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The Link Between Fundamentals and Proximate Factors in Development
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ABSTRACT

The paper introduces a framework for studying the hierarchy of growth factors, from deep to more immediate. The specific setting we examine is 18th and 19th century Germany, when institutional changes introduced by reforms and transportation improvements converged to create city growth. We assess the impact of institutions on growth by allowing two ways for institutions to affect growth. Institutions can directly affect growth, or it can impact on trade, which in turn affects growth. Once we separately quantify the link from institutions to trade, and trade to growth, the independent effect of institutions on growth is small. This suggests that part of what is often understood as trade's effect on growth can be attributed to institutional change. It is straightforward to apply this framework to other settings.

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1 Introduction

Recent research on growth has begun to examine the deeper, fundamental causes of growth. This has proved very productive because relative to proximate factors, the greater distance between fundamental factors and economic growth allows for a more powerful causal analysis. At the same time, the relationship between the more proximate causes of growth and the deeper causes remains a bit of a puzzle. Do fundamental changes such as institutional reform affect the incentives to accumulate capital, to engage in trade, to adopt new technologies, or all of these things? To date we know little on how fundamentals exert their effect on the causes, or other manifestations, of growth.

As a result of the French Revolution, institutional changes were imposed upon certain areas in Europe and in particular, a number of German areas experienced drastic improvements in their economic institutions. Later, in the 19th century, parts of Germany saw major improvements in interregional connections due to steam railways. A priori, deeper institutional changes and transportation improvements could have had independent effects on city growth. Alternatively, these effects could be interdependent, in which case it would be important to be able to separately quantify the channels through which institutions impact on city growth.

We begin by showing that both institutions, as well as railways, affect the proximate factor that we focus on in this paper, namely, trade: $Trade = Trade(Inst, Rail)$. Our measure of trade is based on spatial price gaps. Since many factors have transactions cost aspects to them, price gaps reflect the strength of integration between markets. We then link institutions via trade to growth: $Growth = \beta_1 Trade(Inst, Rail) + u$. Central to the paper is that we separate the growth impact of institutions through trade from non-trade channels by including an independent institutions effect: $Growth = \beta_1 Trade(Inst, Rail) + \beta_2 Inst + u$. The separate identification of β_1 and β_2 is possible because trade is not only affected by institutions but also by other transactions-cost reducing developments of the 19th century, in particular steam railways. A key finding of the paper is that institutions affect growth to a substantial degree through trade in this period.

Recent work has shown that institutions have a strong effect on growth (Helpman 2004, Acemoglu, Johnson, and Robinson 2005a, La Porta et al. 2008 provide reviews). We also know that institutions affect trade (Greif 1993, Nunn 2007; review by Nunn and Trefler 2013). In addition, a few papers have established a significant relationship between trade and growth (Frankel and Romer 1999), in particular by showing that comparative advantage and the degree of competition affect growth (Young 1991, and Bloom, Draca, van Reenen 2011 respectively). This paper provides a unifying framework by linking institutions, trade, and growth. Our result says that in the set X of $Inst \Rightarrow X \Rightarrow Growth$, trade is important. Arguably, this goes beyond a simple change of labels. Both to understand growth and to assess the early effectiveness of policy, it is necessary to know the chain of events that institutional change sets in motion.

To the extent that trade gives rise to gains in welfare and income, one can view trade as operating at a level similar to other proximate factors, such as physical or human capital.[1] A counter-perspective might be that trade is not so much a proximate cause of growth as an aspect of institutions. In fact, much of the initial work on institutions was interested in capturing the impact of a broad cluster of institutions (Acemoglu, Johnson, and Robinson 2001), whereas later work aimed at pinpointing a crucial subset of institutions (Djankov et al. 2003, Acemoglu and Johnson 2005). If one thinks of trade as an aspect of institutions, then this paper can be viewed as a contribution to the literature on unbundling institutions.

As the placement of railways may have occurred non-randomly, we employ a measure of railway costs, which varies with geographic features at the sub-state level, as an instrumental variable.[2] Of course, geography can have direct effects on trade and on growth (Krugman and Venables 1995), in which case it would not be a valid instrumental variable. Our relatively disaggregated data allows for richer identification strategies than can be applied with country-level data (Nunn 2009).[3] In addition, we can employ placebo checks since steam locomotives were not available in Germany for part of the sample period. The strategy will be successful if conditional on covariates railway costs affected railway building during the later part of the

[1] While trade is less commonly seen as a proximate factor compared to factor accumulation, others before us have considered trade as a proximate factor (Rodrik, Subramaniam, and Trebbi 2004, 132).

[2] Duflo and Pande (2007) and Nunn and Puga (2012) also employ terrain in their studies.

[3] Moreover, city size, our outcome variable, exhibits the same broad patterns as per capita GDP at the country level (Acemoglu, Johnson, and Robinson 2005b), so the issue of whether sub-national results apply at the macro level is not a concern.

19th century. If, however, there is something correlated with railway costs that also switches on in the late 19th century, then identification fails. We perform a number of different analyses to defend the approach.

Similarly, the challenge in estimating the impact of institutions on growth is that both may be driven by a third factor, or there is reverse causation. It is indeed rare that one can treat institutions as exogenous. The shock of French rule to German institutions around 1800 was unusual in that it was plausibly exogenous, and similar to Acemoglu, Cantoni, Johnson, and Robinson (2011; ACJR for short) we use it as our second instrumental variable. The Germans did not choose French institutions; rather, those institutions were imposed upon them. We first use French rule and railway costs as instrumental variables to establish that institutions and railways impact trade. In the next step we use French rule and railway costs as instrumental variables for showing that institutions affect growth primarily through trade. Sections 4 and 5 give a more detailed exposition that provide support for this strategy.

We are not the first to distinguish fundamentals from proximate growth factors, and neither are we the first to examine the hierarchy of growth causes.[4] There is evidence, for example, that Protestantism in 19th century Prussia raised income mostly because it led to human capital accumulation (Becker and Woessmann 2009). Also, Dell (2012) proposes inefficient land reform as the mechanism why conflict in early 20th century Mexico had negative economic consequences. Identification is a central challenge in any study of the hierarchy of growth factors. Nineteenth century Germany provides a setting in which a causal growth analysis of fundamentals and proximate factors can be pursued.

The finding of a significant role for railways in 19th century Germany fits well with recent studies on the impact of infrastructure projects (Michaels 2008, Duranton and Turner 2012), especially railways in history (Donaldson 2012, Donaldson and Hornbeck 2012, Hornung 2012). The main difference between our analysis and other infrastructure papers is that we include the institutional conditions that were behind railway building as the central element of the analysis. We do this by applying the well-known two-sample instrumental variables approach developed

[4] An influential example is Rodrik, Subramaniam, and Trebbi (2004). Often the so-called mechanisms question concerns also the hierarchy of growth causes. In an earlier paper, we have studied a hierarchy of market size effects (Keller and Shiue 2008).

by Angrist and Krueger (1992). It allows us to employ rich bilateral data on prices and railway lines by combining this information at the city-pair level with information on population size and institutions at the city-level.

It is likely that during this period, there were not only other fundamentals apart from the one we investigate–including geography (Diamond 1997, Sachs 2001), cultural environment (Clark 1987), or religion (Weber 1930)–but other proximate factors as well besides trade. Although we do consider a broad range of alternative explanations in order to ensure that our fundamental and proximate factors are both important to growth, our motive in this paper is not to rank the relative importance of one fundamental versus another, or one proximate factor versus another. Rather, the contribution is to formulate and test a set of empirical implications on the role of institutions for economic performance.

The paper is structured as follows. The following section gives the background necessary for interpreting the results. Section 3 introduces the sample and discusses the main variables, with additional information given in Appendix A. In section 4 we estimate the impact of institutions and railways on price gaps. Section 5 turns to the impact of institutions on growth, showing that trade can be separated from other channels through which institutions may work. This section also introduces the two-sample approach of mapping city-pair to city observations, and concludes with a discussion of the main findings. More information on our two-sample instrumental variables approach is found in Appendix B. A number of final issues are raised in section 6.

2 Historical Background: German Cities in the 19th Century

Over much of the 19th century, a central feature of the region known as Germany today was that despite centuries of shared language and culture, there were numerous independent states that chose their own policies and institutions. The 1848 borders of these states are shown in Figure 1. Politically, the German states had shifting alliances, at times fighting wars on opposing sides. Economically, their institutions and policies differed. Larger states such as

Prussia or Bavaria were geographically disjoint and cities in different parts of the same state were subject to different policies. This motivates why our analysis is at the sub-state level of the city, rather than at the state level. The location of the sample cities is shown in Figure 2, and their characteristics are discussed in section 3.2 below.

This era affords us with a mixture of common and idiosyncratic shocks that affected German economic development during the 19th century. Two of these stand out and are at the center of our analysis. First, there was change in the economic institutions governing these areas which was parallel to broader waves of political changes. The French Revolution in 1789 was the major source of new ideas that swept throughout Europe. In Germany it led to deep institutional change that varied across regions according to how strongly a particular region was affected by French rule. Although to some extent reversed during a period of restoration after 1815, the French Revolution had a profound impact on German regional development throughout the 19th century, which may have only been superseded by the political unification of Germany in the year 1871. Our sample period ends soon after (in 1880), and it begins in the year 1820 after the disruptive consequences of the Napoleonic armies had faded away.

Second, Germany experienced a transport revolution during the 19th century in the form of the rapid building of steam railways. The introduction of railways was accompanied by widespread economic change in many countries (Fogel 1964, Donaldson 2012), and in Germany it has been argued that the associated reduction in the costs of trade affected not only the patterns of specialization but also the pace of structural change (Fremdling 1975, Gutlerbet 2012). Unlike in the politically unified France, railway building in Germany was a highly decentralized decision, where local governments and business groups mattered (see the histories of individual railway lines in Fremdling, Federspiel, and Kunz 1995).

These two features taken together make for a unique, almost laboratory-style setting in which to study the relationship between institutional change, trade, and growth. The next section provides additional information on railways in Germany, followed by a synopsis of institutional changes as a consequence of French rule.

2.1 Steam Railways in 19th Century Germany

The invention of steam locomotives in England in the early 1800s fueled widespread railway line construction, and it was arguably the most important of a series of transport innovations (paved roads, improvements in waterways) that accompanied 19th century economic growth. The first German railway track was a 4-mile line from Nurnberg to Fürth opened in December 1835. The first longer track was opened between Dresden and Leipzig in the year 1841 (70 miles). Thereafter, additional miles of rail were laid down swiftly, and between 1840 and 1877 the length of German railway lines grew 60-fold. The pace of railway line building in Germany during this time was faster than either in England or in France (Putzger 2003). Figure 3 shows the German railway system by 1880, the last year of our sample.

How did this system emerge? One person who saw the potential of steam railways early on was the German-born economist Friedrich List, who proposed a national plan for Germany's railway system in the year 1833; it is shown in Figure 4. List's plan was in fact never implemented. What List had planned was to connect the larger German points of trade with each other. The reality, however, was quite different (see Fremdling, Federspiel, and Kunz 1995). In the year 1850, for example, the major Southern cities of München (Bavaria), Stuttgart (Wurttemberg), and Karlsruhe (Baden) were still not connected (see Figure 5). Also, the train line from the Holstein city of Kiel heading south ended for a long time in the city of Altona (also Holstein), just short of the major port of Hamburg. The reason for these 'omissions' was that the development of train lines often focused on the states' own territories, without much consideration given to national railway interests. Therefore it is unlikely that the train system as it actually evolved was optimal in the sense of maximizing German economic welfare.

The immediate impact of steam trains was that transport costs between two cities were reduced (Fremdling 1995). In 19th century Germany, average freight rates on roads are estimated at around 40 Pfennige per tonkilometer. Early railways cut these rates to about 11, and rail improvements brought this down to less than 4 Pfennige by the end of the sample (Gutlerbet 2012, Table 2). We study the impact of railways by comparing the speed with which prices converged between cities served by railways versus between cities not served by railways. Generally, railways were important for the transport of many low value-to-weight ratio goods, including grain (O'Brien 1983, 1-2). While we do not have comprehensive information on wheat traded via railroads, the great majority of all Bavarian grain exports to the south in the early 1850s was transported on railways (Seuffert 1857, Chapters 5, 6). Even though there is evidence that railways mattered, the importance of railways for transporting grain varied greatly because transporting grain by ship was cheaper yet. In the late 19th century, for example, sending grain from Posen (in East Prussia) to Cologne by railway was at least three times as expensive as transporting it by ship via Rotterdam and the Rhine River (Köttgen 1890, 64).

Some evidence on price gaps before and after the introduction of railways is shown in Figure 6. The average wheat price gap in the years before steam rail transport is around 0.15, and it falls to about half that after a railway line is opened. While the placement of steam trains is taken as exogenously given and alternative explanations are not accounted for, this figure is consistent with railways bringing down price gaps. There is also evidence for heterogeneous effects, with a decline of 0.20 for initially high price gaps (at the 95th percentile).

2.2 French Rule and Institutional Change

The main driver of institutional change in Germany during this time was arguably the influence from France.[5] The ideas of the French Revolution of 1789, including separation of powers, democracy, and human rights, found many followers in German areas, even though after the defeat of Napoleon in 1815 conservative forces were able to regain their hold on political power. Revolutionary ideas from France swept through the German lands again in the year 1830 and more significantly in 1848.

Not all the new political ideas were implemented, but the ones that were had a strong impact on economic institutions. When political rights imply economic rights, this follows naturally. For example, a constitution stating that everybody, including the king, is equal before the law also stipulates that there is equality before the law for disputes in commercial transactions. Other changes in economic institutions were related to the idea of freedom. For example, many German cities saw the abolishment of the requirement that skilled workers had to be members of the local crafts guild.

[5] This section draws on Acemoglu, Cantoni, Johnson, and Robinson (2011; ACJR).

There were a number of ways through which economic institutions were changed. First, the institutional changes were implemented as the direct consequence of French rule during the time of the French Revolutionary Wars. Between 1792 and 1795, for example, the seigneurial regime and the guilds were abolished in the Rhineland. The institutional changes did not end with the rise of Napoleon to power, however, because he continued to implement the reforms initiated before.

Second, even after Napoleon's defeat several German areas either kept or put in place French-inspired institutional reforms. This may be interpreted as a recognition of the need to modernize on the part of German elites. Given France's military victories, it was apparent that German military, administration, and ultimately her economy was inferior to the French. While some of the institutional change in the German cities was defensive in nature, ACJR show that the decisive push was externally triggered by French rule during Revolutionary and Napoleonic times.

Turning to some preliminary evidence on the relationship between institutional change and city population growth, Figure 7 contrasts population size for the group of cities that implemented institutional change relatively early with the group of cities that did so relatively late. We see that cities implementing institutional change early were larger in the 1820s. The typical city in either subsample grew by about one percent per year through the 1840s, before the size of cities among the early adopters starts to grow much more rapidly. This figure is consistent with a positive impact of institutional change on city growth. At the same time, this interpretation of Figure 7 takes the timing of institutional change as exogenously given, an assumption that we evaluate in section 5.

3 Data

The paper focuses on forty German cities during the period 1820 to 1880. The sample period starts in 1820 because the first years of the 19th century were tumultuous years of war and changing alliances that differ greatly from the following years.

The list of the forty cities is given in Table 1A. Economic growth is measured in terms of the cities' change in population size, a standard way of studying growth at the city level. Our sample is largely determined by the availability of wheat price data, as well as by the available information on the individual sample cities (statistical coverage was only fully harmonized after the German Reich had been founded in 1871). We use data every five years to reduce the effect of serial correlation in annual data. Although the sample is moderate in size, it covers the major German areas, and it has broad geographic coverage (see Figure 2), which is important for any analysis of trade in a historical context.[6] While trade here is international in the sense of between cities in independent polities, the ethnic and cultural proximity of the population suggests elements of domestic trade as well. We will return to this issue in section 6. As this is an unbalanced panel, sample composition might be of concern. Therefore, we emphasize results from a sample where each city has more than sixty percent of all possible observations (shown as the Base Sample in Table 1A). Cities from two polities, Bavaria and Mecklenburg, are strongly represented in the sample; as our analysis will show, this does not drive the results (see Table 5).

The sample covers many of the largest cities, such as Berlin, Hamburg, and München, but also relatively small towns such as Boizenburg. The distribution of city size in the sample is approximately log-normal, see Figure 8. Specifically, the inclusion of the smaller cities means that the left tail is covered relatively well; whereas focusing on population sizes of 5,000 and above, a common data restriction, would mean the left tail is largely absent. There is no comprehensive information on migration for these cities. Migration movements however often led to redistricting. Hence, we can gauge the importance of migration by employing an alternative population series that incorporates redistricting. The main sources of population data are the eKompendium German HGIS, Kunz (2013a), and Deutsches Städtebuch, Keyser (1939).

Our measure of trade is the absolute value of the percentage price difference of wheat (p) between two cities (j and k) in a given year (t) : or $P\_gap_{jkt} \equiv |\ln(p_{jt}) - \ln(p_{kt})|$. The Law of One Price is a central equilibrium condition in the theory of arbitrage, and the literature that has studied deviations from the Law of One Price is voluminous.[7] Grain was the main

[6] To reduce the influence of the regionally varying customs liberalizations, we exclude city-pairs between which customs barriers were already abolished by 1820; see Keller and Shiue (2013) on the customs liberalizations during this period.

[7] For example, Dybvig and Ross (1987), Froot, Kim, and Rogoff (1995), and O'Rourke and Williamson (1999). Engel and Rogers (1996) study the variability of price gaps.

foodstuff during this time when the majority of the workforce was still working in agriculture, and among all grains wheat was the most important one in this area. The local price of wheat was primarily affected by random weather fluctuations determining local harvests, and the integration of wheat markets across space.[8] Even though there were some regions that had a comparative advantage, including the Black Sea area, Eastern Prussia, and the United States of America, wheat was produced in the vicinity of most sample cities, with the identity of exporters and importers changing over time. While some producers were big, there was a sufficiently large number of small-scale suppliers producing a relatively homogeneous good that the assumption of a perfectly competitive market seems reasonable as an approximation.

Generally, both price and quantity measures have been extensively used to study trade (e.g., O' Rourke and Williamson 1999, Frankel and Romer 1999, respectively). Quantity information tends to be less available in historical settings such as ours. Since we do not have information on the quantities of wheat that were traded, a given price gap tells us only that the transactions costs cannot have been larger than the price gap (because that would be inconsistent with arbitrage), and not the precise level of transactions costs. At the same time, declining transport costs due to steam railways, improved contract enforcement, or increased trust between traders, among other things, are all plausibly reflected in lower price gaps. Moreover, because of its of central importance, wheat has been studied more than any other commodity in pre- and early-industrial Europe (e.g., Persson 1999, Jacks 2006, and Federico 2007). For lack of direct evidence, we have to assume that wheat is representative for inferring transaction costs for other goods. One advantage of wheat relative to most other goods is that it is rather homogeneous so that there is little reason to believe that the quality shipped varied substantially with distance (Alchian-Allen conjecture). In the empirical sections 4.3 and 4.4, we will examine the importance of not directly observing trade by separating out major coastal and inland waterway ports. Due to the low cost of ship transport, it is plausible to assume that trade took place in these cities more frequently than in other cities, and price differences involving these port cities would then be equal to, as opposed to the upper bound of, transactions costs.

