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THE LINK BETWEEN FUNDAMENTALS AND PROXIMATE FACTORS IN DEVELOPMENT

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ABSTRACT

We examine the role of trade for the impact of institutions on growth. The paper finds that in 19th century Europe, institutional reform improved trade. Another channel that benefited trade was the introduction of steam trains, and institutional reform contributed there as well by making it more likely that railways were run privately, where trade gains were larger than for state-run railways. The main result of the paper is that once we have modeled the institutions impact on growth through trade, there is no other impact of institutions. The approach outlined in this paper is also suitable for studying the link between fundamentals other than institutions and their proximate growth factors.

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1 Introduction

Recent research on growth has focused on fundamental factors, including institutions, culture, and geography. This was preceded by many years of analyzing proximate factors, such as capital accumulation, technical change, or trade. Fundamental factors are important because they are the ultimate drivers, as opposed to manifestations of growth.[1] There is now substantial evidence that fundamentals can have important effects on growth, and we know a lot about the relationship between proximate factors and growth. However, it is much less clear through which proximate factors the fundamentals operate. Arguably, this is crucial for understanding economic development and for designing welfare-enhancing policies. This paper analyzes the link between fundamental and proximate factors in economic development.

Specifically, we examine the role of trade for the impact of institutions on growth. The setting is 19th century Europe. A number of German states experienced drastic improvements of their economic institutions, which we evaluate as a fundamental determinant of development (Acemoglu, Johnson, and Robinson 2005a). Empirical analysis confirms that these fundamentals led to higher growth. Our paper seeks to better understand the mechanisms by addressing two issues. First, we ask how institutions affect proximate determinants of development. The most salient proximate factor here is trade. Institutional change may directly affect trade through improved contract enforcement, for example, and it may affect trade indirectly by changing the speed of adoption of new technology. Second, we quantify the extent to which institutions affect growth through improvements in trade versus through other channels. Figure 1 depicts our approach. Shown on the left side is the impact of institutions on growth. The middle part of Figure 1 depicts institutions and steam trains as determinants of trade. The right side of Figure 1 shows the extent to which the institutions exert their effect on growth through trade (right arrows), versus channels independent of trade (left arrow).

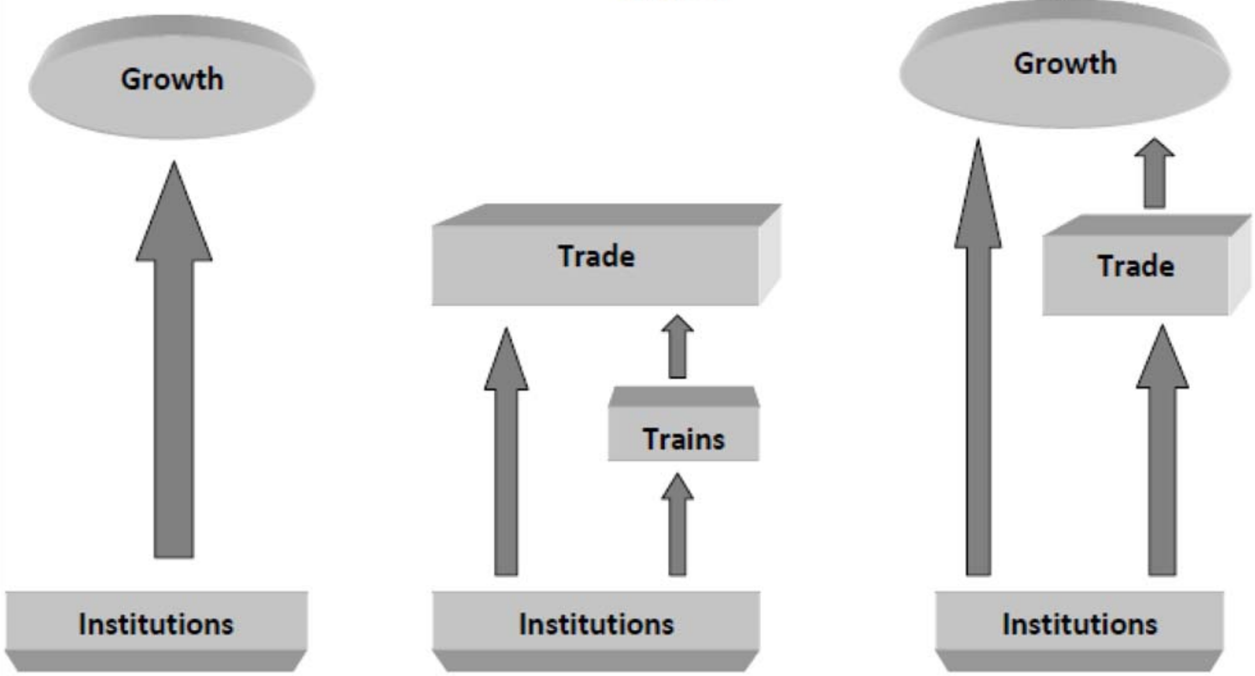
The paper focuses on the adoption of steam railways, the main transport innovation in Germany and other places during the 19th century. Trade is analyzed in terms of spatial price gaps.[2] We show that these gaps came down significantly once two markets became linked by steam train. In addition, through both direct and indirect channels better economic institutions had a strong positive impact on trade; quantitatively, the direct effect is somewhat larger than the indirect effect. Note that our analysis allows for institutional change and steam train adoption to be endogenous, and there is evidence that they were in 19th century Germany.

The main result is that the effect of institutions on growth is predominantly through trade. This means that it has been possible to identify the channel through which fundamental factors affect development in this setting. The analysis takes us one step closer to understanding the interaction between fundamentals and proximate factors that are the mechanisms through

[1] See North (1990), Acemoglu, Johnson, and Robinson (2005a); Weil (2009) is an undergraduate textbook treatment.

[2] Prices give key information on trade and its effects (e.g. Stolper and Samuelson 1941). Both price and quantity measures have been employed in the literature (O'Rourke and Williamson 1999, and Frankel and Romer 1999 respectively). While in historical settings the availability of price data often surpasses that information on quantities, we expect that either would lead to similar results for the questions at hand.

Figure 1



which fundamentals translate into economic outcomes—and it should help to see how economic policy might lead to higher welfare.

Our paper is part of a small literature that links the deep causes of development to proximate factors. Most closely related is Acemoglu, Johnson, and Robinson (2005b) who show that conditional on the initial degree of absolutism across countries, which is taken as given, the onset of Atlantic trade was an important determinant of living standards in post-1500 Europe. Our analysis differs in that we exploit an external shock generating variation in institutions, thereby reducing endogeneity concerns.[3] The quasi-natural experiment approach is complementary to work that endogenizes economic and political institutions (Acemoglu, Johnson, and Robinson 2005a).

There are only few papers that shed light on the relationships between different factors in their effect on development. Our paper is closest to work by Rodrik, Subramaniam, and Trebbi (2004) who show that good institutions can positively affect trade. However, in contrast to us they do not find a major role for trade; this may be explained by differences in the research designs of the studies.[4]

[3] The shock is the French occupation of German states, where we build on Acemoglu, Cantoni, Johnson, and Robinson (2011).

[4] We propose external instruments for trade, including a measure of the cost of railway building imposed by the geography of the terrain. In contrast, Rodrik, Subramaniam, and Trebbi (2004) focus on the gravity-based trade instrument of Frankel and Romer (1999), which as shown by Rodriguez and Rodrik (2001) can fail (Table

Linking fundamental to proximate causes of growth introduces arguably important conceptual and empirical guideposts for tracing out the effect of institutions on development. Other fundamental causes of development that have been emphasized include cultural beliefs (Greif 1994), religion (Weber 1930), and geography (Sachs 2001). Our approach is in principle applicable to all of these factors. At the same time we believe that in the context of 19th century Germany institutional differences were more important than climatic differences, for example. Nevertheless, certain geographic and religious factors play a key role in the analysis below.

