B.5 Baden

The first 150 miles in Baden were all constructed by the Baden State Railway, and nearly all subsequent miles were as well. Baden does have an above average amount of municipal railways (Karlsruhe, Mannheim, Freiburg & Altbreisach, and Waldkirch). Baden is also noteworthy for being the only state to initially adopt a non-standard gauge, but did eventually switch to conform to the rest of Germany.

Table B.5: Expansion Rates: Baden

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	41	0.05	0.08	0.00	0.43
StateRate	41	0.04	0.08	0.00	0.43
NonStateRate	41	0.01	0.02	0.00	0.08

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

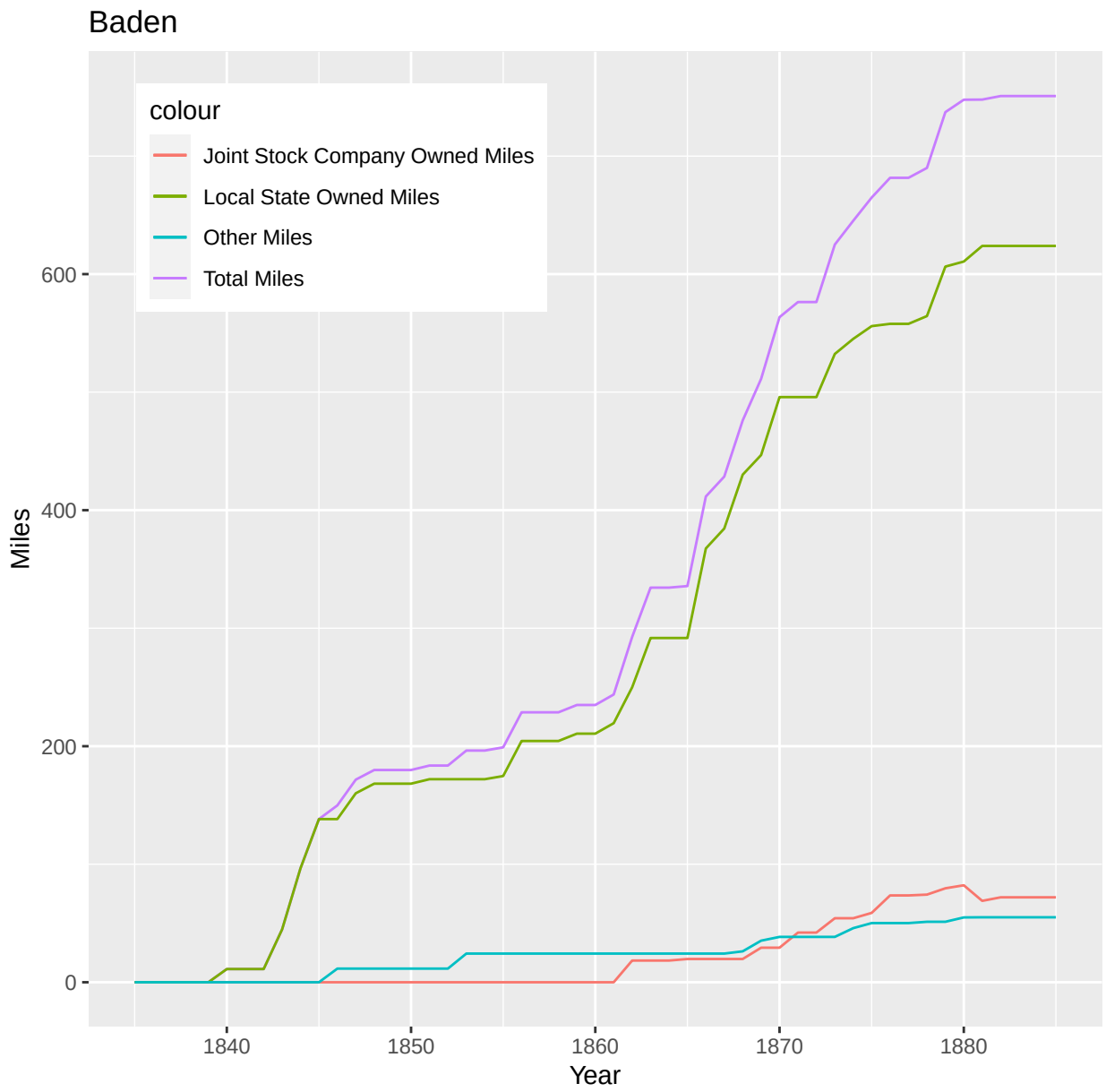


Figure B.5: Baden's Railroad Construction

B.6 Hesse-Darmstadt

One of the few cases where JSCs dominated. First miles were JSC, and except for a short stint from 1845-1857, JSCs owned the majority of railway miles. Even after the nationalization of the Upper Hessian company, JSCs remained the major player.