The information on institutional change is developed along the lines of ACJR. For every city

[8] We abstract from storage, which we believe was not of first-order importance for the analysis. For treatments of trade-cum-storage, see Shiue (2002) and Steinwender (2014).

and year in the sample, we code 0/1 variables for (1) the abolition of guilds, (2) a guarantee of equality before the law, and (3) the possibility to redeem feudal lands. Any of these three conditions are indicative of good institutions in the sense that they are conducive to economic efficiency. Perhaps most controversial is the abolition of guilds. In late 18th century Germany, guilds tended to control entry to all major occupations and also at times restricted the adoption of new technologies (Ogilvie 2004). While guilds in Europe and other countries have not always been in the way of economic efficiency (Epstein 1998, Shiue and Keller 2007), on balance we hypothesize that the abolition of guilds is a sign of improvement of economic institutions. The empirical results below are consistent with this hypothesis.[9]

Note that these three indicators capture aspects of regulation for different economic sectors. Moreover, each of these variables is a proxy for regulations affecting a whole range of activities. For example, the regulation of crafts that abolished the requirement of guild membership (*Gewerbefreiheit*) often coincided with equivalent concessions permitting the setting up of manufacturing activities. Similarly, the year in which equality before the law was guaranteed through a written civil code was correlated with the presence of a written commercial code. In general, these indicators could be interpreted as a general sign of an efficiency-oriented economy, rather than as *the* institutions that matter for economic performance.

We define the institutions in a particular city k and year t, $Inst_{kt}$ as the average of these 0/1 indicators. We refer to this at times as the institutional quality of a city. Further, when the unit of analysis is the city pair we form the average of the institutional qualities of the two cities in that pair. As will become clear from the analysis below, the main results are not specific to particular ways of defining institutional quality.

Our measure of railways is based on the development of the railway network over time. We employ digital maps showing the location of train lines in every year of the 19th century (Kunz 2013b). The $Rail_{jkt}$ variable equals one if in year t there existed a direct railway connection between cities j and k, and zero otherwise.[10] It is parallel to the information on

[9] ACJR employ a second agricultural institutions indicator, the abolition of serfdom; we have considered this variable as well, finding that it does not add much to our analysis.

[10] Freight rates, on which we do not have comprehensive information, presumably increased with distance. Because our 0/1 railway variable does not capture that, we include bilateral distance directly in the regression.

price gaps in that it covers rich bilateral variation. Note that the bilateral rail connections are not independent–the rail connection between München and Bamberg went over Nurnberg, for example (see Figure 3)–and consequently, if railways did affect the strength of arbitrage this effect would not be identical for all city pairs. The importance of this effect varies with bilateral distance, which will be included in all specifications below.

Table 1A reports the earliest bilateral train connection for each city in the sample. For example, the first train connection in the sample was in the year 1841, for both Dresden and Leipzig, because in this year the direct line between these two cities was opened. Alternatively one could conceivably define a city-level variable that is equal to one when a city gets its first railway station, or a variable that counts the number of railway connections to other sample cities. This could be misleading in the German context, however, because numerous local railroad networks were started simultaneously but actual connections between them occurred only much later.[11]

The instrumental variable for institutional change is the length of French rule during the period 1793 to 1815. The fact that French rule was externally imposed is an attractive feature, from the point of view of this approach. In addition, the primary French motives were not economic but defensive (buffer zone versus Austria and Prussia) and ideological (exporting the ideas of the French revolution) in nature. Our variable is defined as effective French rule–that is, rule through France or through a French-controlled satellite state, excluding purely military occupations– in the area in which a particular city is located.[12] The length of French rule ranges from 0 to 19 years in our sample, with higher values mostly in Germany's west and northwest, see Table 1B.

We employ the costs of operating a particular railway as instrumental variable. A major determinant of whether a particular route would be built in the first place has to do with the feasibility and costs of operating a track. Railway costs are derived from Nicolls (1878) who presents information on how much freight capacity had to be given up when operating on steeper versus flatter terrain. According to this source, if a locomotive has 1,200 tons pulling capacity on flat land, this goes down to 1,150 tons with a gradient of 5 feet to the mile, down

[11] Also, we include city fixed effects which account for differences in when cities had their first railway.

[12] In addition to moving to a city-level analysis, we have extended ACJR's dataset, see Appendix A.

to 939 tons if the gradient is 10 feet to the mile, and so on (see Appendix A for details). Using this information to fit a smooth cost function, we apply the ArcGIS least-cost distance module in a 90 by 90 meter grid to compute the costs of the least-cost railway routes for all city pairs in the sample. Because these railway costs necessarily increase with distance between cities j and k, we divide by distance to arrive at the average gradient cost of terrain between j and k in terms of foregone railway freight capacity. Summary statistics for this data are given in Table 1B.

In addition to this data we employ a range of other variables in the analysis. They include information on coal deposits (Gutlerbet 2012), religion, secondary schooling (Keyser 1939), ship transport (Kunz 2014), geographic information on the cities longitude and latitude as well as their distance from Paris, and other variables. Summary statistics are shown in Table 1B, and definitions and sources are given in Table D.

4 The Impact of Institutions and Railways on Trade

In order to examine the impact of institutions and railways on trade, we use French rule and railway costs as instrumental variables. The terrain surrounding the location of cities is well-known to affect the costs of railways (Nicolls 1878), although this hypothesis has not been tested in this context. Institutional change in German cities during the 19th century is related to the length of French rule around the turn of the century, building on the work by ACJR. In this section, we present the identification strategy, followed by the reduced form relationship between trade, railway cost and French rule. We then turn to our estimates of the impact of railways and institutions on price gaps.

4.1 Identification Strategy

Our approach requires that the instrumental variables, railway costs and French rule, affect the endogenous variables, railways and institutions. This can be tested by the following two regressions:

$$Rail_{jkt} = \sum_{s=1}^{3} \delta_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \delta_{2s} I_{st} French\_rule_{jk} + \delta_{jk} + \delta_t + Q^{'}\beta + u_{jkt} \qquad (1)$$

and

$$Inst_{jkt} = \sum_{s=1}^{3} \nu_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \nu_{2s} I_{st} French\_rule_{jk} + \nu_{jk} + \nu_t + Q'\beta + e_{jkt} \qquad (2)$$

where $Rail_{jkt}$ is a dichotomous variable which is 1 if there was a direct train connection between cities j and k in year t; $Inst_{jkt}$ is an index of the average institutional quality in cities j and k in year t; R_cost_{jk} is the log cost of railway operation between cities j and k, per unit of distance; and $French\_rule_{jk}$ is the average years of French rule in cities j and k. Further, I_{st} is an indicator variable for each of three time-windows of roughly equal length (1820-35, 1840-1860, and 1865-1880), while δ_{jk}, ν_{jk} and δ_t, ν_t are city-pair and year fixed effects, respectively. The term $Q^{'}\beta$ is equal to $\sum_{s=1}^{3} \beta_s I_{st} Dist_{jk}$, with $Dist_{jk}$ defined as the bilateral distance between cities j and k; distance may be important for the impact of railways on price gaps, and it could also be that transport costs changed differentially for short versus longer distances.

We then use railway costs and French rule as instrumental variables in the following regression:

$$P\_gap_{jkt} = \beta_1 Rail_{jkt} + \beta_2 Inst_{jkt} + \beta_{jk} + \beta_t + Q^{'}\varphi + \varepsilon_{jkt}, \qquad (3)$$

where P_gap_{jkt}, our measure of trade, is the absolute value of the percentage difference in the prices of wheat of cities j and k in year t.

This instrumental variables approach requires the following two assumptions (Angrist and Pischke 2009, Ch. 4.4, 4.5). First, railway costs and French rule must be correlated with the establishment of railway connections and institutional change. Second, railway costs and French rule must be uncorrelated with other determinants of trade: $corr(R\_cost_{jk}\varepsilon_{jkt}) = 0$ and $corr(French\_rule_{jk}\varepsilon_{jkt}) = 0$. These exclusion restrictions will be satisfied if conditional on covariates, railway costs and French rule are as good as randomly assigned and if railway costs and French rule have no effect on the price gap through channels other than railways and institutional change.

The reduced form regression between price gaps and the instrumental variables is given by:

$$P\_gap_{jkt} = \sum_{s=1}^{3} \rho_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \rho_{2s} I_{st} French\_rule_{jk} + \rho_{jk} + \rho_t + Q^{'}\theta + \epsilon_{jkt}. \quad (4)$$

While the exclusion restrictions rely on correlations with the unobservable ε_{jkt} and are hence untestable, we shed light on the plausibility of the exclusion restrictions by augmenting the reduced-form regression (4) with other variables, Z:

$$P\_gap_{jkt} = \sum_{s=1}^{3} \rho_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \rho_{2s} I_{st} French\_rule_{jk} + \rho_{jk} + \rho_t + Q^{'}\theta + Z'\rho_z + \epsilon_{jkt}. \quad (5)$$

This sheds light on the exclusion restrictions because if the reduced-form coefficients ρ_{1s} and ρ_{2s} change drastically upon the inclusion of a particular Z, French rule and railway costs are not uncorrelated with ϵ_{jkt}, which signals the possibility that the instrumental variables operate not exclusively through railways and institutions.[13]

In section 5 of the paper, French rule and railway costs will serve as instrumental variables in our city growth analysis. Analogous requirements for that instrumental variables approach exist and will be discussed below.

4.2 The Reduced-Form Relationship between French Rule, Railway Costs, and Price Gaps

We begin with the reduced-form price gap regression, equation (4)

$$P\_gap_{jkt} = \sum_{s=1}^{3} \rho_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \rho_{2s} I_{st} French\_rule_{jk} + \rho_{jk} + \rho_t + Q^{'}\theta + \epsilon_{jkt}.$$

The coefficients on the time-invariant variables $French\_rule$ and R_cost can be estimated in addition to city-pair fixed effects because the instrumental variables have a time-varying effect through the inclusion of twenty-year window indicators, I_{st}. This is important especially for

[13]The reduced-form coefficients also shed light on β_1 and β_2, see Angrist and Pischke (2009).

railway costs: they should matter only once steam locomotives arrived in Germany, which was after the year 1835.

The OLS coefficients for railway cost are both positive, indicating that higher railway costs lead to higher price gaps (Table 2, column 1).[14] This is in line with expectations: high railway costs mean few railways, which kept price gaps relatively high. In contrast, French rule reduces price gaps, consistent with French rule leading to institutional improvements that benefited trade.[15] These results are supportive of the instrumental variables approach.

Robustness Checks We first consider the importance of sampling for our results. In the present case, the influence of changes in sample composition appear to be limited because we find the results with the sample of all 40 cities to be similar as those for the base sample (see column 2, versus column 1, respectively). Second, we see that the mere existence of French rule, a 0/1 indicator, is less strongly correlated with price gaps than the length of French rule in years; this provides support to the instrumental variables strategy (column 3). Third, we turn to specification issues. The dependent variable, P_gap is bounded from below by zero in contrast to OLS which assumes support from $-\infty$ to $+\infty$. OLS is also relatively prone to outliers. To see whether boundedness and outliers unduly bias our findings we apply other estimation methods. Using median regression gives broadly similar results to OLS (Table 2, column 4). Moving closer to the lower bound, column 5 shows that at the 25th percentile the impact of French rule and railway costs is qualitatively the same as according to OLS. These results suggest that the boundedness of P_gap does not greatly affect the OLS results. We also employ a robust regression routine that lowers the influence of outliers, finding less precisely estimated but broadly similar results (column 6).

Overall, the effects of railway costs and French rule on price gaps are as expected.

4.3 Instrument Validity: the Trade Exclusion Restrictions

The previous section documented the reduced-form relationship between railway costs, French rule, and price gaps. Here we present evidence on these relationships in the presence of other

[14] Compared to the omitted period of 1820-35, where ρ_1 is zero by construction.
[15] The omitted period is 1865-1880, when French rule had no longer a price-gap reducing effect.

channels that might be at work. For example, a difficult section of terrain such as a mountain range might present challenges not only to railways but also to other means of transport that could change price gaps. If the other potential correlates did not change much over time, such as foot traffic over the mountain range, then this will be captured by the city pair fixed effects. Some factors may have changed however, and here we address a number of them.

Our approach is to estimate the augmented reduced form regression (5) and to see whether the reduced form coefficients on French rule and railway costs are affected in the presence of alternative explanations, which are introduced one at a time. Results are shown in Table 3. Column 1 gives the reduced-form results of Table 2, column 6 again. Column 2 presents the first alternative, which is an indicator of whether the two cities were connected by railway according to List's 1833 railway plan. If the List plan anticipated early railway building, the inclusion of this variable may weaken the railway cost coefficients, and it may also detect whether price gap reductions are just a sign of a generally promising region. To allow for possibly time-varying effects we include the List plan variable interacted with a fixed effect for each decade;[16] these coefficients are shown in the lower part of Table 3. We see from the results that the List variable has no major effect on price gaps, and the reduced form railway and French rule coefficients are largely unchanged.

Second, railways were particularly important for coal-producing areas because they allowed for cost effective trade in coal. Further, coal producing areas experienced relatively high growth during the 19th century, and this growth may have reduced price gaps.[17] While coal was important in Germany's 19th century industrialization, there is little evidence to suggest that coal has a major effect on our identification strategy (column 3).

Third, geography and climate of an economy may be an important fundamental cause of economic growth (Diamond 1997, Sachs 2001), and we know that geography helps to explain the extent of trade (Tinbergen 1962, Nunn and Puga 2012). While climatic differences in the sample are relatively small, differences in elevation are more pronounced. Northern areas in Germany tend to be level whereas significant elevations are seen in the South. We look for the

[16] The exception to this are the final years of the sample period, 1865-80.

[17] Gutlerbet (2012) has emphasized the role of coal for regional development in 19th century Germany recently. The promise of future growth in German coal areas may also be one reason for French rule.

effects of geography in terms of latitude and longitude. Latitude tends to enter with a negative coefficient (column 4), perhaps reflecting the greater availability of cheap waterway transport in the North of Germany.[18] At the same time, conditional on covariates there is no evidence that latitude or longitude are highly correlated with price gaps *and* the proposed instrumental variables.

Fourth, there may also be cultural differences that explain why price gaps came down at different speeds. For example, Max Weber (1930) famously argued that the Protestant work ethic is conducive to economic efficiency. In line with Weber's thesis, we find that price gaps tend to be lower among predominantly Protestant cities, though this effect is more or less orthogonal to our instrumental variables (column 6). Becker and Woessmann (2009) have recently emphasized that Protestantism is highly correlated with human capital accumulation in 19th century Prussia. To examine these issues, we include an indicator for whether a city had formal secondary schooling (*Gymnasiums*) early on, which is seen as a general measure of the local emphasis on human capital. Our results are in line with Becker and Woessmann's findings in the sense that the patterns of coefficients for Protestantism and human capital are quite similar (columns 6 and 7). At the same time, the French rule and railway cost coefficients remain largely unchanged.[19]

Fifth, another consideration is that the probability of French rule may have been negatively correlated with the distance from Paris, not least because a short distance kept military and occupation costs relatively low. As we are interested in the impact of French rule on the institutions of German cities, and not the mere fact that a city was ruled by France, we present a specification with the distance from Paris on the right hand side. If the inclusion of distance from Paris wipes out the French rule coefficients we cannot identify the institutions effect on price gaps. However, the relative ease of occupation as captured by distance from Paris does not account for the entire French rule effect (column 8).

As the sixth factor, we consider the influence of water transport and international trade.

[18] We measure latitude in column 4 as the average latitude of the two cities, recognizing that when the two cities' latitudes are quite different the average will not be close to either city's latitude. We have explored this issue by redefining the variable, finding that it has only a limited effect in our analysis. A number of results are shown in Table A, columns (4A) and (5A).

[19] Similar results are obtained using the actual year of opening of a city's first *Gymnasium*.

Rail transport competed with waterway transport, and transport costs by water fell over time as well.[20] While adding an indicator of waterway location shows that cities with access to water transport experienced more rapidly declining price gaps, the railway cost coefficients do not change by much (column 9). Further, the nature of wheat trade in central Europe changed during the 19th century due to increasing imports from the United States of America. If important, it should affect coastal cities more than other sample areas, especially at times of low grain prices in the United States. Including an indicator for this into the reduced form we see quite similar patterns as before, making it unlikely that such effects greatly affect our instrumental variables strategy (column 10).[21]

The waterway indicators are also useful to assess the importance that in general the price gaps give only the upper bound as opposed to the exact level of bilateral transactions costs. Port cities had access to the low-cost mode of transportation during this time (Gutlerbet 2012, Table 2), making it likely that wheat was traded at these cities in virtually every period (even when a port city would not export its own wheat production or import for its own consumption, there could be entrepôt trade). This allows us to examine how important it is for our results that it is unobserved whether trade actually took place in a given year between two particular cities. The reduced-form results in Table 3 are not very different when we control for ship transport or not (column 1 versus columns 9 and 10), which suggests that the fact that price gaps are only equal to transactions costs whenever there is actual trade plays a limited role in our analysis. This result is in line with recent findings in Steinwender (2014).[22]

These results may be summarized as follows. While there are some changes in the reduced-form coefficients through the inclusion of other variables, the signs on the proposed instrumental variables never change, and the highest p-value of the test of joint significance for the instrumental variables is 0.8%, compared to 0.2% in the baseline estimation. Overall, the results of Table 3 provide support that the trade exclusion restrictions hold.

[20] See Gutlerbet (2012), Table 2.

[21] We have also considered communications improvements such as the telegraph, however, city-level information was not available to us.

[22] There, whether one focuses on periods in which there is trade or not, the evidence that the introduction of the telegraph lowered the New York-Liverpool price gap for cotton is equally strong in a statistical sense (Steinwender 2014, Table 2).

4.4 The Impact of Institutions and Railways on Trade

In this section, the first stage relationships between instrumental variables and the endogenous variables, institutions and railways, are analyzed. We also explore whether these relationships differ across various subgroups in the sample to see how local the relationships are.

First-stage Relationships We begin with the first-stage relationship between the instrumental variables and institutional change, see Table 4, column 1. A longer French rule leads to a higher institutional quality. This is consistent with French rule triggering institutional improvements, in line with ACJR's findings at the level of polities. The first-stage F-statistic is 4.30. Even though it is lower than the rule of thumb of 10 proposed by Staiger and Stock (1997), French rule is not a weak instrument. The F-statistic is robust and the city-pair level clustering allows for an arbitrary variance-covariance matrix capturing potential serial correlation in the residual error term (Wooldridge 2002). The individual coefficients for French rule are each significant at a 1% level, the p-value of the F-statistic is 0.2%, and the partial R^2 is 0.18.[23] In the following discussion, the focus will be on the railway first stage because railway costs R_cost_{jkt} is intrinsically a bilateral variable and as such it is particularly well suited to city-pair analysis. We will return to the institutions first stage in section 5.4 below.

We estimate negative coefficients on railway costs (significant in 1840-60), indicating that higher railway costs reduced the probability of a railway connection between two cities, relative to the placebo period of 1820-35 during which steam locomotives were not yet available in Germany (Table 4, column 2). The first-stage F-statistic is around 25 and the p-value is virtually 0. French rule tends to lower the chance that railways exist. As noted above, France was slower than Germany in building railway lines so this is consistent with the impact of French rule on institutions.