This analysis is also part of a small but growing set of papers that rely on sub-national data to study comparative development. As Nunn (2009) notes, this has the advantage that richer identification strategies can be applied and finer mechanisms can be studied than with country-level data. A general concern with sub-national level data is whether the findings generalize to the macro level. In our case there is evidence that the measure of sub-national development, city population growth, exhibits the same broad patterns as GDP per capita at the country level (Acemoglu, Johnson, and Robinson 2005b).

Moreover, the finding of a sizable impact from railways in the 19th century German states parallels Donaldson's (2010) result for a somewhat later period in India; one difference is that military motivations for railroad building were stronger in India compared to the German states.[5] This paper also relates to work on the effect of U.S. railways by Fogel (1964), Fishlow (1965), and more recently Atack, Bateman, Haines, and Margo (2010) as well as Donaldson and Hornbeck (2012). Our work differs in that the roles of rail transportation and trade are seen in the context of the institutional determinants of income growth. Finally, our analysis does not explicitly address the "modernization hypothesis", the idea that economic development spurs the introduction of higher quality institutions (see Barro 2012). While neither the timing of events nor the historical record suggests a strong reverse effect from growth on institutions in this case, we leave this matter to future work.

In the remainder of the paper, the following section 2 provides an overview of institutional setting in Germany under which trade took place, and also discusses the changes that occurred in the course of the 19th century. Section 3 describes the data that will be employed in this study, with more details given in the appendix. All empirical results can be found in section 4. Along the lines of Figure 1, we first estimate the impact of institutional change on growth (left side), followed by an analysis of the causes of trade (middle of the figure) before determining the extent of the growth effect of institutions that operates through trade (on the right side). Some concluding observations are presented in section 5.

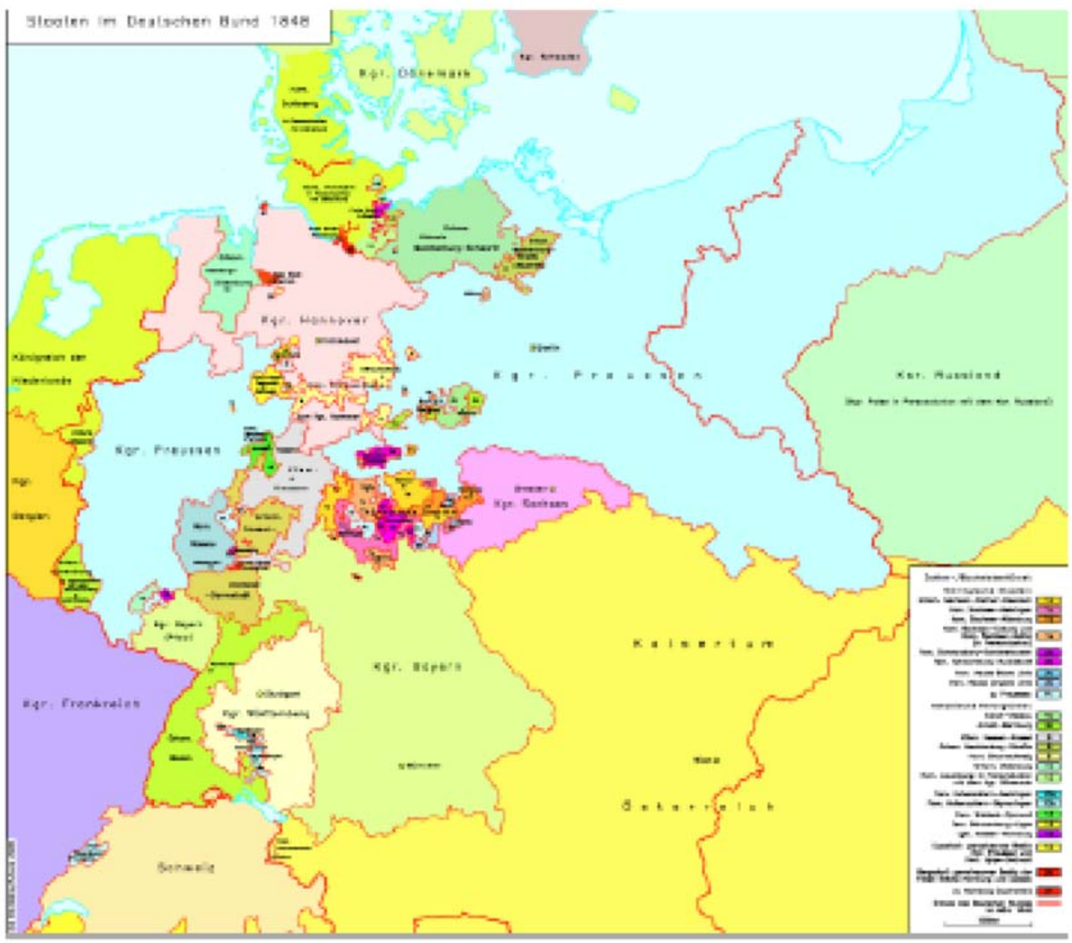
7).

[5] In earlier work we have compared the role of steam trains for trade with that of customs liberalizations in a larger European setting (Keller and Shiue 2008).

2 Nineteenth Century Germany: Institutions and Commodity Markets

During the 19th century Germany consisted– until the formation of the German Reich in the year 1871–of several independent nation states. The larger states included the kingdoms of Prussia, Bavaria and Saxony, while among the smaller states was the Grand Duchy of Hesse-Darmstadt, for example. In addition, there were a number of free cities, such as Hamburg and Frankfurt. Figure 2 shows the territorial boundaries in the year 1848:

Figure 2



While there are several noteworthy aspects of Germany's 19th century development, of key interest for our purposes is the fact that the independent German states were making different choices in terms of the political and economic institutions they adopted, and whether they gave a high priority to railway building or not.

2.1 The adoption of steam railways in Germany

European economic growth from the 19th century on also coincided with a series of innovations in transportation.[6] These innovations included paved roads, improvements in waterways, railways, in materials such as iron and steel, and later on, steam power, but the rapid increase of railway construction was particularly important. In the 1830s and 1840s British suppliers of locomotives dominated the market, and railway iron exports were an important export for Britain, while countries on the continent started to produce their own railway inputs at a later stage.

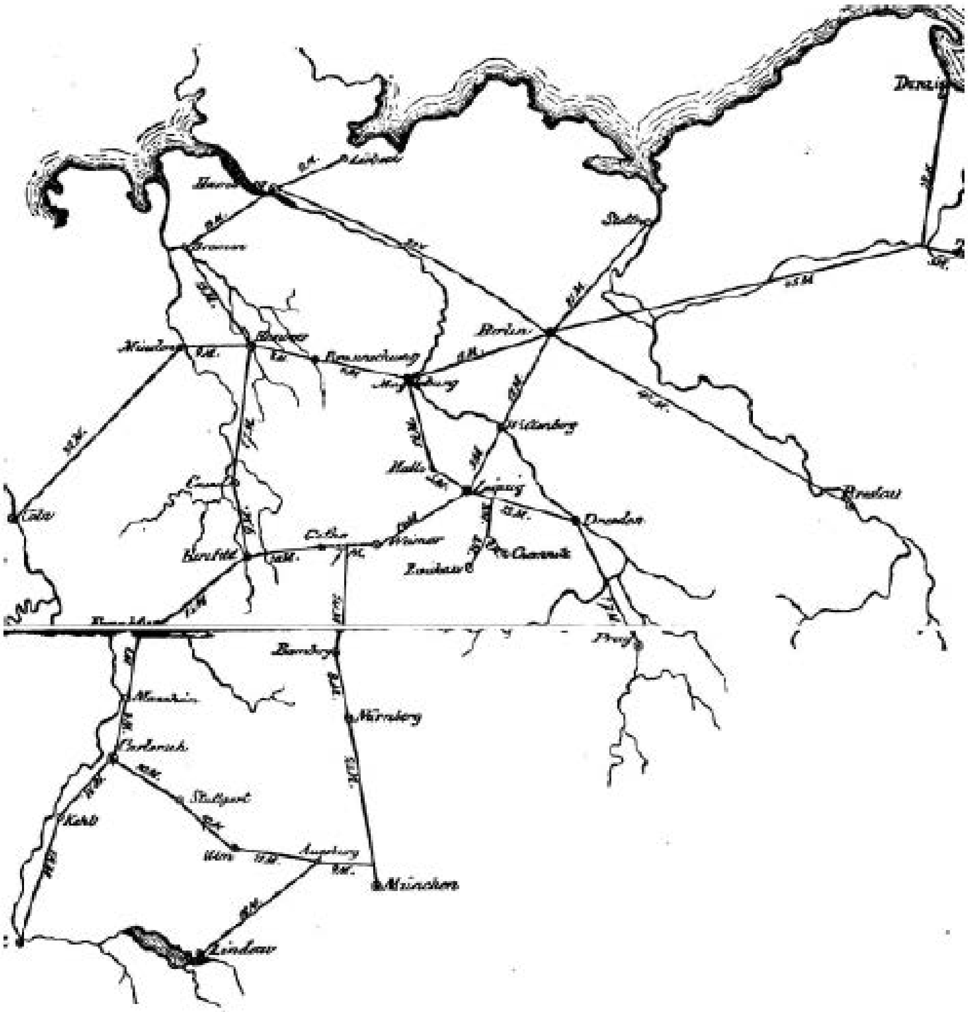
The first German railway was opened in December 1835. With only 4 miles of tracks, it was a short suburban line located in Bavaria, between Nurnberg and Fürth. The first longer route (70 miles) was built in Saxony in 1839. Thereafter, additional miles of rail were laid down swiftly. By 1847, there were over 2,000 miles of rail in Germany (Henderson 1959, 147), and almost all main railway lines were completed by 1877 (Milward and Saul 1977, 42).