Caveat: I treat the Main-Neckar railway as being one of the state railways of Hessen-Darmstadt, however this is not entirely true. It was grouped with state railways by contemporaries [@deutsche1851], but the headquarters was placed in Hessen-Cassel, although there is a note that it was jointly administered by Hessen-Cassel, Hessen-Darmstadt, and Frankfurt. However, @dumjahn1984 does not mention the Main-Neckar company by name, and instead treats it as a state railway, noting only whether particular lines were constructed by "Hessen", "Hessen and Frankfurt", or "Hessen und Kurhessen"[1] Because none of the lines attributed solely to Hesse-Cassel lay within Hessen-Darmstadt, treating the remainder as the state rail of Hessen-Darmstadt seems accurate, except to the extent that Frankfurt was involved.

Table B.6: Expansion Rates: Hessen-Darmstadt

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	39	0.08	0.15	0.00	0.71
StateRate	39	0.02	0.12	0.00	0.71
NonStateRate	39	0.05	0.11	0.00	0.41

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

[1] In this context, Hessen refers to Hesse-Darmstadt and Kurhessen to Hesse-Cassel.

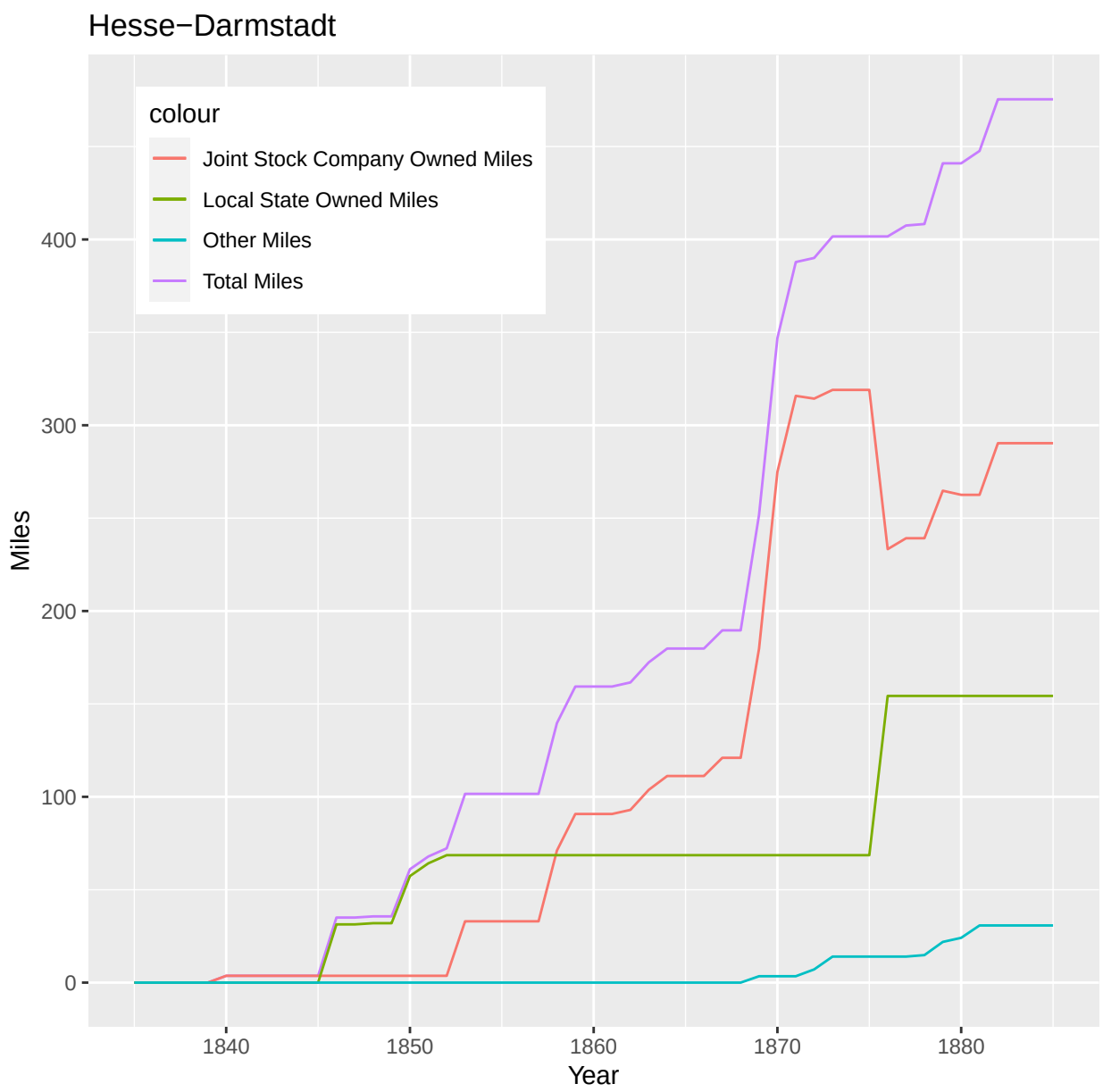


Figure B.6: Hessian Railroad Construction

B.7 Wuerttemberg

The purest example of a state system. Only two short lines operated by JSCs, and the "Other" miles are rounding errors from border crossings. HOFFMAN (1969) explains why.