The instrumental variables approach will estimate the impact of railways on price gaps for those city-pairs that were induced by relatively low railway costs to establish railways. To see which types of cities were affected by railway costs, we estimate the railway first stage for

[23] See also Angrist and Krueger (2009, 215) on these issues, including an update on Staiger and Stock (1997).

various subsamples of our data. This also shows whether the instrumental variables estimates are very local–that is, whether railway costs matter only for a few observations–or not.

We first distinguish cities that are relatively far from each other from others that are more nearby. The coefficients on railway cost tend to be negative and quite similar (columns 3 and 4). Distinguishing cities that are relatively far from Paris from those that are closer to Paris shows that high railway costs deterred railway building in both subsamples (columns 5 and 6). Further, while railway connections were generally more prevalent in Protestant than in Catholic areas, high railway costs reduced railway building in both sets of cities (columns 7 and 8). There is also evidence that high railway costs deterred railway building for cities with access to water transport (column 9).[24] The reason for this may be that most navigable rivers in Germany flow South to North so that transport in the East-West dimension still requires overland transport, including by rail. Overall these results indicate that the first-stage relationship for railways holds in a number of subsamples, and our instrumental variable estimates are not identified from a small number of special observations.

We now estimate the effect of railways and institutions on price gaps:

$$P\_gap_{jkt} = \beta_1 Rail_{jkt} + \beta_2 Inst_{jkt} + \beta_{jk} + \beta_t + Q^{'}\varphi + \varepsilon_{jkt}.$$

The railway instrumental variables coefficient is about -0.1 and the institutions coefficient is about -0.4 (Table 5, column 1).[25] Both the introduction of railways and better institutions improved trade in the sense of bringing down price gaps. The instrumental variables coefficients are larger (in absolute value) than the OLS which are also shown. There could be a number of reasons for this, including measurement error in the railways and institutions variables, or that OLS suffers from omitted variables bias. Another potential reason is that instrumental variable estimation for railways measures the local average treatment effect on city pairs induced to have

[24] The subsample of waterway city pairs is relatively small, and no overly strong conclusions should be drawn.

[25] The Kleibergen-Paap (2006) F statistic here is 21.65, well above Stock and Yogo's (2005) critical value for 10% maximal bias and iid errors of 4.72 (LIML), providing more evidence on the power of the instruments. Results from overidentification tests are not reported because it can be expected that the second-stage results depend on which subset of instruments is employed, given that we have two endogenous variables. The first stage regressions of Table 5, column 1 are shown in Table 4, columns 1 and 2. Full detail on the first stages of Table 5 are not reported for space reasons; they are available upon request from the authors.

railways (whereas OLS measures the correlation across the full sample), or the instrumental variables violate the exclusion restrictions. These possibilities cannot be fully evaluated because they depend on characteristics that are not observed.

However, it is unlikely that violating the exclusion restrictions is the main reason why the instrumental variable estimates are larger (in absolute value) than the OLS because above we have seen little evidence that the instrumental variables are, conditional on covariates, correlated with other determinants of price gaps (Table 3). Additionally, the impact of the instrumental variables was not systematically different depending on the strength of the first stage (Table 4). In contrast, omitted variables bias is likely, given the many factors that could influence railway building and institutional change, and because both railway and institutions variables vary only between 0 and 1, measurement error is likely as well. Moreover, to the extent that the introduction of steam ships improved trade for waterway cities, the substitution between railways and steam ships would mean OLS is upwardly biased, consistent with our finding that the OLS estimates are larger than the instrumental variables estimates.

The institutions effect is quantitatively larger than that of railways, with standardized beta coefficients of -0.76 and -0.20, respectively. Some readers may find this result surprising at first. However, it has become increasingly clear that institutions affect trade in a range of markets (product, financial, and labor). Also, not only formal institutions defining the contracting and property rights environment matter but informal institutions sustained by repeated interaction, networks, or cultural beliefs do so as well.[26]

Robustness checks We first examine the influence of the city pair structure of the data on our inferences. A disturbance to the price in city k will affect all observations involving city k, and to address this issue we cluster by city (not city-pair); as seen from column 1A, our inferences are unchanged. Second, we explore the role of our railway cost variable. The bilateral railway cost proxy is derived from an U.S. railway building manual using a specific cost function (see Appendix A for details), for example. However, if instead we abstract from the specific values and map our railway cost measures into ten bins with values from 1 to 10,

[26] See Nunn and Trefler (2013). These authors examine the relationship between domestic institutions and comparative advantage, but similar arguments apply to trade that is not based on comparative advantage.

the results are similar (Table 5, column 2). This suggests that the specific values of our railway cost estimates play a limited role.

We then gauge the influence of changing sample composition by comparing the restricted with the full-sample results, finding no qualitative difference. The representativeness of our sample cities for Germany as a whole is explored in the city- and state-weighted regressions. The finding that both railways and institutional change reduce price gaps holds also when we reduce the number of Bavarian and Mecklenburg cities in the sample (see Table 5, columns 3 to 7).

We examine whether alternative definitions of institutions and French rule change the results. There is a smaller impact of institutions if redeemability of feudal lands is incorporated in the definition of institutions, although it still remains larger than the railways effect (for this and the following, see Table 6). Results are similar if we adopt a 0/1 measure of French rule, or code French rule in the Hanse cities (Hamburg, Bremen, and Luebeck) the same as everywhere else even though the nature of French rule there was different.[27] The 19th century is also a period of broad political change, and we explore the impact of wars, the 1848 revolution, and the formation of the Zollverein, finding that the influence of these events on the results is limited.

Two other issues are first, the definition of P_gap, our measure of trade, and second, potential spillover effects. To the extent that waterway observations are more informative on transactions costs than non-waterway observations because water transport was low-cost and hence there was more frequently trade, separating out waterway observations is a way to see whether more informative observations give substantially different results. We find that this is not the case (compare column 9 of Table 6 with column 1, Table 5).

Second, there could be spillovers from a rail connection between cities j and k for other cities, and these may either be positive or negative. This is a well-known challenge in research on place-based infrastructure projects that we cannot fully resolve in this paper. It is possible to shed some light on this issue in our historical setting, however, because early railways were

[27] The overriding goal of French rule in the Hanse cities was to enforce the continental blockade towards England, which by its very nature was detrimental to trade.

generally built on a state-by-state basis and centered on the state capital. This created a hub-and-spoke system, and a new rail connection between two state capitals often led to connections between smaller cities in different states as well. If there are substantial positive spillovers, then one would expect them to result from rail connections between state capitals. Excluding state capitals from the sample, however, leads to qualitatively similar results as before (Table 6, column 10). This suggests that spillovers of this type do not affect the main findings.

Exploring the robustness of our results further, we introduce the same factors that were considered earlier directly as additional controls in the instrumental variables estimation equation:

$$P\_gap_{jkt} = \beta_1 Rail_{jkt} + \beta_2 Inst_{jkt} + \beta_{jk} + \beta_t + Q^{'}\beta_x + Z'\beta_z + \varepsilon_{jkt} \tag{6}$$

For example, we ask whether adding an indicator variable for being on List's railway plan from the year 1833 changes the instrumental variables results. Results are shown in Table A in the Appendix; this table also addresses the issue that variables such as latitude have no natural city-pair level definition by presenting results for alternative definitions. Overall, the results shown in Table A confirm our results. The inclusion of additional variables does not weaken identification, the railways and institutions impact on price gaps remains quite similar to that in the baseline regression without additional variables, and our approach to define the city-pair average of variables such as latitude has no strong bearing on the results.

We now turn to the relationship between institutions, trade, and growth.

5 Institutions, Trade, and Growth

In this section, we are interested in the institutions' impact on growth through trade.[28] Using the result from the previous section that institutional change affects trade, we have

$$City\_Size_{kt} = \beta_1 Trade\left(Inst, X\right)_{kt} + \varepsilon_{kt}, \tag{7}$$

[28]For simplicity here we focus on trade, not also railways, and write $Trade(Inst, X)$ in the following. The railway impact should be picked up by trade because railway costs is one of our instruments.

and the question is how much of institutions effect on city growth goes through trade (β_1) versus not through trade (β_2):

$$City\_Size_{kt} = \beta_1 Trade\,(Inst, X)_{kt} + \beta_2 Inst_{kt} + \varepsilon_{kt}. \tag{8}$$

Since institutions may be endogenous (and trade surely is) we employ French rule and railway costs as instrumental variables. Note that institutions and city size vary at the level of the city while our measure of trade (the price gap between cities j and k) is a city-pair variable. To implement equations (7) and (8) we map city-pair analysis to a regression at the level of the city. We adopt the two-sample instrumental variables approach introduced by Angrist and Krueger (1992).[29] In the current context, this amounts to constructing city-level averages of city-pair variables. The approach is described in Appendix B.

In the following, we focus on the reduced-form relationship between city size and the proposed instrumental variables. We then estimate the impact of trade on city size (equation 7), before turning to the role of trade in the overall impact of institutions on growth (equation 8). The section concludes with a discussion of our findings.

5.1 The Reduced-Form Relationship between French Rule, Railway Costs, and City Size

In the reduced form we estimate the impact of French rule and railway cost on city population. Consider the following equation:

$$City\_Size_{kt} = \mathbf{\Gamma} + \sum_{s=1}^{3} \gamma_{s1} I_{st} French\_Rule_k + \sum_{s=1}^{3} \gamma_{s2} I_{st} (\overline{R\_cost_k}) + u_{kt}. \tag{9}$$

The dependent variable is the log of the population of city k in year t, $French\_Rule_k$ is the log length of French rule of city k in years plus one, and $\overline{R\_cost_k}$ is the average of the railway costs of city k to other cities j in the sample. The sample variation is in city and time dimension

[29] Earlier work on trade and growth using a two-sample approach includes Frankel and Romer (1999) who aggregate bilateral trade estimates from a gravity equation to the country level. More broadly, see also Angrist and Pischke (2009, 147-150) and Inoue and Solon (2010).

$(K \times T)$, as opposed to city pair and time dimension $(JK \times T)$. In the baseline sample there are 268 observations, down from 2,166 city pair observations. Equation (9) includes city- and year fixed effects, as well as the average bilateral distance between city k and its trade partners: $\Gamma \equiv \gamma_k + \gamma_t + \sum_{s=1}^{3} \gamma_{s3} I_{st}(\overline{dist_k})$. The latter is the city-level counterpart of bilateral distance between j and k.

If French rule caused institutional improvements that led to city growth, it should enter equation (9) with positive coefficients. Moreover, if high railway costs held back trade because they reduced railway building, we expect negative coefficients on railway costs after 1835 (once steam locomotives had become available). The estimation results for equation (9) are shown in Table 7, column 1. While not all coefficients are statistically significant, the results are in line with the proposed instrumental variables effects. The institutions result also confirms ACJR's analysis at the city level. Similar results are obtained for the full sample of forty cities (column 2). We also present results that downweigh influential observations. While this raises slightly the French rule effects and lowers those of railway cost, the results are broadly similar (column 3). Overall, these results support the instrumental variables strategy.

5.2 The City Size Exclusion Restrictions

Are the instrumental variables valid in the city size regression? In addition to being quasi-randomly assigned, the instrumental variables can neither directly impact city size nor be strongly correlated with a determinant of city size. We have seen above that railway costs do not have a large impact on institutional change (Table 4). The previous section has shown that French rule affects trade, so if trade impacts city size clearly there is room for French rule to affect city size through another channel, namely trade. Does this invalidate the instrumental variables strategy? No, because a better trade environment is in part caused by institutional change, which is the link that we are interested in. Identification requires independence conditional on the included covariates, and the question is whether French rule is correlated with trade or other determinants of city size that are not controlled for. To see how likely this is we estimate the city size reduced form equation (9) augmented with other plausible determinants of city size; our main interest is whether the reduced-form estimates on railway cost and French rule are substantially changed.

Results are shown in Table 8. Each of the columns 2 to 9 reports results from introducing, flexibly with decade fixed effects, one possible determinant of city size on the right hand side of equation (9). For example, in column 2 the added variable is coal production. While city size is higher in coal areas in the early sample years, the coefficients for the proposed instrumental variables do not change much compared to the baseline in column 1. This provides support for the identification strategy.

While the determinants of city size need not be the same as those of trade, many are plausibly the same and we analyze largely the same group as discussed earlier in Table 3. Geography in terms of latitude tends to amplify the coefficients on French rule and railway cost and does not pose a threat to identification. Cities further east tend to be smaller as the results for longitude show (column 4). The French rule coefficients shrink somewhat upon inclusion of longitude, which is not surprising given that French rule was less prevalent further east; importantly, the coefficients on French rule are positive even after controlling for longitude. Adding Protestantism yields results similar to those for latitude, probably because Protestants tended to live in the north while Catholics lived in the south of the German areas.

Looking at human capital as a determinant of city size, there is no evidence that relatively early establishment of secondary education affected city growth in our sample (column 6). Including distance to Paris would reduce the French rule coefficients if the French successfully targeted to rule areas with a high growth potential. We find, if anything, the opposite (column 7). Turning to the ease of trade, first we see that the location of a city on a waterway does not consistently affect city population (column 8). In contrast, cities that were relatively more likely to engage in international trade because they were on the coast tend to be relatively large (column 9). Differences in access to coastal trade and international markets obscure to some extent the detrimental impact of railway costs on city size, because controlling for coastal location amplifies the negative impact of high railway costs on city size.

Taken together, railway costs and institutions have reduced-form city size effects in line with expectations, a result that is robust to the inclusion of other potential determinants of city size. This supports the proposed instrumental variables strategy.

In the following section, we turn to the instrumental variables estimation.

5.3 The Trade Channel of Institutions' Effect on City Size

In this section we estimate the effect of trade on city size using the following specification:

$$City\_Size_{kt} = \Gamma + \beta_1 Trade\left(Inst, X\right)_{kt} + u_{kt}. \quad (10)$$

French rule and railway costs are employed as instrumental variables for trade. The first stage regression is as follows:

$$P\_gap_{jkt} = \sum_{s=1}^{3} \rho_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \rho_{2s} I_{st} French\_rule_{jk} + \rho_{jk} + \rho_t + Q^{'}\theta + \epsilon_{jkt}, \quad (11)$$

Applying the two-sample instrumental variables approach, we construct a city-level measure of average price gaps from the predicted values of equation (11), see Appendix B. This variable is in turn inserted into equation (10) to estimate β_1 :

$$City\_Size_{kt} = \Gamma + \beta_1 \overline{Trade(\widehat{Inst}, X)_{kt}} + u_{kt}, \quad (12)$$

where we write $\overline{Trade(\widehat{Inst}, X)_{kt}}$ in order to emphasize that this trade variable captures the impact of institutional change. We obtain an estimate of -4.75, significant at a 5% level (Table 9, column 1).[30] This provides evidence that institutions increase growth by reducing price gaps in our sample. We can use median regression instead of least squares in the first stage to address the boundedness of price gaps; it leads to a similar estimate (though the first stage is weaker; see column 2). The question we turn to in the next section is the extent to which institutions exert their impact on growth through trade.

[30] Following Bjorklund and Jantti (1997), for valid inferences we bootstrap over the two-sample instrumental variables procedure.

5.4 Institutions and Growth: Trade versus non-Trade Channels

To determine the importance of trade for the impact of institutions on growth, ideally we would like to put institutions and trade in the city size equation. If adding institutions turns the effect of trade to zero, then trade is not an important proximate factor for institutions. Trade plays a role only if it remains significant after institutions in included. We have just estimated the impact of trade on growth, see equation (12). One can recast our question by asking whether $Inst$ is an important omitted variable in this equation. Adding institutions to equation (12) yields

$$City\_Size_{kt} = \Gamma + \beta_1 \overline{Trade(\widehat{Inst}, X)_{kt}} + \beta_2 Inst_{kt} + u_{kt}. \tag{13}$$

Given that institutions may be endogenous, we use the length of French rule as an instrumental variable. Because French rule is used to predict the trade variable, an important question is whether the instrument has still enough power to estimate β_2 separately from β_1. To find out, we examine the first stage relationship between institutions and French rule conditional on trade.

The institutional quality of a city is increasing in the length of French rule (Table B, column 1), with a p-value of the F-test for inclusion that is virtually zero. In contrast, the trade variable is far from being significant at standard levels. This is evidence that the impact of French rule on institutions is in part orthogonal to trade. The results also indicate that trade does not affect institutions. The latter is not always the case; Acemoglu, Johnson, and Robinson (2005b), for example show that the rise of Atlantic trade did affect institutions in a number of European countries. We do not know at this point whether this is a true difference relative to their result, or driven by the specifics of our context, data, or specification.[31]

Using French rule as the instrumental variable, we turn to estimating the impact of institutions through trade and non-trade channels, equation (13). The institutions coefficient is positive–city size is increasing in institutional quality–though the coefficient is not precisely

[31] Also of interest to us is to learn about the characteristics of cities that were induced by French rule to improve their institutions (columns 2 to 12, Table B). We see that with the exception of cities far from Paris and in Germany's east, French rule is a significant determinant of institutions. Moreover, trade does usually not have an impact on institutions. Overall, these subsample results provide additional support that an institutions effect independent of trade can be estimated.

estimated and is insignificant at standard levels (column 3, Table 9). In contrast, trade has a coefficient of about -4.2, not very different from column 1 (although less precisely estimated). Alternatively, using median regression for the trade first stage yields similar conclusions (column 4).[32]

It is worth emphasizing that the results do not mean that institutions have no causal effect on growth. Institutions do have a positive impact on long-run growth. This can be seen perhaps most easily by dropping the trade variable and estimating the impact of institutions (instrumented with French rule) on growth; the coefficient is positive and significant at about 0.5 (Table 9, column 5). Rather, our results indicate that trade is important as a channel through which institutions affect growth in our context. As we have shown in section 4, institutional change led to changes in trade. It is useful to think about this in terms of timing, as this helps to establish the causal channels (see also Angrist and Pischke 2009, 68). The trade coefficient picks up an effect that is ultimately due to institutions.

Robustness Checks We first examine the role of the railway cost instrumental variable. Using a step function as opposed to the actual railroad cost estimates leads to similar results (column 6). One might also be concerned that our finding that institutions work through trade is obtained because our indicators of institutions are relatively close to trade. Equality before the law, e.g., might matter directly for trade disputes. To explore this issue, we replace equality before the law with redeemability of feudal lands, an indicator of the power of landowners versus their tenants. Now the trade coefficient is around -4.5, while institutions remains insignificant (column 7). Employing a broad definition of institutions based on all three indicators, the trade coefficient is similar in size (column 8). This suggests that our results are not sensitive to particular aspects of institutional quality.

We have also explored the role of sample composition for the results. First, consider different numbers of observations. In the full sample of 40 cities, the trade coefficient is about -3.1 while the institutions coefficient is slightly larger and close to significance (p-value of 0.139; column 1, Table 10). Full sample results for the broad institutions measure are similar (column 2).

[32] The complete first-stage results of the specifications in Table 9 are available upon request from the authors.

The results are not driven by the low-population states because we find similar results with state-population weighting (column 3). There is more evidence that city size matters: the trade coefficient changes to about -1.7 when the regression is weighted by city size, suggesting that the impact was relatively larger for the smaller cities in the sample.