What were the relative contributions of private business groups versus the state in this railway building? Most early commentators considered Germany around the year 1830 as so underdeveloped compared to England that it was taken as a given that there would have been insufficient demand for railways to justify private, profit-seeking investments.[7] Consequently it was up to the governments to either establish state railways or heavily subsidize private railways. In fact, the states' early involvement in railways varied strongly, with Hanover, Baden, and, Brunswick, and Wurttemberg building state railways, while Prussia and Saxony initially only had privately run railways.

Moreover, it is quite likely that the state early on did not actually facilitated railway building. In particular, often the state governments slowed down issuing railway concessions, and perhaps most importantly governments heavily regulated in which locations the tracks were eventually built. Each state's government was primarily concerned with its own territory, and connections between major cities in different states rarely existed. This is most clearly seen by comparing the proposal by Friedrich List for a national railway system in Germany from the year 1833 (Figure 3a) with the railway track actually existing in the year 1850 (see Figure 3b).[8]

[6] A good survey is O'Brien (1983).
[7] This account draws on Fremdling, Federspiel, and Kunz (1995).
[8] List's proposal can be found in Krause (1887), pages 24-25.

Figure 3a: List'sches National Railway System Proposal, 1833



While List had planned to connect the larger German points of trade with each other, the actual train system that emerged looked quite different. For example, the major Southern cities of Munich, Stuttgart, and Karlsruhe were still not connected by the late 1840s, most importantly because they were located in three different states (Bavaria, Wurttemberg, and Baden, respectively).[9] Also, the train connection going south from Kiel (in the state of Holstein) for a long time ended in the city of Altona (Holstein), just short of the major town of Hamburg, because Hamburg was an independent state (Free city). The former Hanse town of Luebeck was not yet connected to other cities by rail as of 1850, in part also because it was an independent state.

[9]Eventually, Karlsruhe was connected to Stuttgart by the end of 1853, while Stuttgart was connected to Munich by the end of 1854.

Figure 3b: Germany's railroad system on Dec 31, 1850

These examples illustrate that the early railway policies of states tended to be focused on the own territory. Private proposals to construct inter-state railways already in the 1830s were often rejected, typically for fear of losing trade that originally went through the state's own territory to other states. While in the early phase the state governments slowed down railway building, in a later phase the states engaged in an almost race-like competition to build railway connections. The tipping point was usually reached when a neighboring state had eventually decided to build a railway (or to provide licenses for private operators), because then it was thought that the suspected detrimental, trade-diverting effect could only be prevented by speeding up railway building in the own territories. This second phase has been credited for the fact that railway building in Germany overall has been relatively fast compared to some of its European neighbors, for example the more developed France (Fremdling et al. 1995).

How important were railways as a means of transportation for grain? Generally, railways were important for low value-to-weight ratio good such as coal, construction materials, metal goods, and also grain (O'Brien 1983, 1-2). At the same time, the importance of railroads for transporting grain varied greatly. While it was cheaper to transport grain by railroads than by other means of land transport, trains could not compete with transport by ship. In the late 19th century, for example, sending grain from Posen (in East Prussia) to Cologne by train was at least three times as expensive as shipping it to Rotterdam or Antwerp and then up the Rhine river (Köttgen 1890, 64).

Consequently, long distance grain trade in the southeast direction, parallel to the major rivers (Elbe, Rhine, and Danube), was hardly ever done by rail. At the same time, transportation of grain on railways was of utmost importance when it connected the drainage areas of the main rivers.[10] Grain transportation on railways was also of major significance whenever sea or river transport, even if indirect, was not an option. For example, the great majority of all grain exported from Bavaria to the South in the early 1850s was transported on railways (Seuffert 1857, Chapters 5, 6). The attractiveness of transporting grain on railways was not only affected by geographic features. Also the freight rates per ton-kilometer mattered (Hohorst and Fremdling 1979, 64-65). However, data availability on freight rates is far from complete. As a result, the main information on railway building employed in this study is the time at which two cities became connected by a train connection. We have coded this using maps that show the completion of the train network in Germany on a year-by-year basis (IEG 2013). The train network in Germany for the year 1850 is shown in Figure 3b.

In that year, according to Figure 3b there existed a rail connection between Berlin and Leipzig, but not between Berlin and Munich (München in the map). That had to wait until the segment between Leipzig and Nurnberg had been completed. While this bilateral approach ignores certain effects, for example that the existence of the Berlin-Munich connection implied that Berlin was also connected to Nurnberg (because the Berlin-Munich connection went actually over the city of Nurnberg), it has the advantage of a repeated event-study nature at the same time when it is plausibly capturing the first-order effects. Below we will also investigate the role of third-market and general-equilibrium effects for our results.

2.2 Institutional change in Germany

Many of the most significant institutional changes within Germany were undertaken during the invasion of the French revolutionary armies.[11] In the Rhineland, for example, between 1795 and 1798 the seigneurial regime and the guilds were abolished. Moreover, the institutional changes did not end with the rise of Napoleon to power, because to some extent he continued to implement the reforms initiated by the revolutionary armies. While at times Napoleon was more inclined to compromise with local elites, in most places there was a significant attempt to continue and deepen the reforms brought by the French Revolution. In particular, Napoleon saw the imposition of the civil code (*Code Napoléon*) in areas that he controlled as his most important reform.

After the defeat of Napoleon in the year 1815, the institutional reforms in some German states were kept in place while in others they were rolled back. The key determinant of which way it would go was the presence and strength of new elites that had benefited from the

[10] For example, the completion of the *Köln-Mindener* railway in the year 1847 was crucial for transporting the relatively cheap Prussian grain to the emerging industrial areas of the Rhine-Ruhr (Fremdling and Hohorst 1979, 64). The availability of paved roads and canals also influenced how important steam railroads were.

[11] The reader interested in more detail and references might want to consult Acemoglu, Cantoni, Johnson, and Robinson (2011), on which this section draws.

reforms. Information on the nature and the timing of some of these institutional reforms, in particular (1) the establishment of a written civil code that guaranteed equality before the law and (2) the abolition of guilds is taken from ACJR.