Table B.7: Expansion Rates: Wuerttemberg

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	38	0.08	0.12	0.00	0.44
StateRate	38	0.08	0.12	0.00	0.44
NonStateRate	38	0.001	0.003	0.00	0.01

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

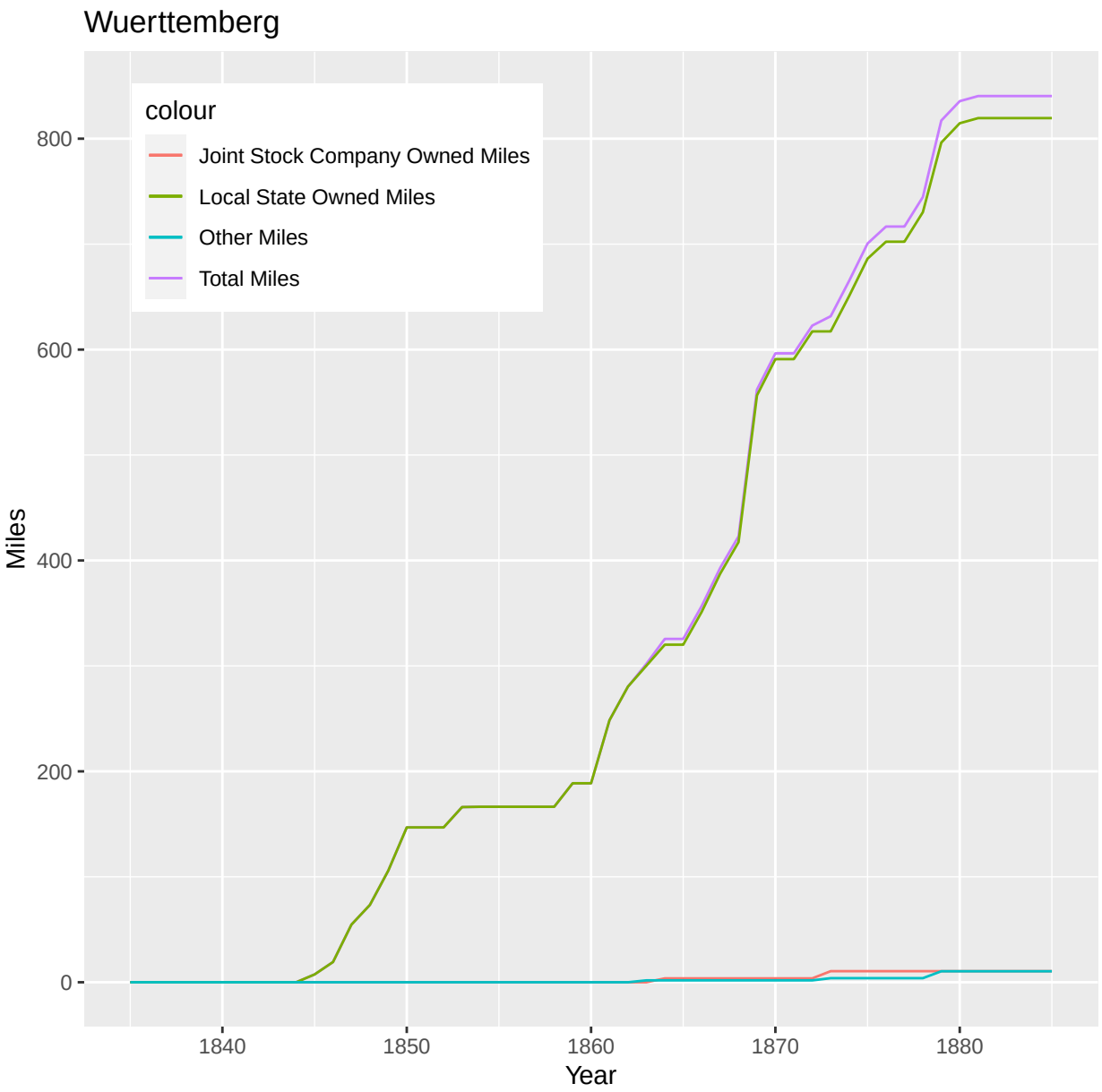


Figure B.7: Wuerttemberg's Railroad Construction

B.8 Saxony-Weimar-Eisenach

The only state in the sample which appears to have never operated its own railway. Almost all construction is by JSCs, except for a small section operated by the neighboring state of Schwarzburg-Sondershausen.

Table B.8: Expansion Rates: Saxony-Weimar-Eisenach

Statistic	N	Mean	St. Dev.	Min	Max
TotalRate	38	0.05	0.11	0.00	0.46
StateRate	38	0.00	0.00	0.00	0.00
NonStateRate	38	0.05	0.11	0.00	0.46

Sources: Derived by author from Kunz and Zipf (2008) and Dumjahn (1984).