Next, we turn to the creation of the Zollverein customs union. It might not only explain part of the gains from trade, but, as a tool of Prussia to rule other German areas, the Zollverein might be correlated with the growth effects of Germany's political unification. To see whether this might be important we include the geography-based predictor of Zollverein membership of Keller and Shiue (2013). As it turns out, the Zollverein predictor does not strongly affect the trade nor the institutions coefficient (Table 10, column 5). The same holds when we include controls for revolutionary activity around the year 1848 and the wars during the sample period (columns 6 and 7).

How strong is the evidence for spillover effects in the sense that improvements in one city's trade affected growth of another city? As before, we drop state capitals from the sample, finding that the trade coefficient is considerably smaller than before (column 8, Table 10, compared to column 3, Table 9). Apparently, a given improvement in trade affects growth in state capitals more strongly than in other cities. In contrast, the treatment status of the Hanse cities, in which French rule took a special form because it was mainly concerned with enforcing the continental blockade, does not affect the results by much (column 9).

We conclude this analysis by exploring measurement issues in the left hand side variable. A potential concern is that city size may not be fully comparable across cities and over time due to migration between cities. Migration is not fully observed. However, migration often leads to redistricting of city boundaries, and we have data on an alternative city size series that includes redistricting. According to column 10, employing the alternative city population series that includes redistricting leads to similar results as in the baseline (Table 9, column 3). While this analysis cannot be taken as definitive, it suggests that migration does not play a first-order role for our results.

To summarize, while the trade coefficient is not always precisely estimated, it is usually significant and there is a clear pattern: more trade in the sense of lower price gaps is conducive to city growth. There is less evidence for an independent institutions effect capturing the non-trade effects of institutions.

We now turn to a discussion of these findings.

5.5 Discussion

Economic Magnitudes How important is the institutions effect through trade, economically speaking? We turn to the instrumental variable estimates of equation (10) for a quantification. Because the estimation exploits within-city changes over time, we relate average annual observed city growth ($\Delta City\_Size_{kt}$) to average annual predicted city growth, $\overline{\hat{\beta}_1 \Delta Tr\widehat{ade(In}st)_{kt}}$, using the longest time difference available for each city (40 to 60 years), and $\hat{\beta}_1 = -4.75$ (from column 1, Table 9).

We find that predicting city growth with institutions and trade accounts for more than half of the variation of city growth, see Figure 9. We can also explain the level of city growth at the low end of the distribution, while the model tends to underestimate high rates of city growth. How about the economic importance of institutions through non-trade channels? Taking the point estimates of column 3, Table 9, and computing predicted city growth as $\overline{\hat{\beta}_1 \Delta Tr\widehat{ade(In}st)_{kt} + \hat{\beta}_2 \Delta Inst_{kt}}$, we can account for about 57 percent of the variation in city size growth, up from 54 percent. Based on these results, trade is an economically significant proximate factor in explaining city growth, and the fundamental behind trade is mostly institutional change, as we have seen in section 4. Moreover, trade appears to account for a large part of the total impact of institutions on growth.

Other Forms of Institutions It is possible that trade may be only important as a proximate cause for institutions' impact on growth for the specific set of institutions that we consider, but not for all institutions. Perhaps trade is the proximate factor through which the abolition of trade guilds affects growth, leaving open the possibility that other forms of institutions have growth effects that are orthogonal to trade.

It is worth keeping in mind that our indicators of institutional change cover a range of sectors, legal aspects of different specificity, and they emphasize the situation on the ground (not only *de jure* but whether rules are enforced). Moreover, our indicators are correlated with other elements of institutions (civil code went often hand in hand with commercial code, for example). From this perspective it is reasonable to expect that our indicators are broad correlates of institutional change. Delineating the precise set of institutions that works through trade is an important question, but one that would likely require different identification strategies, and we leave it to future work.

Proximate growth causes correlated with trade Another issue is whether we have unduly biased the analysis by focusing on trade as the proximate factor through which institutions operate. There may be other factors that are correlated with trade whose impact on growth we might attribute to trade. With numerous other possible growth factors, of which there is only a small subset with consistent city-level information for the early 19th century, there is no definitive answer to this question. The focus on trade amounts to a type of exclusion restriction that cannot directly be tested.

The evidence we can bring to bear on this question comes mainly from including other factors into the empirical analysis. Earlier we have shown that the reduced-form impact of French rule and railway cost on city size does not vanish when we control for the location of coal deposits, the propensity to international trade, or religious beliefs, among other things.[33]

We now extend this analysis by including the same factors Z directly into the instrumental variables estimation equation (10)

$$City\_Size_{kt} = \Gamma + \beta_1 Trade(Inst, X)_{kt} + \beta_2 Inst_{kt} + \delta' Z + u_{kt}. \tag{14}$$

These estimations are purely exploratory checks for remaining omitted variables bias. We do not have a particular reason to believe, for example, that latitude should affect city growth conditional on trade and institutions. Estimating equation (14) with the two-sample instrumental variable approach for a range of factors, we find virtually no factor having a significant effects

[33] It is true that due to data availability our measures of these factors do not vary over time, as the price gap measure does, though by introducing decade fixed effects we allow for time-varying effects, which should help make the analysis comparable.

on city size once trade and institutions are included (see Table C).[34] This provides additional evidence in support of trade being an important proximate factor through which institutions affect growth.

How does trade lead to growth? Much of what we know about the impact of institutions on trade concerns comparative advantage (see Nunn and Trefler 2013), which governs which products a country specializes in. In addition, trade can also affect the degree of competition and mark-ups (Levinsohn 1993, Melitz and Ottaviano 2008). One idea of how trade leads to dynamic effects is that it leads to the diffusion of technological knowledge, which affects the rate of innovation (Keller 2002). There are studies relating the extent of so-called R&D spillovers from international trade to domestic institutions (Acharya and Keller 2008, Coe, Helpman, and Hoffmaister 2009). Specifically it has been argued that the introduction of steam railways, which is taken as given, has affected regional patenting in 19th century Germany (Yin 2005, 56-57).

Along these lines we ask whether there is a relation between trade and innovation in our context. Is patenting, a measure of innovation, related to our predicted city growth, $\hat{\beta}_1 \Delta \overline{\widehat{Trade(Inst)}}_{kt}$, capturing the impact of institutional change and railways on growth through trade? We do not have information on city-level patenting, however data on patenting at the regional level is available starting in the late 19th century (Baten, Streb, and Yin 2006). Using this data, we find that cities with greater institutional change not only had improvements in trade during the 19th century but also exhibited systematically higher levels of per-capita patenting in the following decades (Figure 10). Moreover, the correlation is, with almost fifty percent, quite strong. This is consistent with the hypothesis that one mechanism through which trade led to growth in our context was higher rates of innovation.

[34] Only the presence of coal deposits is a significant predictor of city size (+), and this does not affect our findings on $Trade$ and $Inst$. The trade coefficient is between around -2 to -5.5 and generally significant while the institutions coefficient tends to be positive but not statistically significant, confirming our earlier findings.

6 Concluding Remarks

In the setting of 19th century central Europe, we have examined the link between institutional change as a fundamental cause of growth, and the more proximate factors, trade and railways. Our results confirm that institutions have a strong impact on long-run growth. What is new is that we find trade to be important as a proximate factor in this. In this section we discuss a number of open questions.

First, to what extent are our results driven by the setting of 19th century Germany? In particular, should we always expect trade to be conducive to growth? Some of our estimates point to heterogeneous effects, although it is difficult to tell whether they could be strong enough to lead to a core-periphery pattern (requiring positive growth in some, negative growth in other cities). Also, in our context we did not see much evidence that trade affects institutions, but we know that trade can lead not only to inclusive changes but also to exclusive changes that lower economic performance (Acemoglu, Johnson, and Robinson 2005a, Nunn 2008). It is important to learn more about the generality of the findings.

We have analyzed trade that, while not quite domestic, is less international than in much of the literature. As a general matter, domestic trade may be more important than international trade for gravity reasons: it happens more frequently. Productive coexistence almost surely requires the division of labor on a limited geographic space, and therefore domestic trade may be a crucial aspect of that. Trade not only depends on but may also generate another feedback to institutions through increased trust, networks, the absence of war, and greater cultural identity. To the extent that this is good for growth, it will be picked up as a strengthening of integration, explaining in part why we find trade to be so important as a proximate factor for institutions. At the same time, there are also strong commonalities between domestic and international trade: for example, both are subject to gravity and so-called border effects (Hillberry and Hummels 2003, Shiue 2005), and the difference between domestic and international trade may well be more gradual than the domestic versus international distinction suggests. A treatment of institutions and culture when trade is possible over various distances, broadly defined, can be found in Tabellini (2008), and further work along these lines would be useful.

Taking our result that institutions affect growth via trade as given, there is still much to learn about the mechanisms. Was it the more credible enforcement of private contracts? A greater tendency to establish interregional agreements? That private business interests were given more room, including in the operation of railways? While we have arguably peeled off one layer in the understanding of growth, more detailed analysis is needed combined with a suitable framework for quantification to make further progress. Our analysis was limited to one fundamental, institutions, and one proximate factor, trade. While we have considered alternatives, a full-blown study of multiple proximate growth factors would be interesting, even though estimating causal effects would be without doubt challenging. This paper has tried to suggest a way to include additional steps and testable implications that we hope will prove useful in future studies of growth.

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A Details on Data Sources

City population We employ two measures of city population size. The first is based on data in the German Historical GIS Kompendium (Kunz 2013a), which gives the population at the *Regierungsbezirk* level (roughly, county level) for all sample areas. For example, in the case of Prussian cities of Madgeburg and Aachen, our estimates are based on the official population figures for the *Regierungsbezirke* of Madgeburg and Aachen that were collected every three years (in 1822, 1825, etc.). Because in each county the main city is the major influence of population changes, population changes derived from these figures give an accurate estimate of city population changes.

These population trends are combined with information on population levels for the benchmark years of 1800 and 1850, which come from Bairoch, Batou, and Chevre (1988), de Vries (1984), and Mitchell (1980) for the larger cities, and from the local population histories and other sources for the smaller cities. The city population data for non-benchmark years for some of the smallest towns in the sample are our own estimates. Also the city population figures for the year 1800 come from these sources, while the population of each state in the year 1816 is taken from Viebahn (1858).

An alternative source of population data is the Städtebuch data from Keyser (1939), various volumes (employed in specification (10) of Table 10). Its primary advantage is it provides information on the population of cities, as opposed to the counties in which the cities are located, while its disadvantage is that population figures are given less frequently so more interpolation is necessary. The Städtebuch figures also differ in that they generally include any increase in city size due to redistricting. The two city population series are quite similar, with a correlation above 0.99, and we are confident that both series capture the main population developments in the sample well.

Wheat Prices The annual price of wheat in each city is computed from government records of market prices that were typically recorded every month, and in some cases every week. The information on cities in Bavaria and Mecklenburg are taken from Shiue and Keller (2007), while Seuffert (1857) also covers cities in Baden, Brunswick, Hesse-Darmstadt, Hesse-Cassel, Hesse-

Nassau, Saxony, and Wurttemberg. The wheat prices for Prussian markets were provided by Michael Kopsidis, see Kopsidis (2002). In a temporal dimension, the extent of wheat price information varies by city. We have expanded the coverage of the wheat price data using the additional sources, in particular Fremdling and Hohorst (1979), Gerhard and Kaufhold (1990), and Vierteljahrshefte (1935). While our sample covers virtually all German areas, Bavaria and Mecklenburg are particularly well represented (see Figure 2). In Table 5 we show that reducing the influence from cities of these polities does not critically influence our results. General characteristics of the grain prices during this period are discussed in Shiue and Keller (2007).

The price gap sample consists of all city pairs for which we have wheat price information for both cities. Since neither quantity nor monetary units were standardized in the German states during the 19th century, conversion rates are required for our analysis of absolute price differences, and all prices are converted into Bavarian *Gulden* per Bavarian *Schäffel*. The conversion factors are taken from the original sources (see Shiue and Keller 2007 for references) as well as from Seuffert (1857). Specifically, from the latter we have (page 351):

State	Quantity unit	Conversion factor into Bav. *Schäffel*	Monetary unit	Conversion factor into Bav. *Gulden*
Baden	Malter	0.67	Gulden	1.00
Brunswick	Himten	0.14	Thaler	1.75
Frankfurt	Malter	0.51	Gulden	1.00
Hamburg	Fass	0.24	Mark Banco	0.88
Hanover	Himten	0.14	Thaler	1.75
Hesse-Darmstadt	Malter	0.57	Gulden	1.00
Hesse-Cassel	Schaeffel	0.36	Gulden	1.00
Hesse-Nassau	Malter	0.49	Gulden	1.00
Prussia	Schaeffel	0.24	Thaler	1.75
Saxony	Schaeffel	0.46	Thaler	1.75
Wurttemberg	Schaeffel	0.80	Gulden	1.00

Railway data

Direct railway connections The main source of information on the timing of railway connections, as described in section 2.1, are the digital historical maps provided at Institut fuer Europaeische Geschichte (IEG) in Mainz, see Kunz (2013b). At times a train connection existed but it was highly circuitous. At other times, a train connection from a city in the south to another in the north would first go past the northern city's latitude before turning back south (and analogously for east-west railway lines). In these cases there is considerable doubt that the route was indeed the least-cost route, and we assume in these cases that train connections in fact did not yet exist. Because of the hub-and-spoke structure of railway lines centered on the respective state capital virtually all of these determinations are unambiguous; our results are robust to plausible changes in the coding.

Railway cost Our measure is based on how the capacity of a 19th century steam locomotive to haul freight changes as a function of the gradient of the terrain, from Nicolls (1878). Specifically, Nicolls provides the following information on page 82:

Gradient (feet to the mile)	5	10	20	30	40	50	60	70	80	90
Hauling capacity (tons)	1,150	939	686	536	437	367	315	275	242	216

Gradient (feet to the mile)	100	110	120	130	140	150	160	170	180
Hauling capacity (tons)	194	175	159	146	134	123	113	105	98

Five feet to the mile is a gradient of about 0.095%, and 180 feet to the mile is a gradient of about 3.4%. The data is for a "good" locomotive weighing 27 tons, going at a speed of 8 to 12 miles per hour, uphill. We do not know of comparable data for going downhill, and it is assumed that the freight capacity of locomotives varied for downhill trips (due to strains on the brakes, etc.) in the same way as it did for uphill trips.

To convert this into a cost measure, we assume that on flat terrain the locomotive can haul 1,200 tons at the speed of 8 to 12 miles per hour. Then, the cost in terms of foregone freight hauling capacity of a gradient of five feet to the mile is 50 tons $(1,200 - 1,150)$, the cost of a gradient of ten feet to the mile is 261 tons, and so on. We fit a logarithmic function through this data to be able to work with any terrain gradient; this yields an R-squared of 0.98. With this cost function in hand we use a 90 meter x 90 meter GIS map of the relevant area in central Europe and the ArcGIS least-cost distance solver to compute the least-cost routes, as well as the associated costs of those routes, from each city to all other cities in the sample. Lakes are blocked out in this calculation, but not rivers. Because these railway costs are positively related to the bilateral distance between cities j and k, we divide by the bilateral geographic distance to arrive at R_cost_{jk}, the gradient cost of terrain between j and k in terms of foregone railway freight capacity, per unit of direct distance. All geographic distances in this paper are computed using the Haversine formula. Summary statistics for this railway cost data is given in Table 1B.

Institutional Change and French Rule The data on institutional change in the forty German cities is based on Acemoglu, Cantoni, Johnson, and Robinson (ACJR; 2011). To the extent that our analysis covers areas that are not included in their sample, we use the sources given in ACJR, especially Dipper (1980). We define institutions at the city level as the institutions that prevailed in the geographic area of the city. Note that while for the most part the measures of institutional change–abolition of guilds and equality before the law–vary at the state level, for some of the larger states such as Prussia and Bavaria there is also variation at the level of the city.

There are some differences in the way we employ this data. First, we employ three indicators of institutions: (1) abolition of guilds, (2) equality before the law, and (3) the redeemability of feudal lands, while ACJR employ a fourth, the *de jure* abolition of serfdom. This indicator does not play a big role here. Further, in our analysis we compute the average of the 0/1 indicators that indicate the presence or absence of this aspect of the institutions, which varies over time to the extent that the indicators vary over time. In ACJR the authors define their institutional indicator as the number of years that the institutional indicators were in place by a given year

t. As a consequence, we capture primarily the contemporaneous effect while ACJR model a more cumulative effect.

Our data on the length of French rule follows ACJR closely. The criterion for this variable is effective French rule, which is more than purely military occupation. A special case in this respect are the former Hanse cities: Hamburg, Bremen, and Lübeck. In the main results, we code these cities as not French-ruled, even though they actually were French *departements* from 1811 to 1814. We do so because first, French rule was most importantly designed to enforce the continental blockade versus England, which was not only by its nature detrimental to trade but also just different from French rule in other parts of the sample. Second, French rule in the Hanse cities was more tenuous than in other areas; during part of the year 1812, for example, Hamburg was ruled by the Russians. We also show in Table 10 that the main results do not depend on our treatment of the Hanse cities (column (9)).

See Table D for the definitions and sources of other data employed in this paper.

B Two-sample Instrumental variables estimation

In this section we describe the two-sample instrumental variables approach. In the present case, this means constructing a city-level average price gap variable from the predicted bilateral price gaps of the first stage, and employing this variable together with the institutions variable in the city-level population regression.

Let the first stage regression be given by

$$P\_gap_{jkt} = \sum_{s=1}^{3} \rho_{1s} I_{st} R\_cost_{jk} + \sum_{s=1}^{3} \rho_{2s} I_{st} French\_rule_{jk} + \rho_{jk} + \rho_t + Q^{'}\theta + \epsilon_{jkt}, \qquad (15)$$

where $Trade_{jkt}$, $French\_rule_{jk}$, and R_cost_{jk} are, as before, defined as the absolute value of the percentage price gap between city j and city k in year t, the log average length of French rule in cities j and k plus 1, and the bilateral railway cost between j and k, respectively. The term Q includes the bilateral distance terms between j and k interacted with time window indicators. Using OLS we obtain the predicted value of equation (15), $Trade\widehat{(Inst, X)}_{jkt}$. Then, the average predicted price gap for each city k and year t is formed:

$$\overline{Trade(\widehat{Inst}, X)_{kt}} = \frac{1}{N_{kt}} \sum_{j \neq k}^{N_{kt}} Trade(\widehat{Inst}, X)_{jkt} \ , \forall k, t. \tag{16}$$

This variable $\overline{Trade(\widehat{Inst}, X)_{kt}}$ is at the level of the city and hence compatible with the city-level analysis. The estimation equation is given by

$$City\_Size_{kt} = \Gamma + \beta_1 \overline{Trade(\widehat{Inst}, X)_{kt}} + u_{kt}, \tag{17}$$

with estimation results shown in Table 9, column 1.

When we include institutions as a separate variable to this equation, we obtain

$$City\_Size_{kt} = \Gamma + \beta_1 \overline{Trade(\widehat{Inst}, X)_{kt}} + \beta_2 Inst_{kt} + u_{kt}. \tag{18}$$

The potentially endogenous institutions variable $Inst_{kt}$ is instrumented by the length of French rule in city k, interacted with the time period indicators ($\sum_{s=1}^{3} \delta_{s1} I_{st} French\_rule_k$). Equation (18) is a standard IV estimation problem with one endogenous variable ($Inst$), where the trade variable $\overline{Trade(\widehat{Inst}, X)_{kt}}$ is taken as exogenously given. To account for the fact that $\overline{Trade(\widehat{Inst}, X)_{kt}}$ is estimated in a previous stage, we follow Bjorklund and Jantti (1997) and perform inferences based on bootstrapping over the two-sample instrumental variables procedure. The number of bootstrap replications is set at 200. We cluster the errors at the level of the city. Results of these estimations are shown in Tables 9, 10, and C.