The French civil code (*Code Napoléon*) guaranteed equality before the law, but so did the Saxon civil code of 1863. While equality before the law is bound to reduce discretion and favoritism in business relationships, in itself it may not have an economy-wide effect, and for this reason we see it as a general sign of an efficiency-oriented economic institution more than anything else. Moreover, 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 have not been uniformally opposed to efficiency considerations (e.g. Epstein 1998), on balance we hypothesize that the abolition of guilds is another sign of improvement of the economic institutions in a state. The empirical results reported below are consistent with this hypothesis.

We now turn to a description of the data.

3 Data

The sample consists of forty cities in fourteen German states using their pre-unification boundaries. A list of the cities and states is given in Table 1. There are small cities, such as Parchim in the Northern state of Mecklenburg, as well as some of the biggest cities in Europe at the time, in particular Berlin. In terms of size, Berlin was followed by Hamburg, Munich, and Dresden. The number of cities per state varies, as noted in Table 1.

We use data for the overall period of 1820 to 1880. In order to reduce issues of serial correlation the analysis will rely on data every five years, that is, for the year 1820, year 1825, and so on. This gives a maximum of thirteen data points for each of the cities, however the sample is unbalanced and with generally fewer observations in the last two decades. The reason for the unbalanced sample is primarily missing information on the price of wheat.[12] Table 1 gives the average population growth for each city both for the entire sample period 1820 to 1880 and for years for which each particular city has all data. The correlation between these two series is high, at 0.75. We will examine the influence of the unbalanced data further below.

In central Europe at the time the price of wheat tended to be relatively low towards the south and east in our sample. Table 1 gives summary statistics on how the price of wheat differed between cities. On average, the price gaps for the Mecklenburg city of Rostock were 15%, for example, placing it roughly at the mean of the sample. Further, we know that intrastate trade arbitrage was more effective than arbitrage across different states at this time, even if formal customs borders had been eliminated. Therefore we exclude from the sample observations where both cities belong to the same state.[13]

[12] Population figures are typically benchmarked with the years 1800 and 1850 and estimated using district population data; see the appendix.

[13] In the literature this issue is discussed as the border effect; Shiue (2005) presents evidence for 19th century Germany.

We hypothesize that institutional change in the German states is related to the length of French occupation during revolutionary and Napoleonic times. Table 1 lists the number of years of French occupation that each city in the sample experienced. About a quarter of the sample experienced French occupation, and it was ranging from a minimum of 3 to a maximum of 19 years. Also, French occupation is not a state effect: even though both Cologne and Berlin were Prussian cities, the former was French occupied while the latter was not. Table 1 also lists the dates at which equality before the law and the abolition of guilds was effected in each city. Note that in some cities these changes had been implemented under French influence between 1808 and 1816, only to be reversed afterwards. The computation of the institutions indicator below takes these reversals into account.

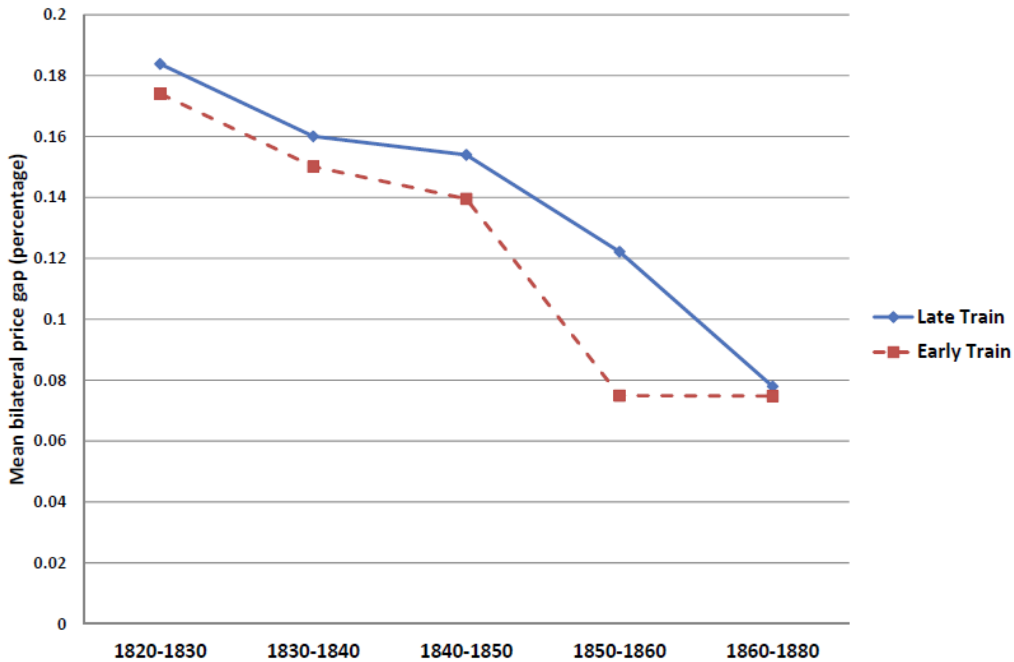
Finally, we employ a railroad construction manual together with GIS methods to estimate the costs of a railway connection between any two cities in the sample. Key determinant was the grade of the terrain, and for this reason our approach is similar to Duflo and Pande (2007) and Nunn and Puga (2012), for example. We employ information on how the performance of a locomotive to haul freight at the time deteriorated as the grade of the terrain increased from Nicolls (1878), see more details in the appendix. Based on this we have constructed a cost function, fed it into a gradient map of the area with 90 x 90 meter cells, and applied the ArcGIS least-cost module to compute the least-cost paths as well as the cost of operating along those paths between each city pair. Our railway cost variable is this cost of railway operation per unit of direct distance, as the crow flies. While the sample, for the most part, does not include highly mountainous areas, there is substantial variation in our bilateral railway cost estimates. For example, the maximum railway costs for connections of Munich is more than 20 times the minimum railway cost (last column), and on average across forty cities this max/min ratio is about five.

We conclude the data overview by presenting some initial evidence on key questions. In Figure 4, we show the price gaps for two types of city pairs, those that established early and those that established late train connections between each other, using the sample median as the cutoff. The figure shows that both sets of city pairs started out in the 1820s with price gaps of around 18%, and slightly lower in the city pairs that would end up building train connections relatively early. Price gaps came down for both sets of city pairs in the 1830s, before in the 1840s and 1850s price gaps fell faster for the city pairs that established relatively early train connections. This difference in timing is what one would expect if trains helped to bring down price gaps. By the end of the sample period, the difference in price gaps between the two sets of city-pairs had evaporated, which is related to the fact that in the years 1875 and 1880 there were train connections between all city-pairs in the sample.

In Figure 5 we show analogously city population developments for the group of city-pairs that implemented institutional change early, versus late. As the measure of institutional change we focus here on the abolition of guilds.

According to the figure, cities that abolished guilds relatively early on had a lead in the 1820 of about 50% over cities that kept guilds longer (about 41,000 to 27,000). By the 1840, this had grown to a lead of 70%, and after 1860 the population advantage of early abolishers of guilds was more than 100%. These figures are consistent with a positive impact of institutional

Figure 4
Price gaps for early versus late train connection market pairs



change on city population.[14] At the same time, this interpretation of Figures 4 and 5 takes the building of train connections and the timing of institutional change as exogenously given. This assumption will be evaluated in the following section. We now move to presenting the results.