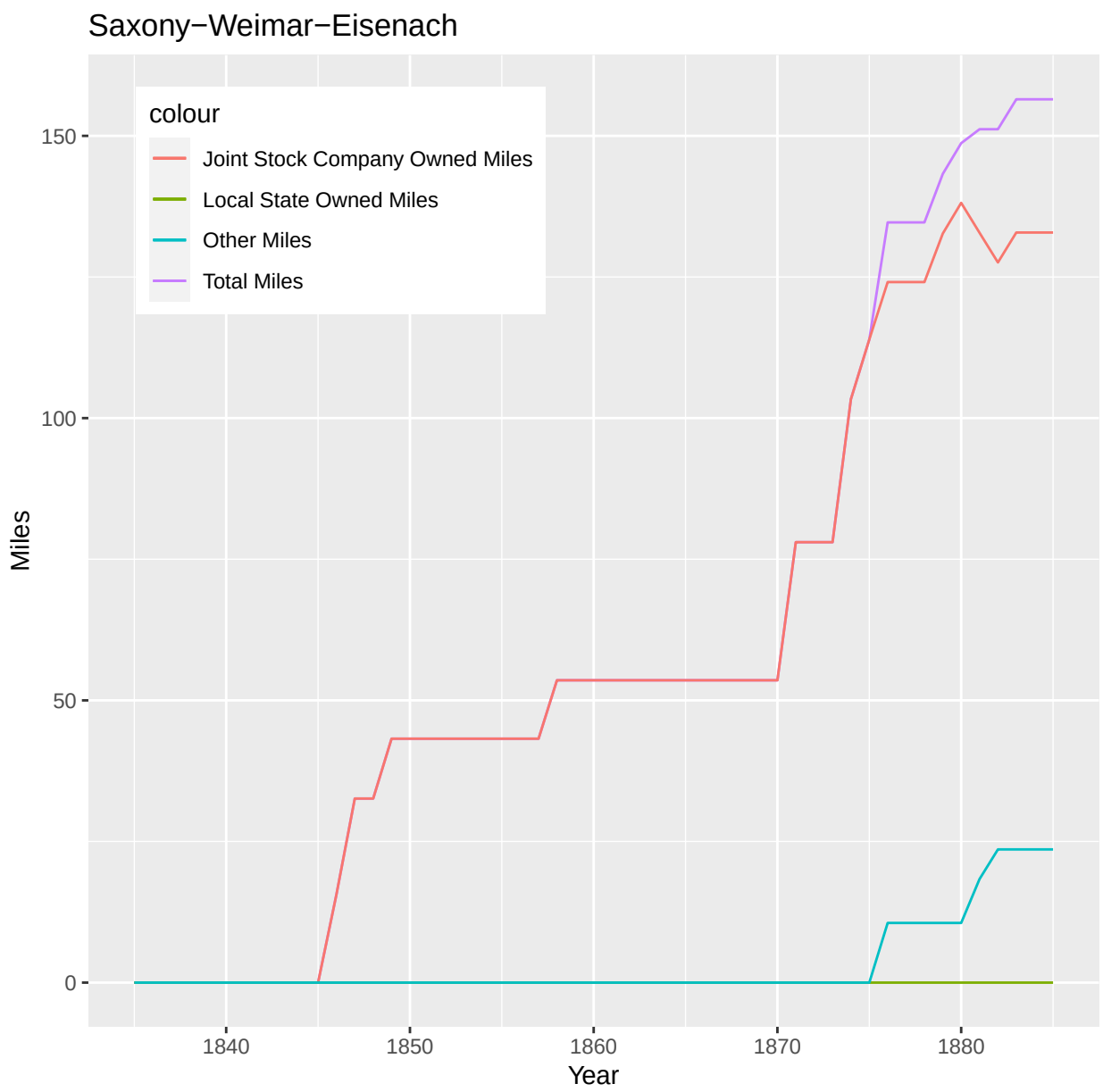


Figure B.8: Saxony-Weimar-Eisenach's Railroad Construction

Appendix C

Budget Data Sources

State	Years	Publication/Source
Baden	1820–1844	Großherzoglich Badisches Staats- und Regierungsblatt
	1845–1868	Großherzoglich Badisches Regierungsblatt
	1869–1885	Gesetzes- und Verordnungs-Blatt für das Großherzogthum Baden
Bavaria	1831–1837	Verhandlungen der Zweyten Kammer der Ständeversammlung des Königreichs Bayern
	1837–1848	Verhandlungen der Kammer der Abgeordneten des Königreichs Bayern
	1849–1871	Verhandlungen der Kammer der Abgeordneten des Bayerischen Landtages
	1872–1873	Gesetzblatt für das Königreich Bayern
	1874–1885	Gesetz- und Verordnungsblatt für den Freistaat Bayern
Brunswick	1832–1885	Gesetz- und Verordnungssammlung für die Herzoglich-Braunschweigischen Lande
Hessen-Darmstadt	1835–1885	Verhandlungen der Zweiten Kammer der Landstände des Großherzogthums Hessen
Oldenburg	1853–1885	Gesetzblatt für das Herzogtum Oldenburg
Prussia	1821–1885	Gesetzsammlung für die Königlich-Preußischen Staaten
Saxony	1835–1885	Löbe (1889)
Sachsen-Weimar-Eisenach	1830–1850	Verhandlungen des Landtags und der Gebietsvertretung von Sachsen-Weimar-Eisenach
	1851–1885	Boelcke (1906)
Württemberg	1820–1823	Königlich-Württembergisches Staats- und Regierungsblatt
	1824–1885	Regierungsblatt für das Königreich Württemberg

Table C.1: Major German States: Official Publications and Sources (19th Century)