Table 1A: Sample cities

City	State	Base Sample	First Rail Connection
Aachen	Prussia	Y	1859
Augsburg	Bavaria	Y	1851
Karlsruhe	Baden		1852
Bamberg	Bavaria	Y	1851
Bayreuth	Bavaria	Y	1853
Berlin	Prussia	Y	1841
Boizenburg	Mecklenburg	Y	1846
Braunschweig	Brunswick		1844
Bremen	Free City		1847
Dresden	Saxony		1841
Erding	Bavaria	Y	1872
Frankfurt	Free City		1852
Goettingen	Hannover	Y	1854
Grabow	Mecklenburg	Y	1846
Hamburg	Free City	Y	1846
Hannover	Hannover		1844
Kassel	Hesse-Cassel		1849
Kempten	Bavaria	Y	1852
Cologne	Prussia	Y	1847
Landshut	Bavaria	Y	1854
Leipzig	Saxony	Y	1841
Lindau	Bavaria	Y	1853
Luebeck	Free City		1851
Mainz	Hesse-Darmstadt		1856
Memmingen	Bavaria	Y	1862
Munich	Bavaria	Y	1851
Muenster	Prussia	Y	1848
Noerdlingen	Bavaria	Y	1851
Nurnberg	Bavaria	Y	1851
Parchim	Mecklenburg	Y	1880
Regensburg	Bavaria	Y	1859
Rostock	Mecklenburg	Y	1850
Schwerin	Mecklenburg	Y	1847
Straubing	Bavaria	Y	1859
Stuttgart	Wuerttemberg		1853
Ulm	Wuerttemberg		1853
Wismar	Mecklenburg	Y	1848
Wuerzburg	Bavaria	Y	1854
Zweibruecken	Bavaria	Y	1857
Zwickau	Saxony		1845

Table 1B: Descriptive statistics

City-Pair Variables	Mean	Stand Dev.	Max	Min	# Obs
Railway Cost	843,102	278,495	3,659,949	173,033	3570
Bilateral Distance (km)	379.9	152.5	746.8	31.8	3570
List's Railway Plan	0.169	0.375	1	0	3570
Zollverein Membership	0.756	0.260	1.935	0	3570
Price Gap	0.153	0.117	0.821	0	3570

Notes: Railway Cost is the average cost of terrain, on a per kilometer basis, in terms of foregone tons of railway freight. Bilateral Distance is the direct distance between a city-pair. List's Railway Plan is an indicator variable equal to one if there was a railway connection between the city-pair on economist Friedrich List's 1833 plan. Zollverein Membership is the log of average distance of the city-pair to coast relative to average distance to coast of city-pairs that are not Zollverein members yet in that year, plus 1; see Keller and Shiue (2013). Price Gap is the absolute value of the percentage price difference of wheat between a city-pair.

City Variables	Mean	Stand Dev.	Max	Min	# Obs
Population (thousands)	58.092	123.49	1122.3	2.8	312
Institutions -- Base	0.202	0.337	1	0	312
-- Feudal Lands	0.486	0.366	1	0	312
-- Broad	0.364	0.308	1	0	312
Years of French Rule	2.471	5.664	19	0	312
French Rule Y/N	0.231	0.422	1	0	312
Latitude	50.89	2.10	54.07	47.53	312
Longitude	10.60	1.76	13.72	6.09	312
Distance to Paris (miles)	415.9	90.0	552.7	212.4	312
Protestant Share	0.642	0.385	0.993	0.004	312
Year of First Gymnasium	1632.01	144.085	1964	1450	312
Coastal	0.266	0.443	1	0	312
Waterway	0.426	0.495	1	0	312
Coal Producer	0.231	0.422	1	0	312
1848 Revolution	0.010	0.098	1	0	312
War	0.074	0.262	1	0	312

Notes: Institutions is defined as the mean of 0/1 indicators that were present in the city in a particular year: (1) guilds were abolished, (2) equality before the law was guaranteed, and (3) it was possible to redeem feudal lands. "Base" is (1) and (2), "Feudal Lands" is (1) and (3), and "Broad" is (1), (2), and (3). Years of French Rule is the number of years the city was under French rule from 1793 to 1815. French Rule Y/N is an indicator variable equal to one if the city was ever under French Rule. Distance to Paris is the direct distance from the city to Paris, a measure of the probability of French occupation. Protestant Share is the time varying share of protestants in the city. Gymnasium is a school that prepares for study at universities. Coastal is an indicator variable equal to 1 if the city is in the first quartile of distance to the coast. Waterway is an indicator variable equal to one if the city is on a river, coast or canal in 1850 according to Kunz (2014). Coal Producer is an indicator variable equal to one if the city had coal production in 1850. 1848 Revolution is an indicator equal to one if the city was experiencing 1848 revolutionary activities and War is an indicator equal to one if the city was experiencing a war.

Table 2: Price gap reduced form

	(1)	(2)	(3)	(4)	(5)	(6)
			French Rule	Quantile	Quantile	Robust
	Base	Full Sample	Y/N	Median	25th Perc.	Regression
[1840-1860] x	0.043**	0.032**	0.045**	0.037**	0.031**	0.042**
Railway Cost	(0.013)	(0.012)	(0.014)	(0.013)	(0.012)	(0.013)
[1865-1880] x	0.045**	0.036**	0.059**	0.061*	0.027	0.047
Railway Cost	(0.013)	(0.013)	(0.014)	(0.034)	(0.026)	(0.037)
[1820-1835] x	-0.019**	-0.023**	-0.012	-0.018*	-0.000	-0.013
French Rule	(0.006)	(0.006)	(0.013)	(0.010)	(0.010)	(0.013)
[1840-1860] x	-0.021**	-0.019**	-0.024+	-0.024*	-0.012*	-0.022+
French Rule	(0.006)	(0.005)	(0.013)	(0.011)	(0.008)	(0.013)
[1820-1835] x	0.025*	0.019*	0.014	0.026	0.024	0.011
Bilateral Distance	(0.010)	(0.010)	(0.011)	(0.030)	(0.030)	(0.027)
[1840-1860] x	0.046**	0.050**	0.032**	0.053*	0.036	0.051+
Bilateral Distance	(0.009)	(0.009)	(0.010)	(0.023)	(0.026)	(0.027)
Observations	2,166	3,570	2,166	2,166	2,166	2,166
Number of city pairs	252	642	252	252	252	252

Notes: Dependent variable is absolute value of percentage price gap between j and k; estimation method: least squares (1) to (3), quantile regression (4) and (5), and robust regression (6) using STATA's rreg routine. ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. Standard errors shown in parentheses. Robust standard errors clustered at the city-pair level (1) to (3); robust standard errors with bootstrapped confidence intervals not imposing normality (4) to (5). All regressions include year - and city-pair fixed effects. Railway Cost as defined in the text. French Rule is log(mean years french rule in the city pair +1). Bilateral distance is the direct distance between the two cities. French Rule Y/N is an indicator variable equal to 1 if either city was ever under French rule.

Table 3: Railway costs, French rule, and price gaps. The trade exclusion restrictions

		Important Regions		Geography		Culture	Human Capital	Easily Occupied	Likelihood of Trade	
	(1) Base	(2) List RR Plan	(3) Coal	(4) Latitude	(5) Longitude	(6) Protestant	(7) Schooling	(8) Dist Paris	(9) Waterway	(10) Coastal
[1840-1860] x	0.042**	0.042**	0.046**	0.038**	0.043**	0.042**	0.039**	0.045**	0.043**	0.039**
Railway Cost	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
[1865-1880] x	0.047	0.046	0.055	0.033	0.047	0.039	0.030	0.044	0.049	0.045
Railway Cost	(0.037)	(0.038)	(0.038)	(0.038)	(0.038)	(0.039)	(0.039)	(0.038)	(0.037)	(0.037)
[1820-1835] x	-0.013	-0.014	-0.015	-0.013	-0.022	-0.014	-0.012	-0.025	-0.008	-0.012
French Rule	(0.013)	(0.013)	(0.013)	(0.013)	(0.018)	(0.014)	(0.013)	(0.018)	(0.013)	(0.013)
[1840-1860] x	-0.022+	-0.023+	-0.021	-0.023+	-0.019	-0.029*	-0.021+	-0.025	-0.017	-0.023+
French Rule	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.014)	(0.013)	(0.016)	(0.013)	(0.013)
[1820-1835] x	0.011	0.012	0.012	-0.008	0.013	0.009	0.011	0.010	-0.000	0.010
Bilateral Distance	(0.027)	(0.027)	(0.027)	(0.029)	(0.027)	(0.028)	(0.027)	(0.027)	(0.027)	(0.028)
[1840-1860] x	0.051+	0.052+	0.048+	0.033	0.051+	0.043	0.049+	0.052+	0.043	0.044+
Bilateral Distance	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)	(0.026)
y20s_X		0.025	0.017	-0.013	-0.012	-0.002	-0.023	-0.024	-0.053+	0.058*
		(0.026)	(0.044)	(0.011)	(0.011)	(0.051)	(0.021)	(0.024)	(0.030)	(0.028)
y30s_X		-0.034	0.067	-0.022*	-0.005	-0.037	-0.025	-0.022	-0.053+	-0.110**
		(0.025)	(0.043)	(0.011)	(0.011)	(0.051)	(0.021)	(0.024)	(0.030)	(0.035)
y40s_X		0.003	-0.006	-0.021+	0.004	-0.046	-0.053**	-0.004	-0.059*	-0.065*
		(0.024)	(0.042)	(0.011)	(0.009)	(0.049)	(0.020)	(0.020)	(0.029)	(0.032)
y50s_X		-0.013	0.040	-0.016	0.004	-0.079	-0.019	-0.008	-0.012	
		(0.024)	(0.042)	(0.011)	(0.009)	(0.049)	(0.020)	(0.020)	(0.029)	
P-val: Jointly Sig Instruments	[.002]	[.002]	[.002]	[.005]	[.008]	[.000]	[.006]	[.004]	[.003]	[.002]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.304	0.309	0.309	0.312	0.305	0.308	0.312	0.305	0.310	0.320

Notes: Dependent variable is absolute value of percentage price gap between j and k; estimation by robust regression; standard errors in parentheses. ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. All regressions include year- and city-pair fixed effects. Railway Cost as defined in the text. French Rule is log(mean years french rule in the city pair +1). Bilateral distance is the bilateral distance between the two cities. List RR Plan is whether the city pair was on List's Railways Map (see Figure 4). Coal is the city-pair average of a dummy for whether each city had coal production in 1850. Latitude and Longitude are the mean latitude and longitude of the city pair. Protestant is the mean protestant share in the city pair. Schooling is whether both cities had a Gymnasium by the year 1580. Distance to Paris is the mean distance to Paris of the city pair, yxxs_X are scaled by 100. Waterway takes the city-pair average for a dummy of whether the city lies on a river, coast or canal in 1850 according to Kunz (2014). Coastal is an indicator equal to 1 if both cities are within the first quartile of distance to the nearest coast and the US price of wheat is below the median. The additional x's (col 2-10) are interacted with decade, 1820s, 30s, 40s, and 50s to allow time-varying impact.

Table 4: Institutional change and railway connections. First stage results

	Institutions	Railways								
	All	All	Bilateral Distance		Distance from Paris		Protestant		Waterway	
			High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
[1840-1860] x	0.005**	-0.126**	-0.093**	-0.146*	-0.152**	-0.058	-0.147*	-0.109**	-0.290*	-0.100**
Railway Cost	(0.002)	(0.031)	(0.025)	(0.063)	(0.037)	(0.044)	(0.068)	(0.036)	(0.133)	(0.032)
[1865-1880] x	0.017	-0.105	-0.117	-0.020	-0.095	-0.232**	-0.044	-0.210*	-0.101	-0.082
Railway Cost	(0.065)	(0.081)	(0.083)	(0.192)	(0.113)	(0.088)	(0.274)	(0.085)	(0.109)	(0.103)
[1820-1835] x	0.078**	-0.020	-0.039	0.011	-0.072	0.062	-0.195	-0.002	-0.015	-0.016
French Rule	(0.023)	(0.037)	(0.067)	(0.054)	(0.184)	(0.063)	(0.159)	(0.045)	(0.017)	(0.056)
[1840-1860] x	0.079**	-0.088*	-0.093	-0.066	-0.318+	-0.012	-0.286+	-0.054	-0.069+	-0.080
French Rule	(0.023)	(0.038)	(0.067)	(0.054)	(0.177)	(0.066)	(0.158)	(0.044)	(0.040)	(0.055)
[1820-1835] x	0.013	0.000	0.143	-0.017	0.009	0.182	0.016	0.321	0.058+	-0.084
Bilateral Distance	(0.041)	(0.080)	(0.471)	(0.097)	(0.089)	(0.235)	(0.095)	(0.213)	(0.030)	(0.127)
[1840-1860] x	0.018	-0.125+	0.154	-0.135+	-0.158*	0.168	-0.102	0.178	-0.124**	-0.168+
Bilateral Distance	(0.041)	(0.065)	(0.456)	(0.076)	(0.073)	(0.244)	(0.082)	(0.223)	(0.045)	(0.100)
F-test	4.30	24.51	16.46	18.92	9.20	17.37	21.99	14.88	13.32	14.93
[p-value]	[.002]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]
Observations	2,166	2,166	1,084	1,082	1,080	1,086	1,077	1,089	378	1,788
R-squared	0.215	0.062	0.034	0.082	0.075	0.038	0.079	0.034	0.114	0.051
Number of city pairs	252	252	129	123	124	128	126	126	42	210
Mean Dep. Var.	0.243	0.120	0.087	0.154	0.167	0.075	0.142	0.099	0.185	0.107

Notes: Dependent variable in column 1 is Institutions--Base as defined in Table 1B; dependent variable in colums 2 to 10 is a 0/1 variable for whether there is a railway connection between the two cities in that year; estimation by least squares. ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. Robust standard errors clustered by city-pair in parentheses. All regressions include year- and city-pair fixed effects. Bilateral distance is the bilateral distance between the two cities. Distance to Paris is the mean distance to Paris of the city pair. Protestant is the mean protestant share in the city pair. Waterway takes the city-pair average for an indicator of whether each city lies on a navigable river, canal, or the coast in 1850 according to Kunz (2014).

Table 5: The impact of railways and institutions on price gaps

	(1) Baseline	(1A) City Clustering	(2) Railway Cost Deciles	(3) Full Sample	(4) State Pop'n Weight	(5) City Pop'n Weight	(6) Bavaria	(7) Mecklenburg
Railways	-0.103*	-0.174**	-0.094*	-0.165**	-0.090+	-0.134+	-0.113**	-0.097*
	(0.043)	(0.044)	(0.044)	(0.056)	(0.052)	(0.078)	(0.044)	(0.045)
Institutions	-0.393**	-0.420**	-0.363**	-0.446**	-0.443*	-0.311**	-0.380**	-0.373**
	(0.119)	(0.137)	(0.115)	(0.150)	(0.194)	(0.084)	(0.120)	(0.111)
[1820-1835] x	0.041*	0.028	0.044**	0.038+	0.012	0.027	0.037*	0.042*
Bilateral Distance	(0.018)	(0.030)	(0.016)	(0.020)	(0.032)	(0.020)	(0.018)	(0.017)
[1840-1860] x	0.052**	0.042	0.057**	0.049*	0.020	0.043*	0.049**	0.052**
Bilateral Distance	(0.017)	(0.028)	(0.015)	(0.021)	(0.031)	(0.022)	(0.017)	(0.016)
First Stages								
Railways F-Stat	24.22	44.23	24.92	19.71	18.30	4.24	22.04	23.52
[P-val]	[.000]	[.000]	[.000]	[.000]	[.000]	[.003]	[.000]	[.000]
Institutions F-Stat	4.25	3.79	5.41	4.36	3.01	2.28	4.16	3.74
[P-val]	[.002]	[0.015]	[.000]	[.002]	[.031]	[.062]	[.003]	[.006]
OLS								
Railways	-0.043**	-0.043**	-0.043**	-0.041**	-0.039**	-0.028**	-0.040**	-0.040**
	(0.009)	(0.009)	(0.009)	(0.007)	(0.009)	(0.011)	(0.009)	(0.009)
Institutions	-0.043	-0.043	-0.043	-0.046	-0.087**	-0.067+	-0.041	-0.041
	(0.032)	(0.032)	(0.032)	(0.032)	(0.029)	(0.036)	(0.032)	(0.032)
Observations	2,166	2,166	2,166	3,570	2,166	2,166	3,570	2,166
Number of city pairs	252	252	252	642	252	252	642	252

Notes: Dependent Variable is absolute value of percentage price gap; estimation method is Hansen et al.'s (1996) GMM limited information maximum likelihood estimator; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1 robust standard errors clustered by city-pair in parentheses, except column (1A) which clusters by city. All regressions include year- and city-pair fixed effects. "State Pop'n weight" weighs each observation at the average of the 1816 state population of the city-pair. "City Pop'n weight" weighs each obs at the average of the 1800 population of the city-pair. "Bavaria" reduces the representation of Bavaria in the sample: the weight of each observation containing a Bavarian city is reduced such that the total sum of regression weights is 10% lower than in (3); "Mecklenburg" reduces analogously the representation of Mecklenburg cities in the base sample.

Table 6: The impact of trains and institutions on price gaps -additional results

	(1) Institutions Feudal Lands	(2) Institutions Broad	(3) Full Sample	(4) French Rule Y/N	(5) Hanse	(6) Zollverein	(7) 1848 Revolution	(8) War	(9) Likelihood of Trade	(10) No State Capitals
Trains	-0.114**	-0.126**	-0.203**	-0.145**	-0.143**	-0.108*	-0.102*	-0.123**	-0.091*	-0.120**
	(0.042)	(0.042)	(0.059)	(0.051)	(0.046)	(0.042)	(0.043)	(0.046)	(0.042)	(0.046)
Institutions	-0.195*	-0.215+	-0.322*	-0.429*	-0.249+	-0.380**	-0.393**	-0.319**	-0.377**	-0.220*
	(0.084)	(0.111)	(0.144)	(0.170)	(0.129)	(0.119)	(0.119)	(0.121)	(0.120)	(0.096)
[1820-1835] x	0.033*	0.027+	0.030	0.031+	0.030+	0.026	0.041*	0.037*	0.038**	0.022
Bilateral Distance	(0.014)	(0.015)	(0.019)	(0.017)	(0.016)	(0.021)	(0.018)	(0.017)	(0.015)	(0.018)
[1840-1860] x	0.046**	0.038**	0.032+	0.043**	0.040**	0.040*	0.052**	0.049**	0.054**	0.034*
Bilateral Distance	(0.013)	(0.014)	(0.019)	(0.016)	(0.015)	(0.018)	(0.017)	(0.017)	(0.015)	(0.017)
Zollverein IV						-0.060				
						(0.052)				
1848 Revolution							-0.004			
							(0.014)			
War								0.047**		
								(0.012)		
Observations	2,166	2,166	3,570	2,166	2,166	2,166	2,166	2,166	2,166	2,094
Number of city pairs	252	252	642	252	252	252	252	252	252	246

Notes: Dependent variable is the absolute value of percentage price gap between j and k; estimation is by Hansen et al.'s (1996) GMM limited informational maximum likelihood estimator; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1 robust standard errors clustered by city pair in parentheses. All regressions include year- and city-pair fixed effects. Bilateral distance is the bilateral distance between the two cities. Institutions measure is "Feudal Lands" in (1), "Broad" in (2) to (5) and (10), and "Base" in (6) to (9), see Table 1B for definitions. French Rule Y/N is a dummy equal to 1 if either city was ever under French rule. Hanse treats French rule in the cities of Hamburg, Bremen and Luebeck as in all other cities, even though the Hanse is a special case. Zollverein is the log average distance of a city-pair to the closest seabord, relative to other city pairs that were not part of the Zollverein in that year, plus 1. 1848 Rev is the mean across the city-pair of an indicator for whether the city experienced 1848 revolutionary activity. War is the mean across the city-pair of an indicator for the times when each city was experiencing wars. Likelihood of Trade doubles regression weight for cities located on waterways. "No State Capitals" drops all city-pairs where both cities are state capitals.