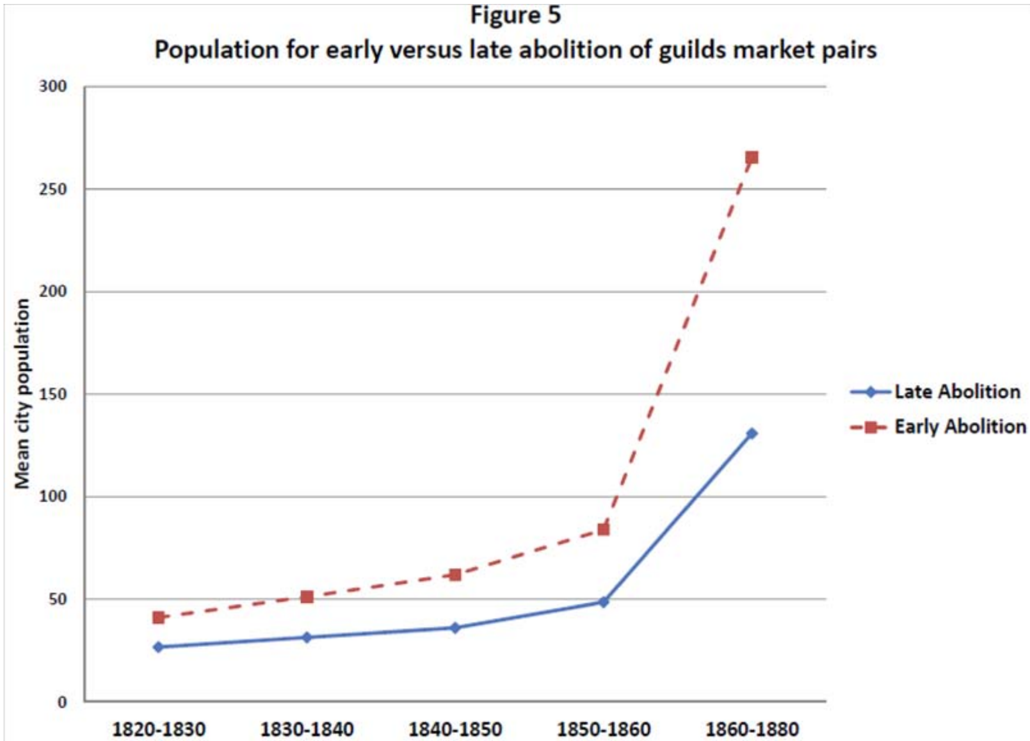
4 Empirical Results

4.1 Institutions and Growth

In this section we evaluate the role of institutional change in fostering growth in German states during the 19th century. In terms of Figure 1 this is seen on the left side. Because for the most part this section replicates results shown by Acemoglu, Cantoni, Johnson, and Robinson (2011), albeit at the level of cities, the analysis will be kept fairly brief, with more results presented in the Appendix.

We are interested in the impact of institutional change on the level of city population. In our bilateral setting, the population of cities j and k in year t is related to the quality of the institutions in cities j and k in year t, $Inst_{jkt}$. This institutions variable captures information on whether in each city in a particular year (1) guilds were abolished and (2) equality before

[14] In both Figures 4 and 5 we focus on city pairs with five or more observations in the sample, to keep the sample relatively balanced.



the law prevailed. Using an indicator for each of the two aspects of institutions and for both cities in a given pair means that $Inst_{jkt}$ ranges between 0 (guilds present and no equality before the law in both cities) and 4 (guilds were abolished and equality before the law was established in both cities), which we scale so that it ranges between 0 and 1.[15]

The estimating equation is given by

$$\log(pop_{jkt}) = \beta_{jk} + \beta_t + \beta_1 Inst_{jkt} + \tilde{\mathbf{X}}'\boldsymbol{\beta} + e_{jkt}, \tag{1}$$

where β_{jk} are city-pair fixed effects, β_t time fixed effects, and $\tilde{\mathbf{X}}$ is a vector of control variables to be defined shortly. The institutions variable $Inst_{jkt}$ is instrumented by the length of French occupation. Consider the following reduced form equation

$$\log(pop_{jkt}) = \gamma_{jk} + \gamma_t + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log\left(fr\_occ_{jk}\right) + \tilde{\mathbf{X}}'\boldsymbol{\gamma} + u_{jkt}, \tag{2}$$

where I_{st} is an indicator variable for each of the three 20-year periods during the sample, 1820 to 1840, 1840 to 1860, and 1860 to 1880, and fr_occ_{jk} is the average length in years of French occupation prevailing in cities j and k, plus one. This specification allows the effect of French occupation to vary with time. The vector $\tilde{\mathbf{X}}$ consists of the bilateral geographic distance

[15] These two aspects of economic institutions are central in our analysis of technology adoption and trade; note, however, that ACJR employ additional measures as well.

between cities j and k interacted with the same 20-year indicator variable I_{st}. To assess the plausibility that French occupation is a valid instrument, we add to equation (2) alternative potential determinants of city population, such as culture or geography, that itself might be correlated with French occupation and therefore could be problematic for identification. This analysis is shown in Appendix A. It turns out that across a wide range of other factors, the length of French occupation is a strong predictor of institutional change in German states. We therefore move to the instrumental variable estimation, see Table 2.

Results for equation (1) are shown in column 1. Beginning with the first stage regression, the results in column 1 indicate that the length of French occupation has a positive impact on institutions. Note that the coefficients for the periods 1820-40 and 1840-60 are quite similar (the excluded period is 1860-80). How strong is the instrument? With the usual heteroskedasticity-consistent standard errors, the first-stage Angrist-Pischke F statistic is about 20 and highly significant. As seen from the top row in Table 2, the two-stage least squares (TSLS) estimate of β_1 in equation (1) is positive with a point estimate of 1.01. This initial result is in line with institutions having a positive impact on city growth.

It is well-known that TSLS is more susceptible to weak-instrument problems than limited information maximum likelihood (LIML), e.g. Davidson and MacKinnon (1993), Angrist and Pischke (2009). Therefore we will report throughout the paper also LIML results. Here, the LIML estimate is virtually equal to one and hence close to the TSLS estimate. What are the estimation results without the IV approach? The simple correlation between city population and institutions is 0.27, and conditional on all covariates the correlation is below zero (see OLS results in column 1). This is consistent with strong attenuation bias. It may also indicate the importance of "defensive" institutional change, as noted in ACJR, which is the idea that even states not occupied by the French started to improve their institutions once these changes became well-known.

We also report estimates when observations are allowed to cluster at the city-pair level, appropriate if there are pair-specific shocks that create dependence over time. The estimates become somewhat less precise and the F-statistic falls but both remain significant at standard levels. Note that the LIML estimate is now lower, at about 0.6.[16] Another approach is to cluster at the level of the city, not city-pair. Some degree of dependence at the city-level must be present, in the sense that if city 1 abolishes guilds in some year this will affect $Inst_{1kt}$ for city-1 observations with *all* cities k. It turns out however that clustering at the city level does not change the inferences, see column 3.

We also examine the influence played by Prussian cities, both because they are a significant part of the sample and because Prussia played a particular role in Germany's 19th century unification process; see column 4 of Table 2. The exclusion of observations with Prussian cities reduces the sample substantially. The first-stage remains strong, and the LIML estimate of the effect of institutions is equal to 0.55, similar to before. It suggests that Prussian cities do not play major role in driving our results.

A number of other robustness checks have been conducted, including size-weighted re-

[16] We apply the Hansen, et al. (2006) LIML estimator which estimates β_1 together with the optimal weighing matrix; the LIML estimates can therefore differ depending on which type of standard errors are computed.

gressions, two-way clustering, and further subsample analysis. These results, discussed in Appendix B, confirm our analysis above. In sum, we find a strong impact of institutions on city growth, in line with the state-level results of ACJR.

We now turn to the determinants of trade.

4.2 Do Steam Trains and Institutions Affect Trade?

In this section we examine the determinants of trade in this sample. We consider in particular two causes, the introduction of steam trains and the improvement of economic institutions. Steam trains generally improved the transportability of high weight-to-value goods, such as coal or grain, over land in 19th century Europe. Our hypothesis is that the introduction of steam trains affected trade through improving spatial arbitrage, leading to lower price gaps between cities. Better institutions might have affected trade through better contract enforcement or by reducing entry barriers. They might also have affected trade indirectly; better institutions may generate a business-minded lobby that has a stake in a more efficient economy. Specifically, institutional change might strengthen a movement that pushes away from an individual state-oriented and inefficient transportation system, or it might increase the rate at which new steam train lines are introduced. In that sense, we consider steam train adoption as a proximate and institutions as a fundamental source of economic performance. In terms of Figure 1 we seek to estimate the relationships in the middle part.