Appendix D

A Brief History of Fiscal Federalism in Germany

The 19th century saw a massive increase in the fiscal capacity of German states (Figure D.1). After the collapse of the Holy Roman Empire, the importation of French institutions following the Napoleonic wars, and the massive consolidation of territory that followed the *Reichsdeputationshauptschluss*, German administrations were forced to undergo massive reforms in the early 19th century. As feudalism and absolutism gave way to constitutionalism, taxation became more regular and took new forms, initially leading to large increases in tax revenues per capita (Figure D.2). Spoerer (2010) summarizes these changes to the direct tax regimes: The southern German states tended to follow the French model most closely, with an emphasis on direct taxation of impersonal wealth (land, buildings, and business). Prussia's tax system was more personal. Prior to 1851, a head tax called the *klassensteuer* was levied based on social standing or occupation. As the bureaucracy became more sophisticated, this was transformed into a proper income tax. Saxony and Baden introduced income taxes in 1878 and 1884 respectively, and Prussia expanded the income tax again in 1891.

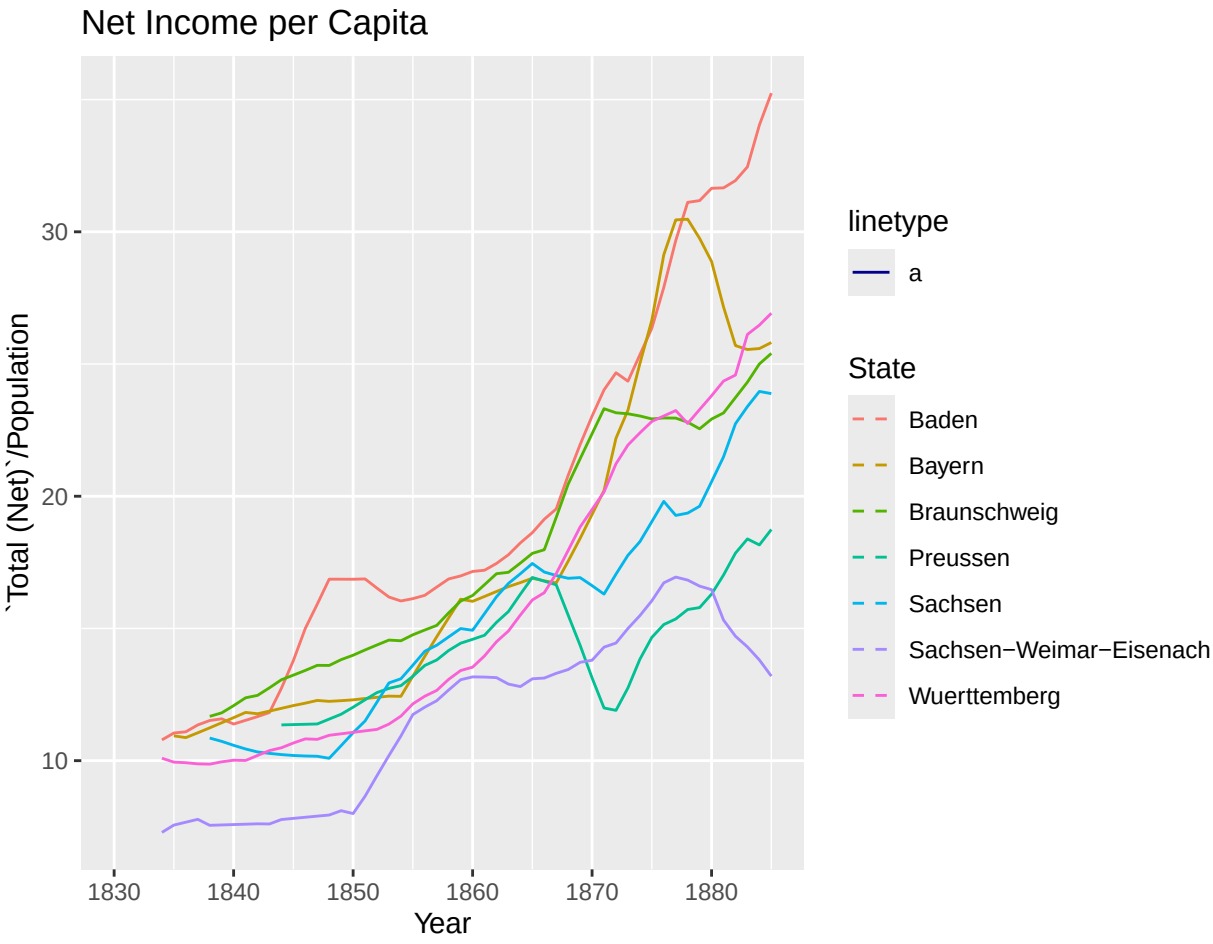


Figure D.1: Net Government Revenue per Capita, 5 year moving average.