Table 7: City size, French rule, and railway cost: Reduced form results

	(1) Baseline	(2) Full Sample	(3) Robust Regression
[1840-1860] x	-0.221**	-0.237**	-0.129**
Railway Cost	(0.060)	(0.061)	(0.023)
[1865-1880] x	-0.374	-0.388	0.018
Railway Cost	(0.338)	(0.344)	(0.036)
[1820-1835] x	0.068	0.071	0.114**
French Rule	(0.048)	(0.047)	(0.007)
[1840-1860] x	0.061+	0.063+	0.122**
French Rule	(0.033)	(0.034)	(0.007)
[1820-1835] x	0.295	0.382+	-0.070+
Bilateral Distance	(0.223)	(0.208)	(0.041)
[1840-1860] x	0.430**	0.415**	0.105**
Bilateral Distance	(0.151)	(0.149)	(0.036)
Observations	268	312	268
Number of cities	28	40	28

Notes: Dependent variable is the log of city population; estimation method: least squares (1) and (2), and robust regression using STATA's rreg routine (3); ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1; standard errors in parentheses, robust and clustered at the city level in (1) and (2). All regressions include year- and city fixed effects. Bilateral distance is the average distance between the city and all others in the sample.

Table 8: City Size, French rule, and railway cost. The city size exclusion restrictions

		Important Region	Geography		Culture	Human Capital	Easily Occupied	Transport	Int'l Trade
	(1) Base	(2) Coal	(3) Latitude	(4) Longitude	(5) Protestant	(6) Schooling	(7) Dist Paris	(8) Waterway	(9) Coastal
[1840-1860] x	-0.129**	-0.093**	-0.194**	-0.227**	-0.285**	-0.133**	-0.206**	-0.129**	-0.127**
Railway Cost	(0.023)	(0.021)	(0.017)	(0.022)	(0.016)	(0.023)	(0.023)	(0.023)	(0.019)
[1865-1880] x	0.018	0.037	0.032	-0.078*	0.059+	0.041	0.045	0.016	0.053+
Railway Cost	(0.036)	(0.033)	(0.033)	(0.037)	(0.031)	(0.041)	(0.041)	(0.037)	(0.029)
[1820-1835] x	0.114**	0.110**	0.157**	0.076**	0.189**	0.117**	0.147**	0.112**	0.110**
French Rule	(0.007)	(0.007)	(0.006)	(0.011)	(0.006)	(0.007)	(0.012)	(0.008)	(0.006)
[1840-1860] x	0.122**	0.126**	0.167**	0.101**	0.201**	0.124**	0.171**	0.123**	0.124**
French Rule	(0.007)	(0.007)	(0.005)	(0.009)	(0.006)	(0.007)	(0.010)	(0.007)	(0.006)
[1820-1835] x	-0.070+	0.010	-0.130**	0.037	0.122**	-0.058	-0.013	-0.074+	0.003
Bilateral Distance	(0.041)	(0.041)	(0.034)	(0.041)	(0.030)	(0.043)	(0.046)	(0.043)	(0.033)
[1840-1860] x	0.105**	0.132**	0.004	0.174**	0.224**	0.095*	0.113*	0.089*	0.127**
Bilateral Distance	(0.036)	(0.035)	(0.032)	(0.037)	(0.028)	(0.037)	(0.044)	(0.038)	(0.029)
y20s_X		0.079**	0.033**	-0.031**	0.308**	-0.025	0.026*	-0.026+	-0.068**
		(0.023)	(0.003)	(0.006)	(0.017)	(0.016)	(0.012)	(0.015)	(0.010)
y30s_X		0.040+	0.047**	-0.027**	0.347**	0.002	0.037**	0.006	-0.029*
		(0.023)	(0.003)	(0.006)	(0.017)	(0.016)	(0.012)	(0.015)	(0.013)
y40s_X		0.016	0.044**	-0.019**	0.336**	0.021	0.047**	0.024+	-0.025*
		(0.021)	(0.003)	(0.004)	(0.016)	(0.016)	(0.010)	(0.014)	(0.012)
y50s_X		-0.005	0.052**	-0.019**	0.354**	0.021	0.054**	0.019	
		(0.021)	(0.004)	(0.004)	(0.017)	(0.016)	(0.011)	(0.015)	
P-val: Instruments Jointly Sig	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]
Observations	268	268	268	268	268	268	268	268	268

Notes: Dependent variable is log city population; estimation method is robust regression; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1; standard errors in parentheses. All regressions include year- and city fixed effects. Railway Cost as defined in the text. French Rule is log(years french rule +1). Bilateral distance is the average distance between the city and all others in the sample. Coal is a 0/1 variable for whether the city had coal production in 1850. Protestant is the mean protestant share in the city. Schooling is whether the city had a Gymnasium by 1580. Waterway is a 0/1 variable for whether the city is on a navigable river, canal, or the coast according to Kunz (2014). Distance to Paris is the direct distance to Paris, yxxs_X are scaled by 100. Coastal is a 0/1 variable for the city to be in the first quartile of distance to the coast and the US price of wheat low. The additional x's (columns (2) to (10)) are interacted with decade, 1820s, 30s, 40s, and 50s to allow time-varying impact.

Table 9: The impact of institutions and trade on city size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Trade Only		Quantile		RR Cost	Institutions	Institutions
	Trade Only	Quantile	Baseline	Median	Institutions	Deciles	Feudal Lands	Broad
Trade	-4.750*	-4.621*	-4.228+	-4.780+		-3.507+	-4.692+	-4.707*
[P-val]	[.015]	[.025]	[.083]	[.071]		[.062]	[.055]	[.045]
Institutions			0.198	0.097	0.469*	0.239	0.165	0.214
[P-val]			[.250]	[.255]	[0.031]	[.231]	[.201]	[.200]
First Stages								
Trade F-Stat	8.21	2.44	8.21	2.44		7.66	8.21	8.21
[P-val]	[.000]	[.045]	[.000]	[.045]		[.000]	[.000]	[.000]
Institutions F-Stat			11.97	4.09	20.18	17.67	19.22	12.19
[P-val]			[.000]	[.028]	[0.000]	[.000]	[.000]	[.000]
OLS								
Trade	0.071	0.071	0.072	0.072		0.072	0.061	0.066
(Standard error)	(0.064)	(0.064)	(0.064)	(0.064)		(0.064)	(0.068)	(0.067)
Institutions			-0.068	-0.068	-0.068	-0.068	-0.215+	-0.268
(Standard error)			(0.284)	(0.284)	(0.256)	(0.284)	(0.115)	(0.182)
Number of obs	268	268	268	268	268	268	268	268
Number of cities	28	28	28	28	28	28	28	28

Notes: Dependent variable is log city population; results from two-sample instrumental variables estimation as described in the text, using Hansen et al.'s (1996) GMM limited information maximum likelihood in columns (3) to (8); least squares in (1) and (2). Bootstrapped p-values from one-sided t-tests with clustering at the city level in brackets (200 replications), except in column 5 which uses robust p-values based on city-clustering. ** p-value < 0.01, * p-value < 0.05, + p-value < 0.10. All regressions include bilateral distance interacted with twenty-year windows. All regressions include year- and city-pair fixed effects (with price gap as dependent variable), or year- and city fixed effects (with city size as dependent variable). The first stage F-stat for Institutions is the Angrist-Pischke multivariate F test of excluded instruments from a regression at the city level, taking the predicted price gaps as given. The first stage F-stat for Trade comes from the city-pair analysis. Column (2) and (4) use median regression in the trade first stage. Institutions measure is "Base" except "Feudal Lands" in (6) and "Broad" in columns (7) and (8); see Table 1B for definitions.

Table 10: The impact of trade and institutions on city size. Additional results

	(1) Full Sample	(2) Institutions Broad	(3) State Pop'n Weight	(4) City Pop'n Weight	(5) Zollverein	(6) 1848 Revolution	(7) War	(8) No State Capitals	(9) Hanse	(10) Re-Districting
Trade	-3.109+	-3.655+	-5.515+	-1.710+	-4.403+	-4.265+	-4.135+	-1.107+	-5.096+	-3.999+
	[.057]	[.090]	[.094]	[0.074]	[.061]	[.068]	[.067]	[.070]	[.061]	[.056]
Institutions	0.337	0.342	0.846	0.598	0.197	0.194	0.169	0.027	0.286	0.251
	[.139]	[.175]	[.262]	[0.279]	[0.228]	[.260]	[.282]	[.477]	[.264]	[.224]
Zollverein Member					0.383					
					[0.340]					
1848 Revolution						-0.016				
						[.333]				
War							-0.047			
							[.200]			
Observations	312	312	268	268	268	268	268	218	268	268
Number of cities	40	40	28	28	28	28	28	24	28	28

Notes: Dependent variable is log city population; Results from two-sample IV estimation as described in the text, using Hansen et al.'s (1996) GMM limited information maximum likelihood. Bootstrapped p-values from one-sided t-tests with clustering at the city level in brackets; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. All regressions include year- and city pair fixed effects (with price gap as dependent variable), or year- and city fixed effects (with city size as dependent variable). Institutions variable "Base" except in (4), (8), and (9) where the institutions variable is "Broad", see Table 1B for definitions. All regressions include bilateral distance interacted with time period. "State Pop'n weight" weighs each observation at the state's 1816 population. "City Pop'n weight" weighs each observation at the city's 1800 population. Zollverein Member is the log of distance to the nearest coast relative to other cities that were not Zollverein members in that year, plus 1; see Keller and Shiue (2013). 1848 Revolution is an indicator for whether the city experienced 1848 revolutionary activity. War is an indicator for whether the city was experiencing wars. No State Capitals drops all cities that were state capitals. Hanse treats French rule in the cities of Hamburg, Bremen and Luebeck as in all other cities, even though the Hanse is a special case (see text). Re-districting is based on an alternative dependent variable where city size incorporates re-districting, in part due to migration.