To address the possibility that the establishment of steam train connections was endogenous, we employ a measure of railway construction cost between any two cities as an instrumental variable. The goal is to estimate the causal effect of institutions and steam trains on trade in the following equation

$$TRADE_{jkt} = \beta_{jk} + \beta_t + \beta_1 Inst_{jkt} + \beta_2 Train_{jkt} + \varepsilon_{jkt}, \tag{3}$$

where the dependent variable $TRADE_{jkt}$ is defined as the absolute value of the percentage gap between the price of wheat in cities j and k in year t, $Train_{jkt}$ is equal to one for years during which there was a direct train connection between cities j and k in year t, and zero otherwise, and the institutions variable $Inst_{jkt}$ is defined as above.

As instruments for institutional change we propose the length of French occupation, as discussed above. The instrument for building train connections is a measure of the costs of railway track building per unit of geographic distance, based on differences in terrain across the city-pairs. We expect that a longer period of French occupation, because it leads to institutional improvements, is associated with lower price gaps. Analogously, higher costs of railway building, because it delays new construction, should be associated with higher price gaps. As an initial check on whether this is indeed the case, we have run an OLS regression of $TRADE_{jkt}$ on the French occupation variable, fr_occ_{jk} and time fixed effects. The coefficient on French occupation is -0.007, with a standard error of 0.002. Running $TRADE_{jkt}$ on the cost of railway building, $rail\_cost_{jk}$, and time fixed effects gives a coefficient of 0.014 (standard error of 0.006). These results conform with expectations.

Reduced form results To examine the suitability of these instruments more systematically and using only within city-pair variation, we consider the following reduced form regression

$$TRADE_{jkt} = \gamma_{jk} + \gamma_t + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log \left(fr\_occ_{jk} \right) + \sum_{s=1}^{3} \gamma_{s2} I_{st} (rail\_cost_{jk}) + u_{jkt}, \quad (4)$$

where I_{st} is an indicator variable for each of three twenty-year windows, as above.[17]

The results from estimating (4) are shown in Table 3, column 1. The coefficients on French occupation (fr_occ) are significantly negative, indicating that a long period of French occupation is associated with a lower price gap. This is what one would expect if French occupation leads to institutional improvements that benefit trade. The estimates for the cost of railway variable are negative for the period of 1820 to 1840, and positive (albeit not significant) for the period of 1840-60. This is consistent with $rail\_cost$ being a suitable instrument because one would not expect that the cost of railway has delayed railway building during the 1820-40 period, essentially because steam railways had only just become available (by way of imports from England). The first railway connecting two cities in our sample was opened in 1841. The period of 1820-40 can thus serve as a placebo period. Of primary interest is whether the coefficient on $rail\_cost$ for 1840-60 is higher than that for the 1820-40 period. As seen from the table indeed the point estimate on $rail\_cost$ increases, moving from -0.028 to 0.015.

Given that the $rail\_cost$ variable is derived from data on the difficulty of the terrain between two cities, one might be concerned that it is correlated with other determinants of the price gaps. For example, a mountain range between cities j and k does not only make it difficult to establish a railroad connection, but even before the invention of trains traveling from j to k, by foot or horse, has always been relatively difficult. However, the walking or riding transportation technologies did not substantially change during the sample period, and consequently these effects are captured by the city-pair fixed effects. Identification comes from within-pair variation over time, and in the following reduced form analysis below we will examine a number of factors, such as the introduction of steam shipping, which might exhibit relevant within-pair variation over time. To this end we augment the reduced form with additional factors, X, and check whether this affects the coefficients on fr_occ and $rail\_cost$ in a major way:

$$TRADE_{jkt} = \gamma_{jk} + \gamma_t + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log \left(fr\_occ_{jk} \right) + \sum_{s=1}^{3} \gamma_{s2} I_{st} (rail\_cost_{jk}) + \sum_{s=1}^{3} \gamma_{sx} I_{st} X_{jk} + u_{jkt}.$$

The results are shown in Table 3. As the first variable we consider whether each city is on a national railroad plan proposed by economist Friedrich List in the year 1834. The plan was never implemented due to the state- as opposed to nation-minded interests of the leaders of German states. However, it arguably captures what a well-intentioned social planner would

[17] We also here include bilateral distance interacted with I_{st} into each regression.

have done in order to maximize the national benefit of German railways. As such, it might be correlated with $TRADE$. From column 2 there is no evidence that the List national train plan variables should be included in the regression (p-value for inclusion of 0.81; bottom of column 2). The reduced form coefficients on French occupation and cost of railway building are largely unchanged, and a formal test that all coefficients are equal to zero is rejected (see bottom of column 2).[18]

Next we examine whether initial city size affects the reduced-form relationship. It could be that initially larger cities were on a different trajectory during the 19th century in that they were better able to benefit from improvements in trade than initially smaller cities. There is some evidence that initial city size matters (p-value of 0.046, see bottom of column 3), and the signs on the population-in-1800 coefficients, γ_{sx}, suggest that cities that were smaller in 1800 experienced somewhat faster improvements in $Trade$ than cities that were relatively large in 1800. Including the population variable in the reduced form tends to strengthen the relationship of the instruments with the dependent variable.

What about other determinants of trade? We first explore the influence of culture, in the specific form of Protestantism (Weber 1930 and others). While the correlation of Protestantism with $Trade$ is fairly weak it does reduce the point estimate on railway cost for the 1840-60 period (column 4). Nevertheless, the railway cost coefficient for 1840-60 is higher than during the placebo 1820-40 period, and the null hypothesis that the instruments do not belong into the reduced form is rejected. We also examine whether different trajectories existed for cities in states where the per-capita burden of public debt was relatively high. Column 5 shows that this is not the case, and the reduced form evidence on French occupation as well as railway cost is largely unchanged.

In columns 6 and 7 we turn to geographic factors. While latitude matters little for trade (column 6), longitude enters with negative coefficients, picking up on average lower price gaps (conditional on bilateral distance) in areas further to the east. The coefficients on French occupation and railway cost tend to get stronger with the inclusion of longitude. Another concern is whether the distance to France's capital, Paris, mattered for the length of French occupation. One might expect that proximity favors a long period of occupation, and if this relationship would be strictly proportional, changes in $Trade$ due to French occupation could not be separated from the impact of proximity to Paris. The inclusion of Distance to Paris leads generally to a strengthening of the effects from French occupation and railway cost on $Trade$ (column 8). In particular, conditional on the Distance to Paris, a given length of French occupation is associated with lower price gaps than not holding constant the Distance to Paris.

In addition, we have explored the influence of improved international trade opportunities during the 19th century, where England (as a source of equipment goods) and the United States (as a source of wheat) might be most important. These improvements in international trade should have been most strongly felt in coastal regions, and in column 9 introduce an indicator variable for location near the coast to assess this issue. Coastal location turns out not

[18] Similar results are obtained for another national railway plan that was never implemented, by Hans Grote in 1835.

to be a significant predictor of $Trade$, and the reduced form coefficients for French occupation and railway cost change little. Other transportation changes might be important during this period, in particular the introduction of steam shipping. In column 10 we include a measure of steam shipping into the reduced form, finding that it is not important.[19]

Other infrastructure improvements may have an influence on bilateral price gaps. First among this may be the telegraph, which was widely used in Germany by 1850, especially between larger cities. However, introducing the number of telegrams interacted with city size in 1800 in the reduced form does not much affect the results (see column 11). Finally, we show results on the particular exchange rate regime between cities, since it is possible that the extent to which price gaps came down between cities is related to establishing fixed exchange rates between the different prevailing currencies.[20] As seen from column 12, fixed exchange rates indeed are associated with lower price gaps, however that is largely orthogonal to the effect of French occupation and railway cost on $Trade$.

Overall, across a broad range of factors we find that the proposed reduced form results do not drastically change. In the following we turn to estimating the second stage.