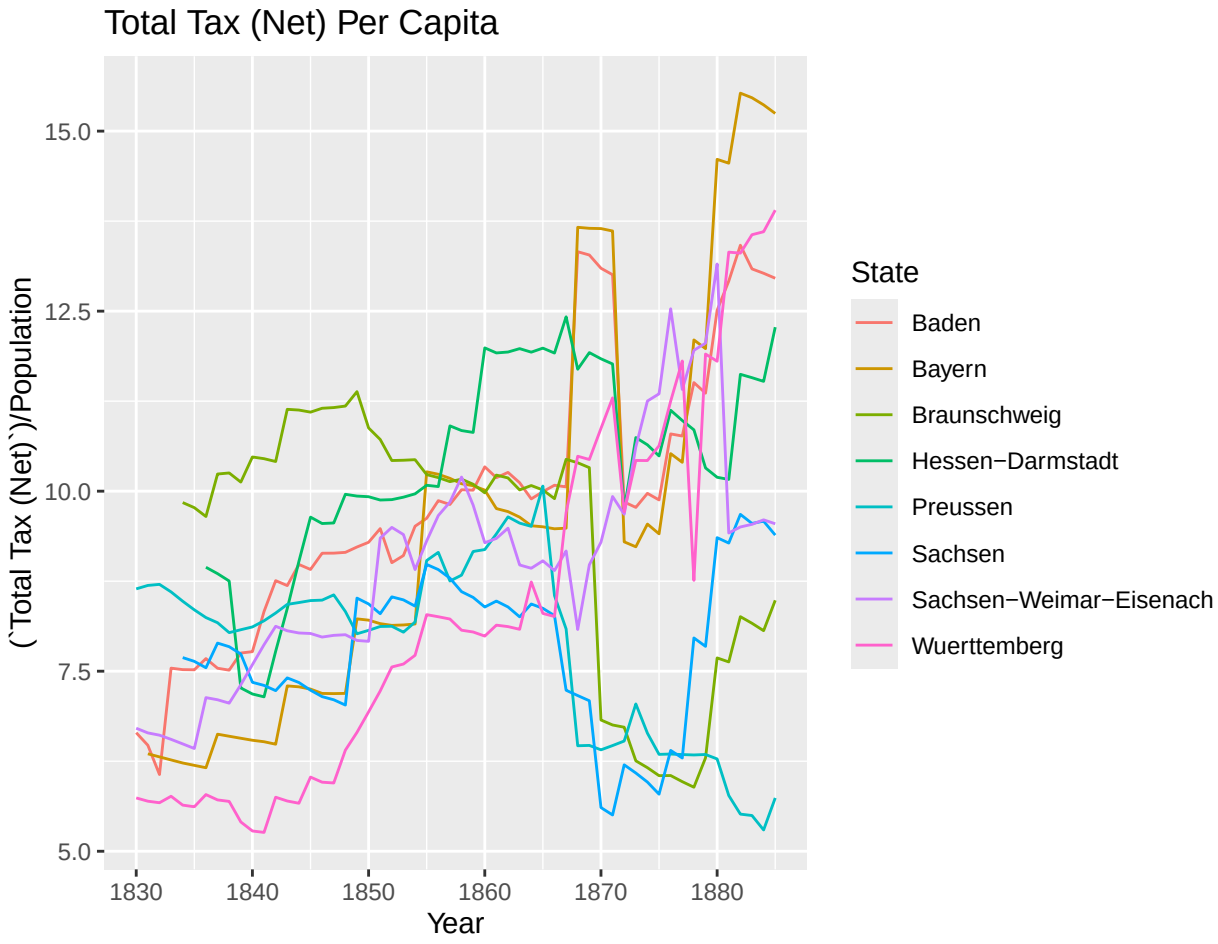


Figure D.2: Net Tax Revenue per Capita

Indirect taxation in Germany has a much more complicated history. The earliest forms were taxes levied on goods entering and exiting city gates, but with the near extinction of the free cities this was no longer relevant. Because of the administrative costs involved, consumption taxes were rare, and only Prussia continued to levy indirect taxes on milling and butchery in its cities. Because of the administrative costs involved, indirect taxes were mostly levied at borders as customs duties and tariffs, with the exception of transit taxes, as access to rivers and railroads was relatively easy to monitor.

Consequently, indirect taxation is one of the few policies administered by a central authority in Germany. By 1842, nearly all of the German states had joined the *Zollverein*, which eliminated customs borders between participating states. Tariffs were set and could only be changed by the unanimous consent of all members (Dumke, 1976). The arrangement was relatively liberal, with low duties compared to contemporary tariffs, and no restrictive quotas. The majority of revenues came from duties on luxuries (Millward, 2013). Most interesting about the *Zollverein* in the context of state fiscal capacity is how revenues were distributed: after members were compensated for the costs of customs administration at their borders, the remainder was shared on a per capita basis (Ploeckl, 2010). Incidentally, the need to properly allocate revenues was the impetus for regular census-taking in many parts of Germany (Gehrmann, 2009). This system is generally considered to have favored East Prussian agrarian interests over artisans and south Germany in general. On the other hand, the revenue distribution system led to lower revenues for the Prussian state, since its population was proportionately low with respect to its volume of trade. Another counterintuitive result of this system was that the smallest states with the least fiscal and administrative capacity became the most dependent on indirect tax revenue.