Figure 1: States in the German Confederation 1848

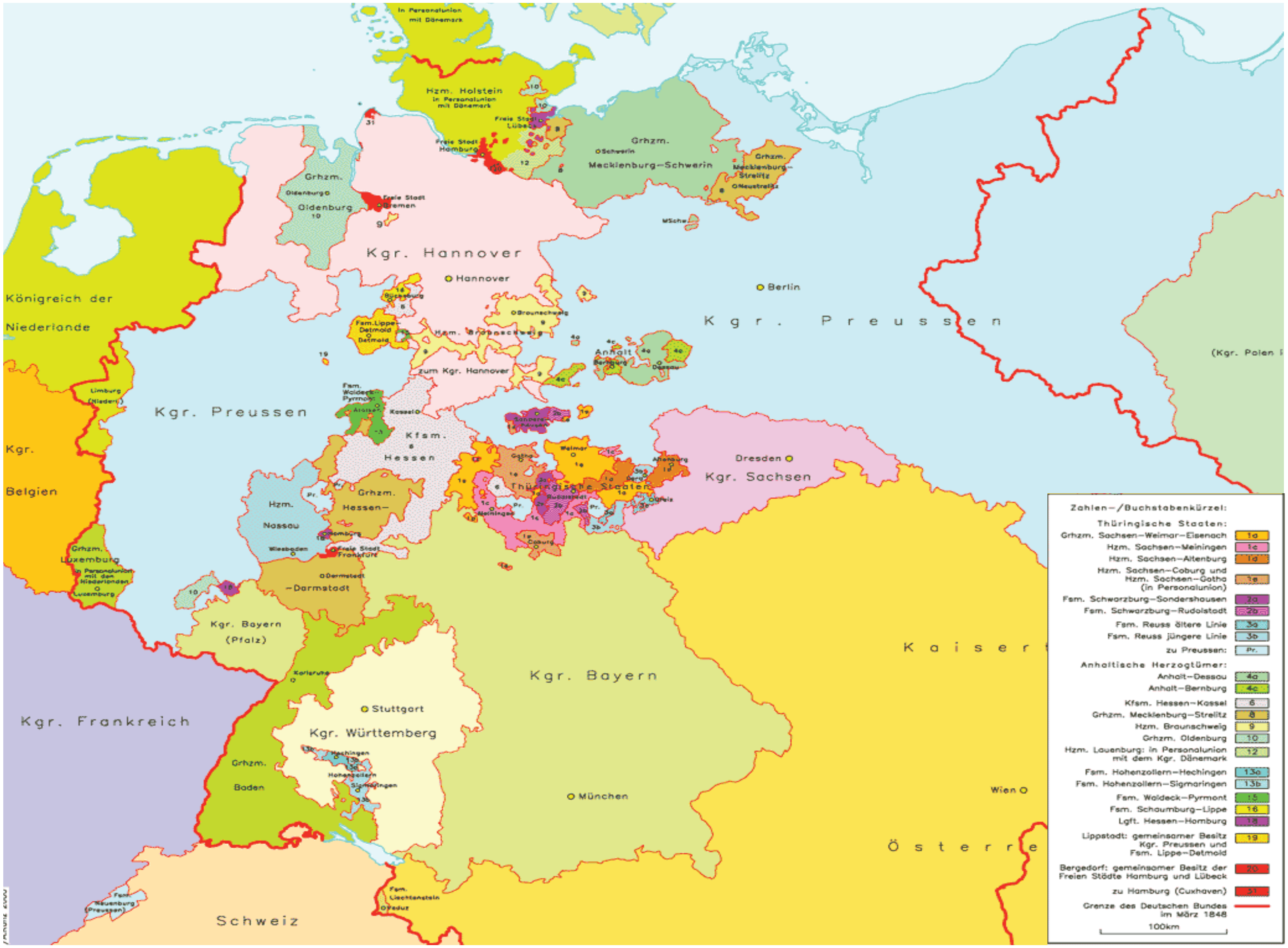

Figure 2: Sample Cities

Figure 3: The Railroad Network in 1880

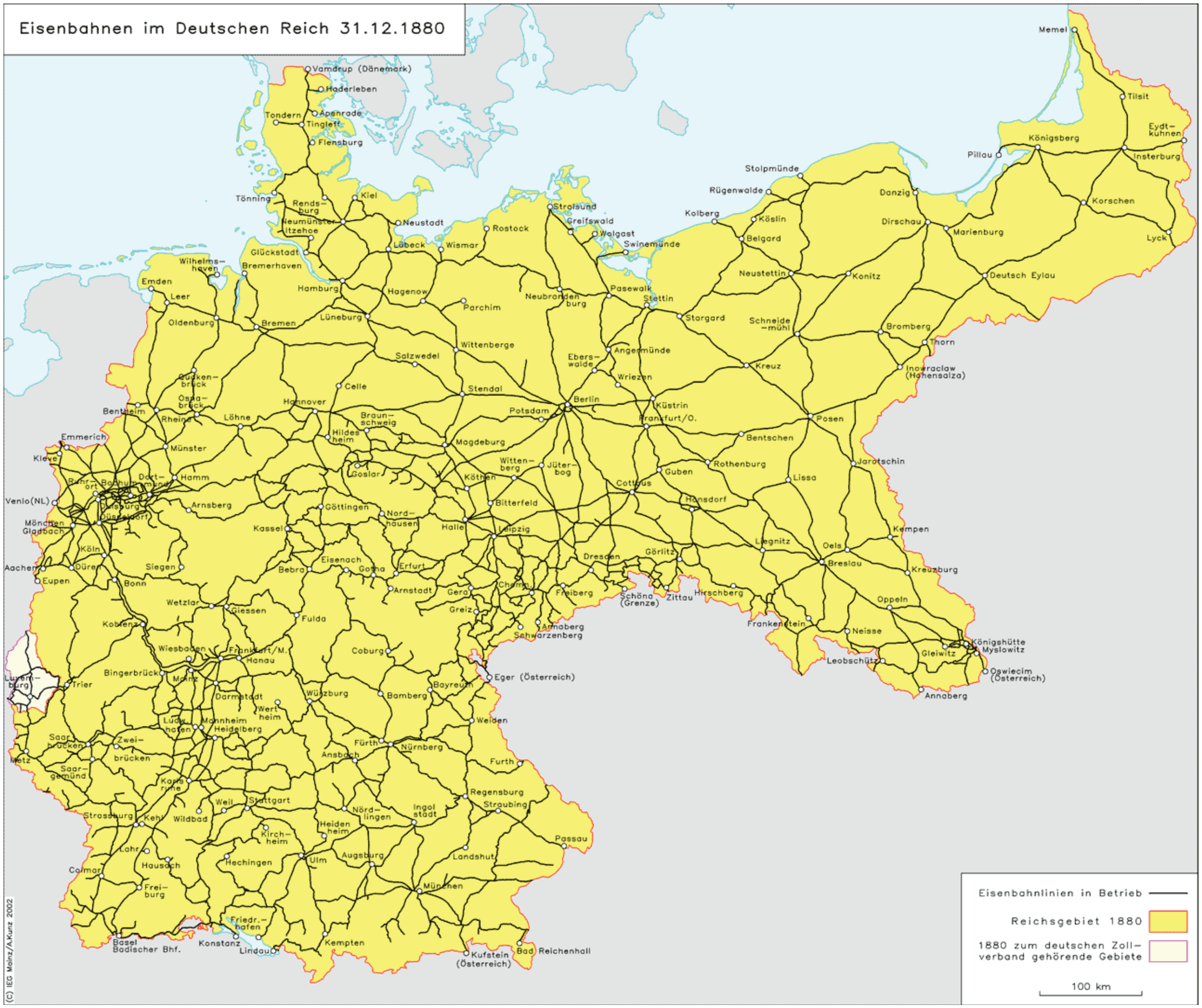

Figure 4: Friedrich List’s National Railway System Proposal, 1833

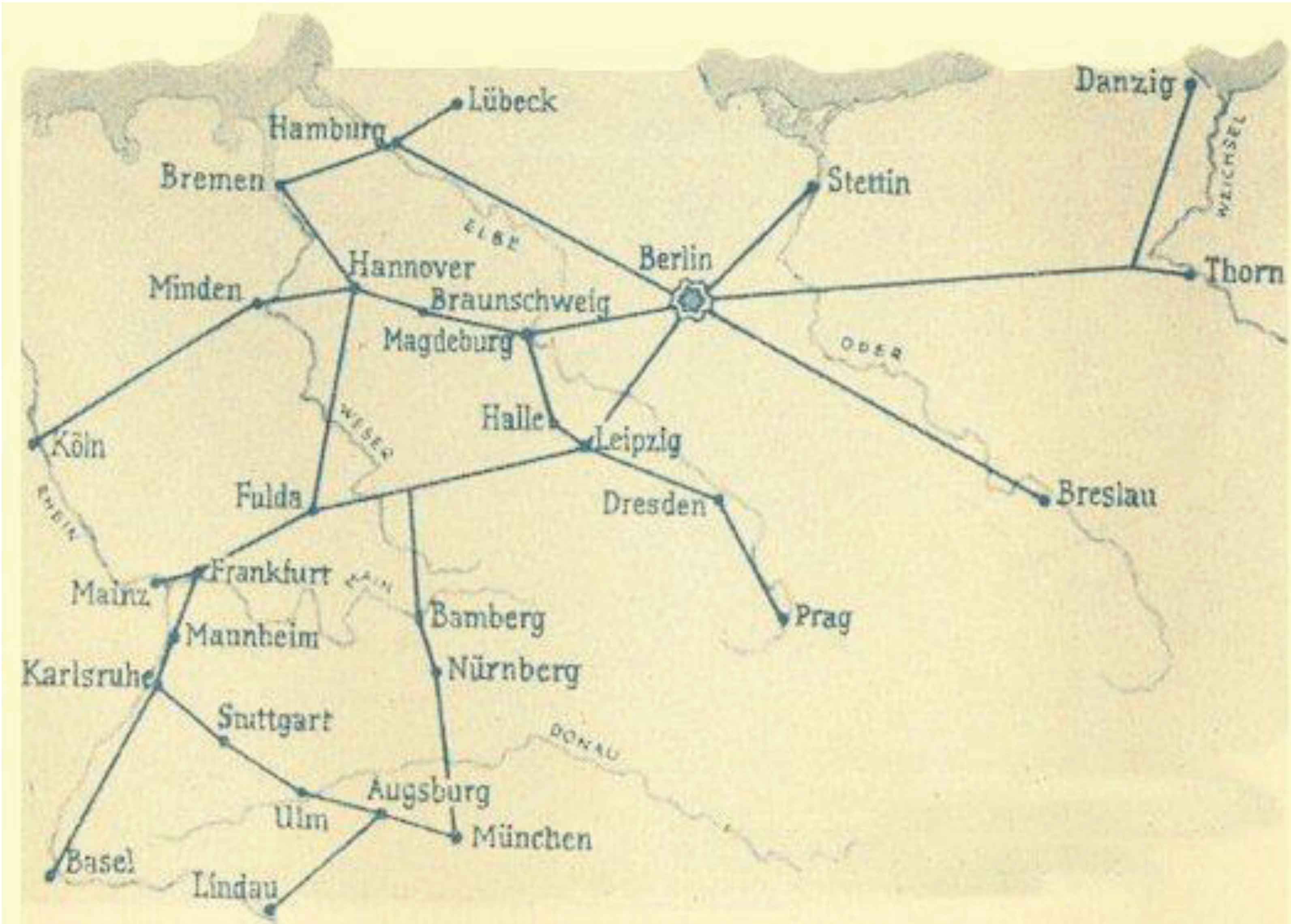

Figure 5: The Railroad Network in 1850

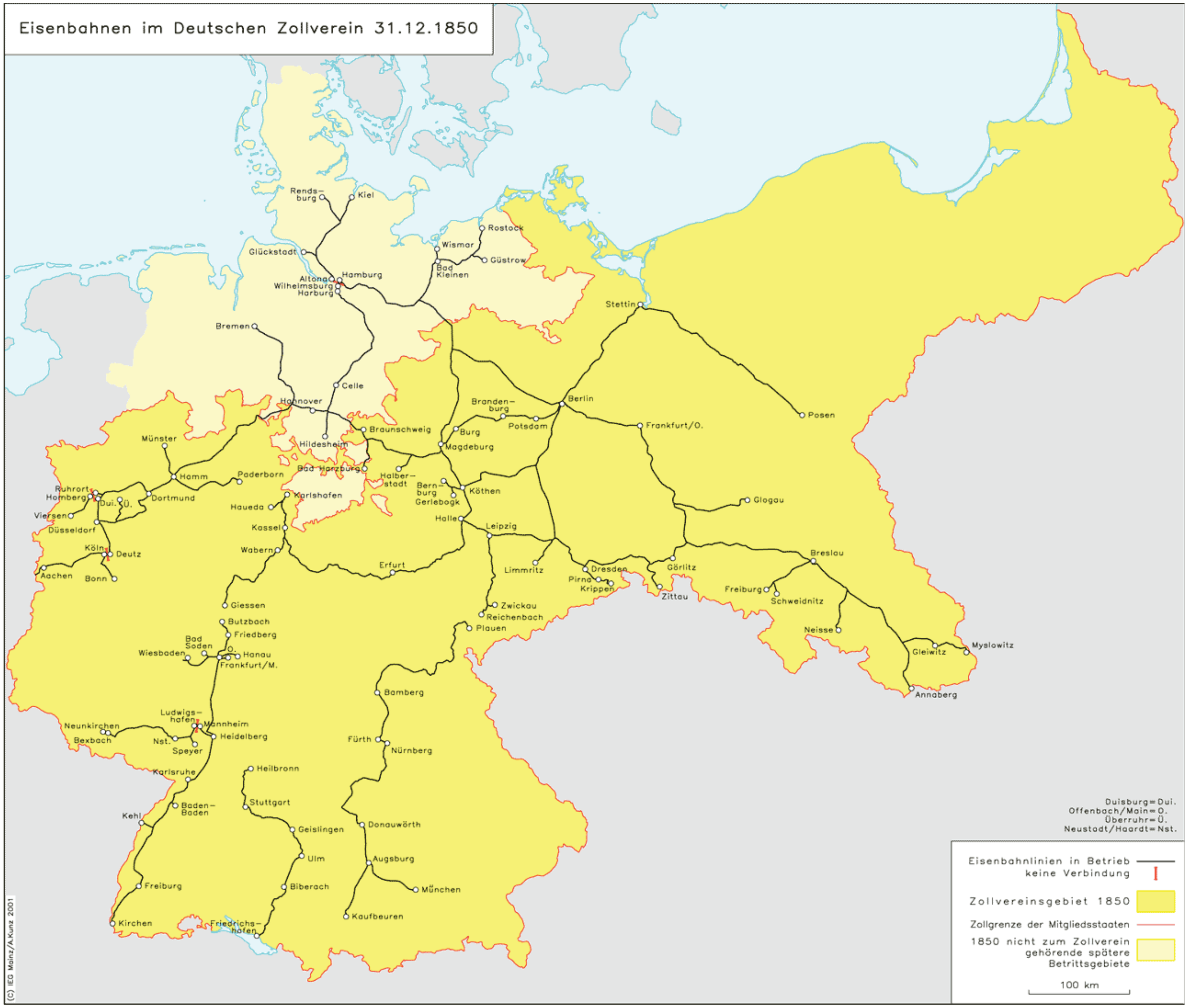

Figure 6: Price gaps before and after steam railways

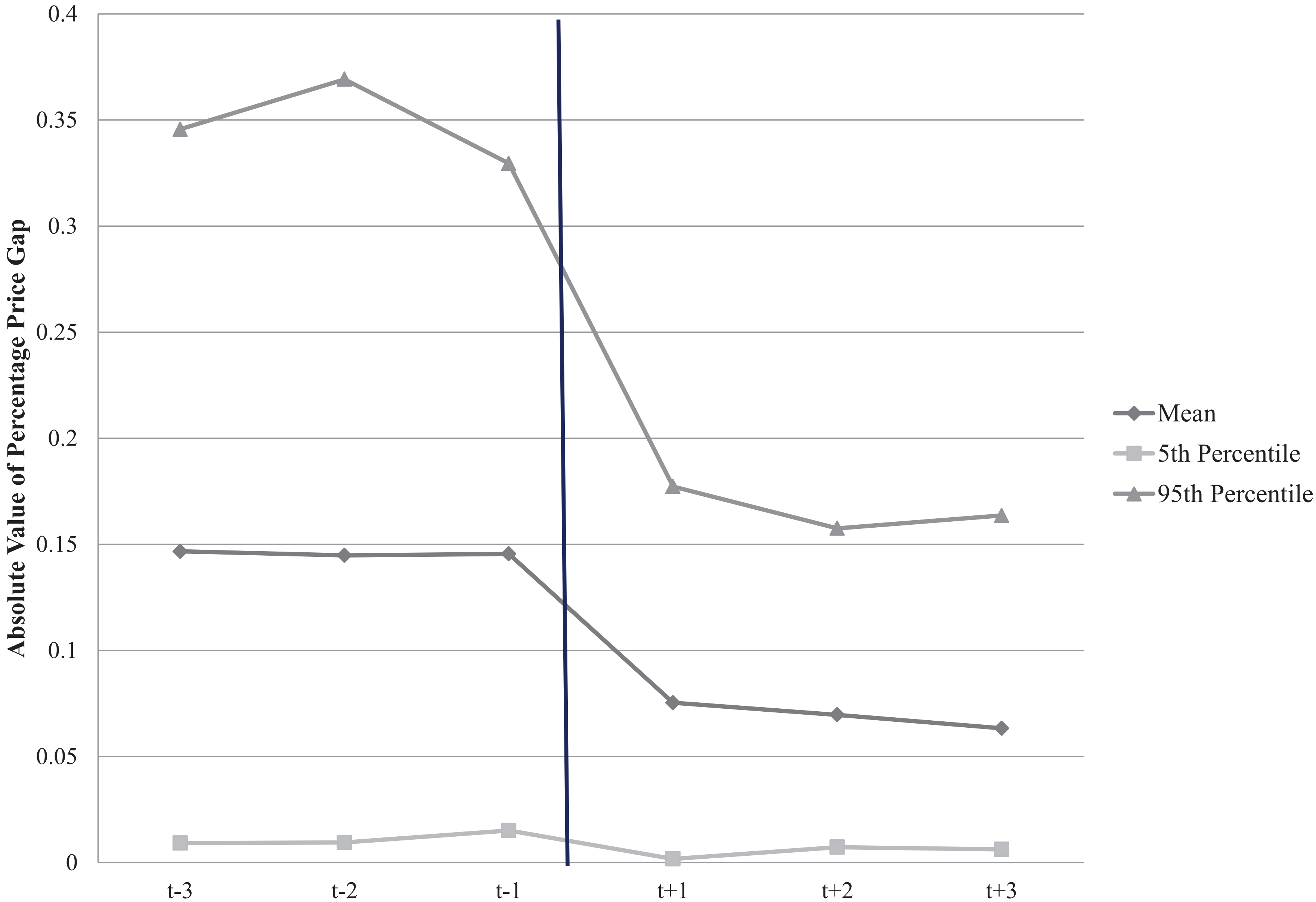

Figure 7: Population in Cities Abolishing Guilds early versus late

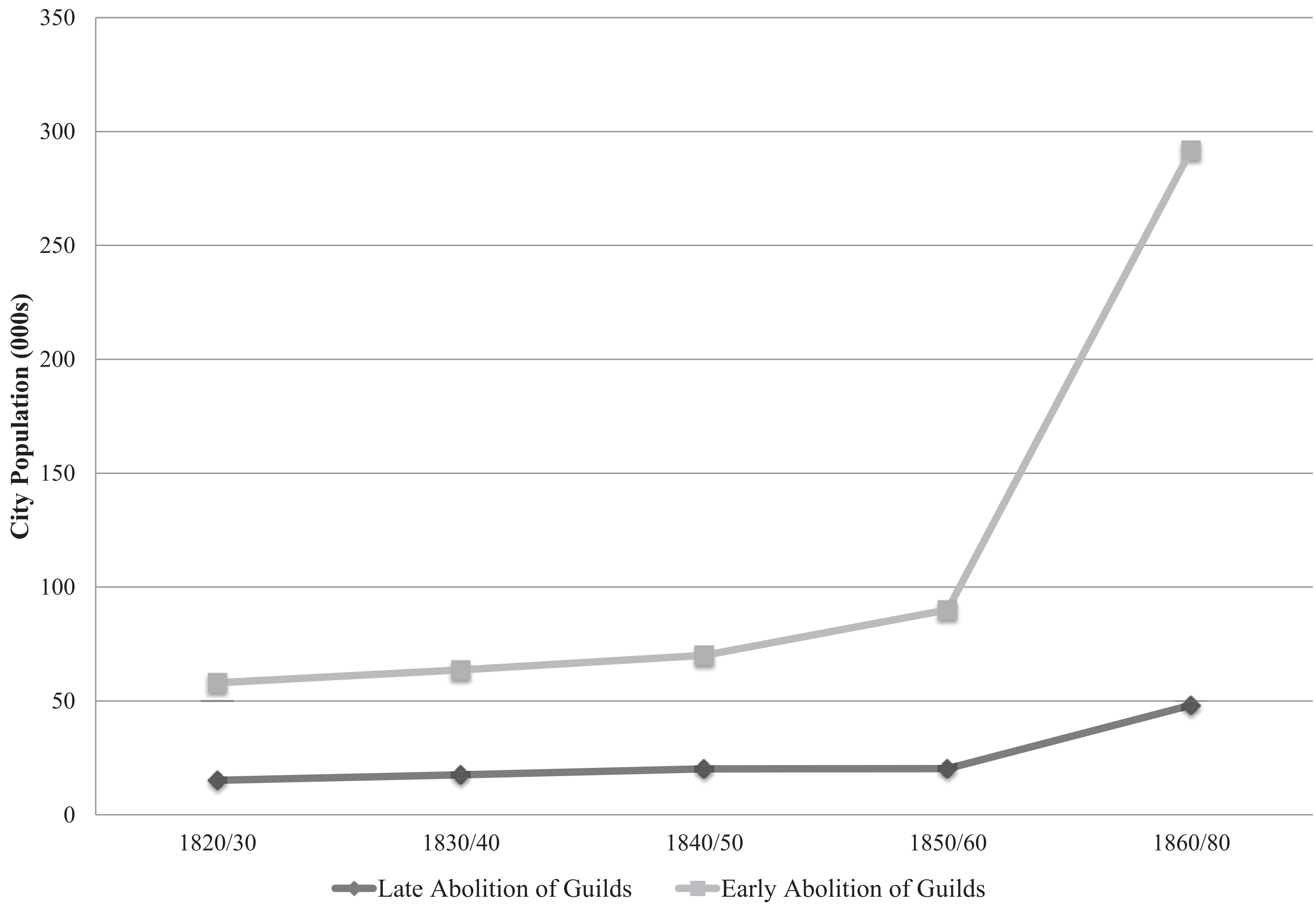

Figure 8. The Distribution of City Size

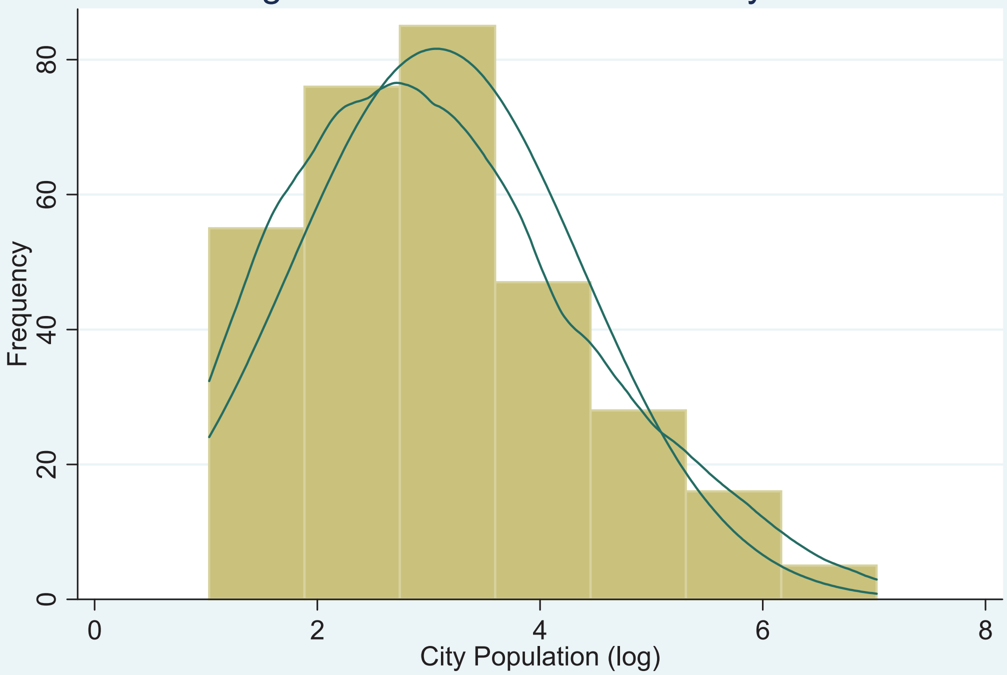

Figure 9: Trade and City Growth

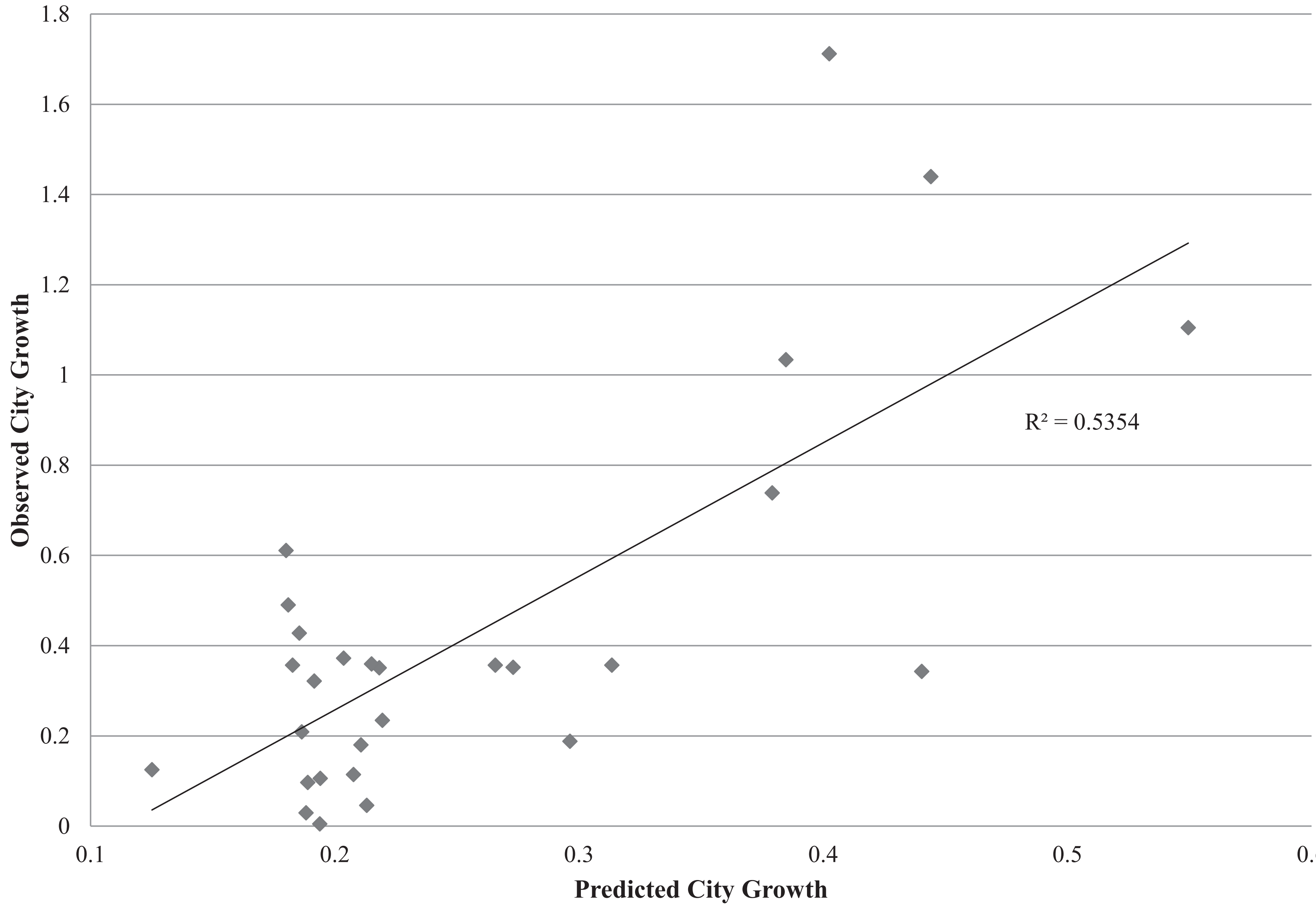

Figure 10: Institutions, Trade, and Innovation

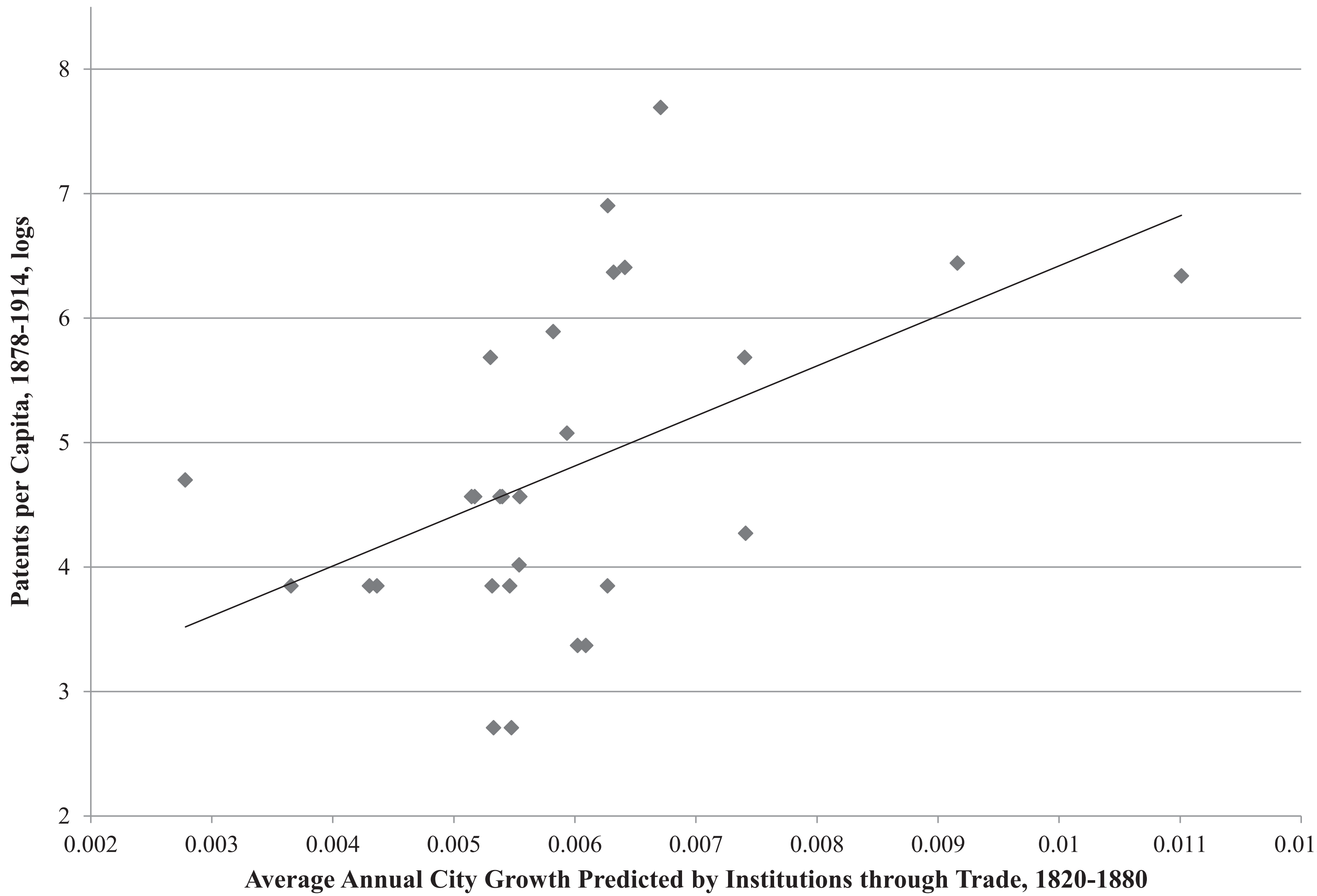

Table A: The impact of trains and institutions on price gaps. Alternative Explanations

		Important Regions		Geography				Culture	Human Capital	Easily Occupied	Transport	Int'l Trade
	(1) Base	(2) List RR Plan	(3) Coal	(4) Latitude	(4A) Latitude'	(5) Longitude	(5A) Longitude'	(6) Protestant	(7) Schooling	(8) Dist Paris	(9) Waterway	(10) Coastal
Railways	-0.103*	-0.109*	-0.125*	-0.123*	-0.127*	-0.221**	-0.229**	-0.085+	-0.096*	-0.230**	-0.134**	-0.076+
	(0.043)	(0.051)	(0.054)	(0.059)	(0.054)	(0.062)	(0.061)	(0.047)	(0.043)	(0.075)	(0.049)	(0.045)
Institutions	-0.393**	-0.410**	-0.393**	-0.392**	-0.333**	-0.389*	-0.408*	-0.453**	-0.342**	-0.351*	-0.356*	-0.387**
	(0.119)	(0.125)	(0.121)	(0.124)	(0.129)	(0.154)	(0.172)	(0.141)	(0.111)	(0.155)	(0.140)	(0.109)
[1820-1835] x	0.041*	0.043*	0.041*	0.034*	0.026	0.044+	0.045+	0.026	0.036*	0.045+	0.031	0.046**
Bilateral Distance	(0.018)	(0.017)	(0.019)	(0.017)	(0.018)	(0.024)	(0.027)	(0.018)	(0.015)	(0.023)	(0.019)	(0.017)
[1840-1860] x	0.052**	0.055**	0.050**	0.049**	0.035*	0.038+	0.042+	0.040*	0.047**	0.040+	0.043*	0.051**
Bilateral Distance	(0.017)	(0.017)	(0.018)	(0.014)	(0.015)	(0.020)	(0.022)	(0.016)	(0.013)	(0.021)	(0.019)	(0.016)
y20s_X		0.000	-0.031	-0.006	-0.002	-0.008	-0.006	-0.035	-0.021	-0.001	-0.062+	0.065**
		(0.022)	(0.036)	(0.010)	(0.009)	(0.010)	(0.010)	(0.046)	(0.018)	(0.018)	(0.033)	(0.022)
y30s_X		-0.060**	0.054	-0.006	-0.009	-0.001	0.006	-0.091*	-0.016	0.001	-0.039	-0.083**
		(0.020)	(0.037)	(0.010)	(0.009)	(0.009)	(0.008)	(0.046)	(0.016)	(0.017)	(0.032)	(0.019)
y40s_X		-0.020	-0.024	-0.009	-0.009	0.007	0.011	-0.065	-0.047**	0.019	-0.054+	-0.073**
		(0.023)	(0.034)	(0.010)	(0.009)	(0.008)	(0.008)	(0.047)	(0.016)	(0.017)	(0.032)	(0.016)
y50s_X		-0.015	0.031	0.005	0.001	0.013+	0.017*	-0.099*	0.002	0.033	0.019	
		(0.021)	(0.035)	(0.014)	(0.012)	(0.008)	(0.008)	(0.050)	(0.018)	(0.021)	(0.028)	
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
Number of city pairs	252	252	252	252	252	252	252	252	252	252	252	252

Notes: Dependent variable is the absolute value of percentage price gap between j and k; estimation by Hansen et al.'s (1996) GMM limited information maximum likelihood estimator; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1; robust standard errors with clustering at the city-pair level in parentheses. All regressions include year- and city-pair fixed effects. Bilateral distance is the distance between the two cities in the pair. List RR Plan is whether the city pair was on List's Rail Map (see Figure 4). Coal is the city-pair average of a dummy for whether the city had coal production in 1850. Latitude and Longitude are the mean latitude and longitude of the city pair. Latitude' and Longitude' reduces regression weight of observations to one half if they are not both above median, or both below median. Protestant is the mean protestant share in the city pair. Schooling is whether both cities had a Gymnasium by 1580. Distance to Paris is the mean distance to Paris of the city pair, yxxs_X are scaled by 100. Waterway takes the city-pair average for an indicator of whether the city lies on a navigable river, canal, or the coast in 1850 according to Kunz (2014). Coastal is an indicator equal to 1 if both cities are within the first quartile of distance to the coast and the US price of wheat is below the median. The additional x's (col 2-10) are interacted with decade, 1820s, 30s, 40s, and 50s to allow time-varying impact.

Table B: French rule and institutional change across subsamples

		Endowments			Geography				Proximity to France		Culture	
	Baseline	No Coal	Waterway		Latitude		Longitude		Distance From Paris		Protestant	
			High	Low	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[1820-1835] x	0.139+	0.138**	0.053+	0.271*	0.225	0.129**		0.134+		0.124+	0.306+	0.033**
French Rule	[.070]	[.000]	[.076]	[.046]	[.237]	[.000]		[.061]		[.082]	[.062]	[.000]
[1840-1860] x	0.136+	0.135**	0.043	0.259*	0.235	0.133**		0.132+		0.127+	0.294+	0.052**
French Rule	[.070]	[.000]	[.122]	[.046]	[.168]	[.000]		[.061]		[.082]	[.054]	[.000]
Trade	1.016	1.354	-5.259	1.055	-1.360	1.292**	2.529	-0.856	-0.304	0.790	0.954	1.137**
	[.256]	[.231]	[.214]	[.130]	[.298]	[.000]	[.115]	[.215]	[.420]	[.164]	[.362]	[.000]
[1820-1835] x	0.244	0.293	-0.111	0.598*	0.670	-0.014**	0.219	0.032	0.572	-0.050	0.671+	-0.676
Bilateral Distance	[.256]	[.194]	[.450]	[.038]	[.153]	[.000]	[.340]	[.344]	[.305]	[.364]	[.062]	[.133]
[1840-1860] x	0.256	0.302	0.011	0.582*	0.808+	-0.014+	0.298	0.038	0.746	-0.041	0.661+	-0.629
Bilateral Distance	[.209]	[.172]	[.359]	[.038]	[.099]	[.072]	[.280]	[.393]	[.165]	[.445]	[.062]	[.253]
First Stage												
Institutions F-Stat	13.21	11.41	0.91	4.84	3.47	31.15		39.44		188.49	6.96	10.38
[P-val]	[.000]	[.000]	[.404]	[.008]	[.031]	[.000]		[.000]		[.000]	[.001]	[.000]
Observations	268	216	112	156	121	147	139	129	134	134	147	121
Number of cities	28	22	11	17	11	17	14	14	13	15	15	13
Mean Dep. Var.	0.209	0.137	0.205	0.212	0.202	0.214	0.101	0.326	0.112	0.306	0.146	0.285

Notes: Dependent variable is the institutions variable "Institutions--Base", see Table 1B for definition; estimation by least squares; ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. bootstrapped p-values based on clustering at the city level (except column 6 where the number of clusters is insufficient) from a one-tailed t-test in brackets. All regressions include year- and city-pair (when price gap is the dependent variable), or year- and city fixed effects (when city size is the dependent variable). All regressions take the predicted price gap from the city-pair first stage using the baseline sample. French Rule is log(mean years french rule in the city pair +1). Bilateral distance is the average distance between the city and all others in the sample. No Coal are cities that had no coal production in 1850. Waterway is an indicator of whether the city lies on a navigable river, canal, or on the coast in 1850 according to Kunz (2014). Protestant is the mean protestant share in the city.