The effects of trains and institutions on trade We now use the French occupation and railway cost variables as instruments in an IV estimation. The structural equation is given by

$$Trade_{jkt} = \beta_{jk} + \beta_t + \beta_1 Inst_{jkt} + \beta_2 Train_{jkt} + \sum_{s=1}^{3} \beta_{s3} I_{st} \log\left(Dist_{jk}\right) + e_{jkt}, \qquad (5)$$

where institutions ($Inst$) and steam train connection ($Train$) are instrumented by the French occupation and railway cost variables. The main results on this estimation are given in Table 4A, while the first-stage coefficients are presented in Table 4B.

According to the two-stage least squares (TSLS) results presented in column 1 of Table 4A, the coefficients on $Inst$ and $Train$ are negative, consistent with an improvement in trade (i.e., reducing price gaps). The first stage for institutions has an (Angrist Pischke) F statistic of around 12, while the trains first-stage F is 25; these are highly significant at standard levels of significance using robust standard errors. We now take a closer look at the coefficients in the first stages, see Table 4B.

The length of French occupation is positively correlated with institutional improvement, as expected, while the cost of railway construction does not matter for institutional change.[21] Moreover, during the period 1840-60 high railway construction cost lowers the probability that a train connection is established. Note that the same is not true during the placebo period of 1820-40. Also interesting are the results for French occupation in the trains first stage. For the period 1840-60, a longer French occupation during Napoleonic invasion times tends to

[19] Due to data availability we are using a national data on steam shipping. However the results are similar when steam shipping is interacted with initial city population, which shows that the introduction of steam shipping did not affect price gaps differently between smaller and larger cities.

[20] In particular before the year 1871, when a common currency, the Reichsmark, was adopted throughout the German Reich.

[21] The omitted period are the years 1860 to 1880.

lower the probability of there being a train connection between any two cities. Thus, there is no evidence that institutional reform has affected trade through speeding up railway building. Also note the negative coefficient on bilateral distance for the 1840-60 period, which simply means that the greater the bilateral distance between any two cities, the lower the probability that there was be a train connection. Overall, the results from the first stages look strong.

Going back to Table 4A, we see that the OLS coefficients for institutions and trains are -0.02 and -0.04, respectively (significant only for trains), which is smaller in absolute value than the TSLS coefficients. This suggests that OLS is upward biased in this case through attenuation bias. From the fact that states primarily focused on their own respective territories it could also be that cities that received train connections relatively early tended to be those for which price gaps fell relatively little (heterogeneous treatment effect). The large difference between the OLS and TSLS estimates in itself points to strong instruments, because weak instruments would push the TSLS towards the OLS estimates (Bound, Jaeger, and Baker 1995). Moreover, the LIML and TSLS results in Table 4A (first column) are quite similar. We also report an F-statistic on the overall strength of the first stages due to Kleibergen and Paap (2006) which is equal to 18.6. This is much larger than the frequently used critical values tabulated by Stock and Yogo (2005).[22] All in all, these results suggest that the equation is identified.

We now turn to results obtained with alternative standard errors. Clustering at the level of the city-pair reduces the first-stage F statistic for institutions to about 4.6 (column 2), while clustering at the level of the city (column 3) reduces it some more without critically affecting significance (at around 1%), and identification remains strong. In column 4 we restrict the sample to gauge the influence of Prussia on these results. As seen from the first-stage regressions in Table 4B, dropping observations with Prussian cities raises the probability of institutional reform for a given length of French occupation. In the second stage, removing Prussian cities leads to a smaller difference in the size of the train and the institutions effect.

A number of additional checks to assess the robustness of these findings are reported in columns 5 to 11 of Tables 4A and 4B. First, we cluster in the time- and the cross-sectional dimension to allow for an arbitrary variance-covariance structure capturing both serial correlation of the residual error terms for a given city-pair and dependence across different city-pairs subject to common shocks not captured by time fixed effects. As seen from the first-stage results, the institutions IVs are still significant at a 10% level, the Kleibergen-Paap (KP) F-statistic is above 30 and the inferences remain unchanged. In column 6 the size-weighted specification gives a stronger influence to the relatively large cities in the sample, and it leads to a somewhat lower (and less precisely estimated) trains effect. This finding may be related to the fact that the IV is larger in absolute value than the OLS estimate, in that it was not necessarily the large, early train adopting cities that experienced the largest reductions in price gaps. For the results in column 7 of Table 4A we have systematically eliminated observations with the highest amount of leverage on the results based on Cook's Distance. This lowers the LIML institutions estimate somewhat whereas that for trains stays roughly unchanged. It

[22] For a 10% tolerable bias, the smallest bias Stock and Yogo (2005) consider, these authors report a LIML critical value with two endogenous variables of 4.72, while for a 20% bias it is 2.99. Note that Stock and Yogo's results are for i.i.d. errors; critical values for non-i.i.d. errors are not available.

appears that leverage favors estimating a relatively strong institutions impact on trade.[23] .

To summarize, the reduced-form analysis has shown that a wide range of alternative determinants of trade do not much affect the estimated relationship between French occupation and railway cost with trade. We take this as evidence that the instruments are valid for estimating causal effects. The analysis has also shown that the instruments are generally strong: TSLS estimates are far from OLS and typically close to LIML estimates, and the first-stage KP F-statistics tend to be relatively high despite demanding assumptions on the data generation process. We find that both train connections and institutional improvements have benefited trade. Quantitatively, the institutions effect is larger than the trains effect, perhaps twice as large. Moreover, the impact of better institutions does not come from the institutional change increasing the pace of train track building.[24]

Then, what is the nature of the institutions effect? In order to better understand the mechanisms we will explore the role of ownership of the railway that was most important for each particular city. While the state always held ultimate control because only the state could issue the necessary licenses, by giving out licenses, in Germany during the 19th century railway companies were organized, financed, and run both by the state and private business groups. In some cases both private and state operation existed even within a given state, for example in Bavaria. In Table 4A we report in column 8a results for cities where the railways were privately operated, while the results in column 8b are for partially or fully state-run operations.

We find that the trains impact on price gaps for privately run railways is considerably larger (in absolute value) than for the full sample, with a point estimate of around -1.0, compared to about -0.20 before. Correspondingly, the trains effect for state-run railways is smaller (in absolute value) than before. How is the institutions estimate affected? It turns out that the institutions effect for privately run railways is also larger than before, although less dramatically so, whereas the institutions variable is insignificant for state-run railways. This suggests that the distinction between state- versus privately run railway is a general indicator of under which circumstances institutional and technological change have the strongest effects on trade. Consistent with that is the result that for privately-run railways, the railway effect is larger (in absolute value) than the institutions effect, whereas this is not the case for state-run railways.

We have also estimated the effect of trains on trade, as well as the effect of institutions on trade separately. In Table 4A we present results from estimating the institutions effect in column 9a while those for trains are shown in column 9b. While individual estimation tends to lead to a somewhat lower institutions and a somewhat higher trains effect, the economic magnitudes from joint and individual estimation are quite similar.

[23] Moreover, as in section 4.1 (Table B1), we have also dropped observations with Bavarian cities, and alternatively, with city state observations from the sample; this does not change the main results.

[24] We do not rely on overidentification tests to assess the validity of the instruments. Such tests essentially ask whether the results using different subsets of instruments are similar. Consequently they are not very useful in the presence of multiple endogenous variables, when different instruments identify specific endogenous variables. Moreover, we have already seen that the railway cost instrument matters when it should (1840-60) but not during the placebo 1820-35 period.

Finally, we are interested in seeing whether the general equilibrium effects that could be missed by our city-pair set-up have a quantitatively large effect on the estimates. There are two sets of results in columns 10 and 11 of Table 4A, that speak to this. First, we show estimates that omit all observations in which both cities are state capitals. As noted in the discussion above (section 2.1), as a first approximation railway building in Germany initially took place on a state-by-state basis, and each state first connected its major cities before railway connections were established between different states. To the extent that these state-specific railway networks are centered on the state capital (hub-and-spoke configuration), we would expect that the building of a connection between state capitals has a larger effect than the average connection in the sample, because it also often helps connecting the relatively smaller cities in each state to each other. As seen from column 10 in Table 4A, the trains coefficient is estimated *higher* in absolute value without the state capital connections, than with them. Arguably, this is the opposite of what one would expect if state capital-to-state capital connections generate large general equilibrium effects in the sense of network benefits for other cities.