The *Zollverein* became obsolete after German unification in 1871, but indirect taxes generally remained the purview of central authorities. The new Imperial constitution respected the federal rights of the states to levy direct taxes, but customs and stamp duties as well as

most luxury consumption taxes were reserved for the *Reich* (Millward, 2013; Spoerer, 2010). One would expect this to lead to a decrease in indirect direct tax revenues, since what was formerly being remitted according to population was now being allocated to the *Reich*. The states, however, were wary of this result. When tariffs began to rise in the 1870s, the states became increasingly aware of just how much revenue was being lost to the central government. In response, they passed in Franckenstein claue in 1879 which set a limit on central indirect tax revenues. All revenues from tariffs and tobacco taxes above 130m *marks* would be redistributed to the states (Hefeker, 2001). Furthermore, as administrative capacity developed, indirect taxation on non-luxury goods became more feasible.

Taxation, however, is only half of the story. Prior to the development of regular taxation, the fiscal bureaucracy of the German states in the 18th and early 19th centuries was primarily concerned with effective management of domain properties, such as forests[1] and mines.[2] The traditional models of fiscal development, from Schumpeter (1918) to Ormrod et al. (1999) have predicted that as fiscal capacity develops, the "tax state" will emerge from the "domain state". As Spoerer (2008) points out, this was not the case in Prussia, Bavaria, or Saxony, and we can see tax's share of government revenues was decreasing across all of the major German states in the 19th century (see figure D.3). However, this does not mean that German states were stagnant or failing to develop more sophisticated revenue streams. Beyond the tax reforms discussed above, the German states were becoming increasingly involved in public enterprise.

The most important public enterprise, of course, was the state railway. Fremdling (1980) shaped all future discussion when he convincingly argued that the Prussian government used railway revenues as a substitute for taxation, or, as Spoerer (2004) terms it, "indirect indirect taxation". One historian even referred to this as the emergence of the "railway state" (Thier,

[1]Lowood et al. (1990) discusses how the "cameral sciences" emerged out of the need to manage forests.

[2]See Cantoni et al. (2024) for a discussion of how the emergence of cameralism and scientific management principles influenced the growth of early German states.