Table C: The impact of institutions through trade on city size. Omitted Variables

		Important Region	Geography		Culture	Human Capital	Easily Occupied	Transport	Int'l Trade
	Baseline	Coal	Latitude	Longitude	Protestant	Schooling	Distance to Paris	Waterway	Coastal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Trade	-4.228 +	-4.291+	-4.812+	-3.580+	-1.873	-3.553+	-3.808+	-3.748+	-3.953+
	[.083]	[.082]	[.099]	[.086]	[.253]	[.062]	[.062]	[.091]	[.064]
Institutions	0.198	0.56	0.276	-0.045	0.632	0.242	0.145	0.409	0.182
	[.250]	[.176]	[.215]	[.807]	[.215]	[.273]	[.443]	[.198]	[.255]
[1820-1835] x	0.054	0.586	-0.120	0.005	-0.215	-0.017	0.079	0.044	0.008
Bilateral Distance	[.479]	[.247]	[.759]	[.396]	[.754]	[.588]	[.407]	[.553]	[.580]
[1840-1860] x	0.381	0.595+	0.184	0.408	0.028	0.303	0.403	0.348	0.334
Bilateral Distance	[.151]	[.066]	[.346]	[.102]	[.544]	[.222]	[.139]	[.132]	[.170]
y20s_X		0.324	0.062	-0.014	0.346	-0.069	-0.000	-0.166	0.067
		[.115]	[.152]	[.746]	[.179]	[.366]	[.320]	[.802]	[.197]
y30s_X		0.406*	0.060	-0.004	0.306	-0.036	-0.000	-0.137	0.024
		[.049]	[.157]	[.695]	[.185]	[.407]	[.351]	[.782]	[.383]
y40s_X		0.154	0.063	-0.007	0.323	-0.088	0.000	-0.112	0.029
		[.159]	[.157]	[.685]	[.179]	[.289]	[.474]	[.751]	[.378]
y50s_X		0.275*	0.060	-0.023	0.268	0.008	-0.000	-0.037	
		[.038]	[.162]	[.761]	[.200]	[.474]	[.304]	[.629]	
Observations	268	268	268	268	268	268	268	268	268
Number of cities	28	28	28	28	28	28	28	28	28

Notes: Dependent variable is log city population; results from two-sample instrumental variables estimation as described in the text, using Hansen et al.'s (1996) GMM limited information maximum likelihood estimator. Bootstrapped p-values based on a one-sided test with city-level clustering in brackets (200 replications); ** p-value < 0.01, * p-value < 0.05, + p-value < 0.1. All regressions include year- and city-pair fixed effects (when the dependent variable is price gap), or year- and city fixed effects (when the dependent variable is city size). Coal is a 0/1 variable indicating coal production near the city in 1850. Protestant is the mean protestant share in the city. Schooling is whether the city had a Gymnasium by 1580. Waterway is a 0/1 variable for whether the city is on a navigable river, canal, or on the coast in 1850 according to Kunz (2014). Coastal is an indicator equal to one if the city is in the first quartile of distance to the nearest coast and the US price of wheat is below the median. The additional x's (col 2-10) are interacted with decade, 1820s, 30s, 40s, and 50s to allow time-varying impact.

Table D1 City-Pair Variable Definitions

Label	Definition	Source
Price Gap	The absolute value of the percentage price difference of wheat between a city-pair.	See Appendix
Railway Connection	An indicator variable equal to one if a direct railway connection existed between the city pair.	Kunz (2013b)
Railway Cost	The average cost of terrain, on a per kilometer basis, in terms of foregone railway freight capacity.	Based on Nicolls (1878)
Bilateral Distance	The direct distance between a city-pair using the Haversine formula.	From longitudes, latitudes, see below
List's RR Plan	An indicator variable equal to one if there was a railway connection between the city-pair on economist Friedrich List's 1833 plan for a national railway system.	Krause (1887)

Table D2 City-Pair Variable Definitions*

Label	Definition	Source
Population	City population in thousands.	Kunz (2013a), Keyser (1939)
Institutions -- Base	Mean of the indicator variables: (1) guilds were abolished and (2) equality before the law was guaranteed.	Acemoglu, Cantoni, Johnson, and Robinson (ACJR; 2011) and see Appendix
Institutions -- Feudal Lands	Mean of the indicator variables: (1) guilds were abolished and (2) it was possible to redeem feudal lands.	
Institutions -- Broad	Mean of the indicator variables (1) guilds were abolished, (2) equality before the law was guaranteed, and (3) it was possible to redeem feudal lands.	
French Rule	The log number of years the city was under French rule from 1793 to 1815, plus 1	
French Rule Y/N	An indicator variable equal to one if the city was ever under French Rule. At the city-pair level, an indicator equal to one if either city was ever under French Rule	
Latitude	The latitude of the city.	www.wikipedia.com -> city
Longitude	The longitude of the city.	www.wikipedia.com -> city
Distance to Paris (miles)	The direct distance from the city to Paris	from latitudes and longitudes
Protestant Share	Mean of the share of protestants in the city between 1820 and 1880	Keyser (1939)
Schooling	An indicator variable equal to one if the city established it's first Gymnasium before the median city in the sample (1580).	Keyser (1939) gives information on date of first Gymnasium; complemented by city histories and school histories (various sources on the internet)
Coastal	An indicator variable equal to 1 if the city is in the first quartile of distance to the nearest coast and the U.S. price of wheat is below the sample median.	Latitudes and longitudes, and U.S. price as the average of wheat price in New York City, Philadelphia, and Cincinnati; source: Jacks (2006)
Waterway	An indicator variable equal to one if the city is on a navigable river, canal, or on the coast in 1850	From http://www.ieg-maps.uni-mainz.de/mapsp/mapw850d.htm ; Kunz (2014)
Coal Producer	An indicator variable equal to one if the city is in an area with coal production in 1850.	Gutlerbet (2012), Figure 2
Zollverein Membership	Geography based predictor of Zollverein membership: distance of the city to the nearest seaboard.	Keller and Shiue (2013)
1848 Revolution	An indicator variable equal to one if the city was in an area that experienced major 1848 revolutionary activity for the sample year 1850; this was the case for Karlsruhe, Berlin, Dresden, Frankfurt, and Zweibruecken.	Hahn (2001)
War	An indicator equal to one if the city is in an area that was experiencing a major war; Hamburg and Luebeck were affected by the First (1848-51) and Second (1864) Schleswig War. The Austro-Prussian War (1866) and the Franco-Pruissian War affected all cities and is captured by year fixed effects**	

*When city level variables are used in city-pair analysis, the variable becomes the mean across the city-pair unless otherwise noted.

** No big differences in results if only cities in the eastern half (Austro-Prussian War) or Western half (Franco-Prussian War) of Germany are affected.