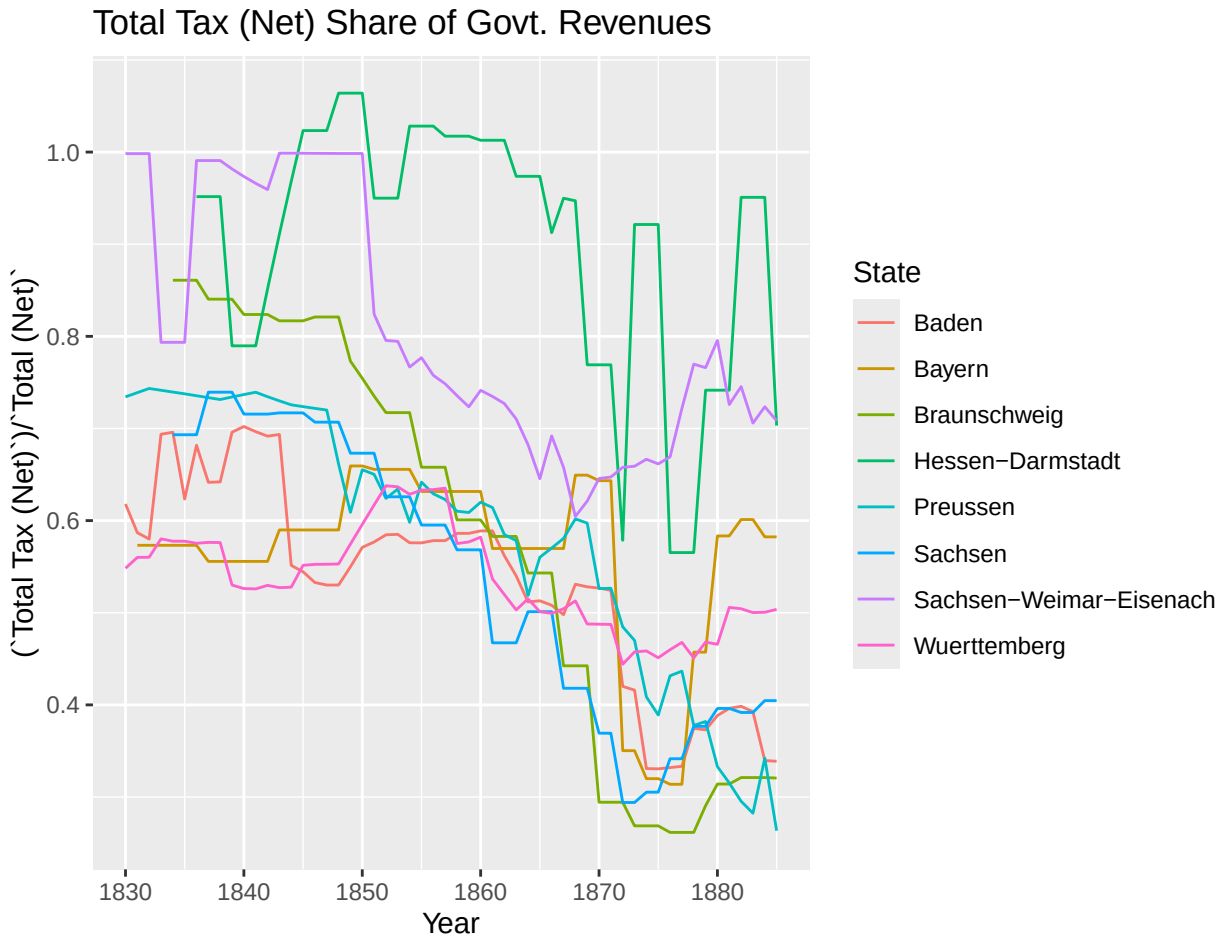


Figure D.3: Tax as a Share of Government Revenues

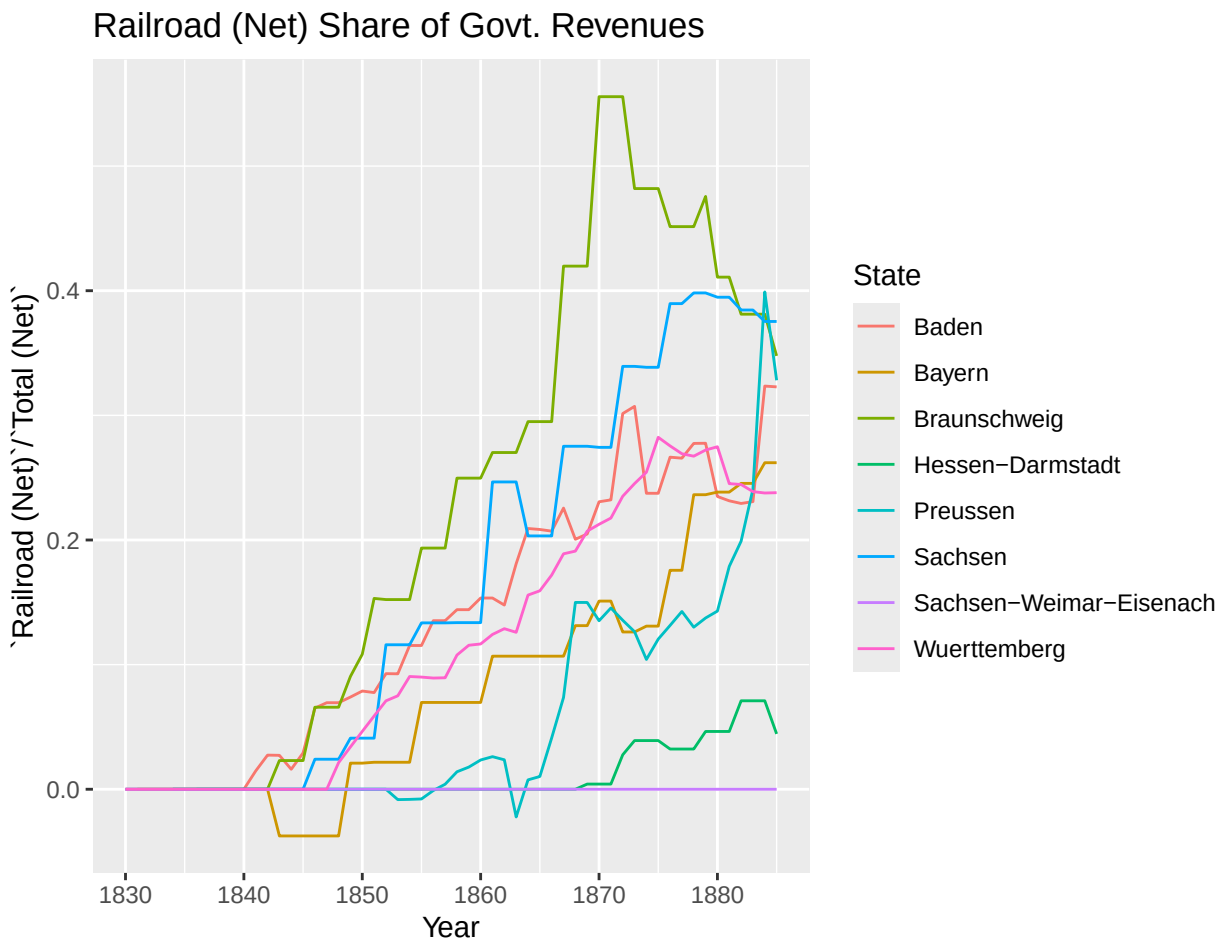


Figure D.4: Railroad Income as a Share of Government Revenues

2000). Figure D.4 shows clearly how revenues related to railroads were rapidly increasing in importance across Germany. It is evident that the "railway state" phenomenon was not unique to Prussia.

In summary, the 19th century saw a great expansion and sophistication of the German states' fiscal apparatuses. The old tax system relied on unpredictable and impersonal extraordinary taxes. New, modern institutions, as well as an expanding bureaucracy facilitated regular taxation that was increasingly tied to actual economic activity. Along with increased taxes, governments increasingly applied their skills managing domain assets to the expansion of public enterprise. Most important among these public enterprises by far was the railroad, which contributed directly and significantly to most states' budgets.