Second, we ask whether relatively early- and relatively late-established train connections appear to have a similar effect on trade. It is reasonably to assume that early connections tend to generate a larger network benefit to other cities than connections that are established relatively late. Further, the level of tariffs in Europe between 1850 and 1875 was relatively low. To the extent that transportation costs and tariffs both affect trade costs, we might expect that the reduction of transportation costs through train connections relatively early during our sample period mattered more than at a later date. In column 11 of Table 4A, we present results for the train connections that were established before the year 1858 (roughly 60% of the sample). Confirming these expectations, the train coefficient is now above that for the full sample, although the difference in point estimates is not large (-0.24 for the full sample, and -0.26 for the early sample, compare columns 9b and 11). Overall, while we miss certain general-equilbrium and third-market effects, they appear to be outweighed by heterogeneous treatment effects, as in the state capital analysis (column 10), or they appear to have a limited impact on the results, as the results of column 11 suggest.

We now turn to analyzing the role of trade and institutions in economic growth.

4.3 The link between institutions, trade, and growth

In this section we ask to what extent the growth effect of institutions works through trade, versus through non-trade factors. Recall that we have seen that institutional improvement has a positive effect on city population growth (section 4.1). Moreover, in section 4.2 it was shown that institutional upgrading improved trade.

We are interested in estimating the causal effect of institutions and trade on population size. Key is here that trade itself is affected by institutions, as just shown in the previous section. We are interested in linking the effect on growth of fundamental factor institutions to a more proximate factor, trade. Suppose the relationship between city size, institutions, and trade is given by $\log(pop) = \beta_1 Inst + \beta_2 Trade + u$, while the relationship between institutions and trade can be written as $Trade = \lambda_1 Inst + \mathbf{Z}'\boldsymbol{\lambda} + e$, where $\mathbf{Z}$ are other determinants of

$Trade$, then substituting the latter into the former yields β_1 as the direct effect of institutions, while $\beta_2 \times \lambda_1$ is the indirect effect of institutions on growth. We are interested in the size of these direct and indirect institutions effects.

Reduced form results Because both institutions and trade could be endogenous we need suitable instrumental variables (IVs). The IVs we consider are the length of French occupation and the cost of railway building. Consider the following reduced form

$$\log(pop_{jkt}) = \mathbf{X}'\boldsymbol{\gamma} + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log(fr\_occ_{jk}) + \sum_{s=1}^{3} \gamma_{s2} I_{st}(rail\_cost_{jk}) + u_{jkt}. \qquad (6)$$

Railway cost is a valid instrument if it does not affect population through a channel other than trade. To see how likely this is we will estimate equation (6) augmented with other potential determinants of population and see whether the estimated γ_{s2} substantially change. We already know from the previous section that railway costs are uncorrelated with institutional change, but that could be different for other factors.

The next question is whether French occupation is correlated with other determinants of population other than institutions. In the previous section we have seen that the French occupation variables were highly significant, so if trade affects population size there clearly is room for French occupation to affect population through trade. Does this violate the IV strategy? No, because a better trade environment is in part caused by institutional change, and this is exactly the indirect effect of institutions that we are interested in. The key question is whether French occupation is correlated with trade–or any other possible determinant of population–through non-institutions channels that are not controlled for in equation (6).[25] To find out, we augment the reduced form with other potential determinants of population, building on and extending the analysis in section 4.1 above.

The results from estimating the reduced form (6) are shown in Table 5, column 1. High railway costs are negatively related to city population, especially during the period 1840-60, which is consistent with railway building and trade increasing city size. The length of French occupation is positively related to city size. We augment the reduced form equation (6) with other potential determinants of growth to see whether they eliminate the proposed instruments. Across a wide range of factors these results are given in the remaining columns of Table 5. There are several findings.

First, the influence of railway cost changes relatively little across alternative additional determinants of city size. The largest impact on the 1840-60 railway cost coefficient comes from including population in 1800, which moves it from about -0.13 to -0.09. The French occupation variables are affected somewhat more, in particular by geography and factors capturing the probability of French invasion (longitude, Distance to Paris). This is similar to the earlier analysis without railway cost. It makes sense that railway cost is affected less than French occupation by alternative city size determinants because railway cost is a more

[25] Recall that railway connections tended to be built at a slower pace where French occupation was long (see Table 4B); at the same time, equation (6) controls for this by including the railway cost variable.

specific variable than the length of French occupation. In either case, however, the reduced form coefficients are robust.

Initial population has a negative coefficient, implying convergence in city population, Protestantism is associated with higher city size as argued by Weber (1930), and higher longitude (further east) is associated with lower city size. The latter may pick up that early industrialization and growth in Germany was strongest in the relatively western Rhine-Ruhr area. The telegraph, as a measure of communications and infrastructure improvements, is positively associated with city size, whereas measures such as initial public debt and fixed exchange rate regimes are not significantly correlated with city size.

We now turn to the second-stage results.

Direct and indirect growth effects of institutions In this section we analyze the extent to which institutions affect city size through trade on the one, and through non-trade channels on the other hand. This is based on the following equation:

$$\log(pop_{jkt}) = \mathbf{X}'\boldsymbol{\beta} + \beta_1 Inst_{jkt} + \beta_2 Trade_{jkt} + e_{jkt}, \tag{7}$$

where the vector $\mathbf{X}$ consists of city-pair fixed effects, time fixed effects, and the bilateral distance controls $(\sum_{s=1}^{3} \beta_{s3} I_{st} \log(Dist_{jk}))$. From section 4.2 above we know that trade itself is impacted by institutions. The question is, to what extent do institutions exert their influence on growth through trade, versus independent of trade. In terms of Figure 1, on the right side, we are interested in the direct arrow from Institutions to Growth, versus those that go via Trade. The variables $Inst$ and $Trade$ in equation (7) are instrumented by length of French occupation and railway cost, respectively. Results can be found in Table 6A.

The first column shows a negative TSLS coefficient of about -2.2 for $Trade$. Recall that our measure of trade is the bilateral price gap between two cities, so the negative coefficient says that improvements in trade raise city population. The $Inst$ coefficient is positive, consistent with institutions raising city population, however it is not quite significant at standard levels. The OLS results have a virtually zero correlation between trade and city population while the correlation between institutions and city population is negative at -0.15. Both the large difference between OLS and IV results and the first-stage F statistics, which are both significant at less than 1%, suggests that the instruments are strong.[26]

Table 6B shows the first stage coefficients. The instruments have the expected signs: institutional change is increasing in the length of French occupation, and higher railway cost is associated with lower trade during the railway construction period 1840-60, relative to the placebo period 1820-40. A longer French occupation is also associated with lower price gaps. Column 1 of Table 6A also reports limited information maximum likelihood for equation (7). The LIML effect of trade on population, β_2, is larger than that by TSLS, while the direct institutions effect, β_1, though positive, is close to zero and not significant. This suggests that much of the effect of institutions on growth works through trade.

[26] The overall F statistics proposed by Kleibergen and Paap is about 3.5, which is quite high relative to the LIML critical values that Stock and Yogo (2005) have tabulated for the case of i.i.d. errors.

The following sets of results are for city-pair clustering (column 2) and city clustering (column 3). Neither assumption changes the main inferences: lower price gaps raise city population, and the direct institutions effect, while positive, is close to zero and insignificant. This result is also obtained if we cluster in both the cross-sectional and the time dimension, see column 7. We have also explored the influence of particular German states on the results. Excluding observations with Prussian cities from the sample leads to a significantly positive direct institutions estimate β_1, see column 4 of Table 6A. At the same time, the trade coefficient is estimated is lower. From the first-stage coefficients (Table 6B), it appears that French occupation has a stronger effect on institutional change in non-Prussian cities than in Prussian cities. How does compare with other states? Excluding observations with Bavarian cities leads to a institutions estimate that is borderline significant, though the impact of trade is affected less than in the case of Prussia (column 5). The four city states matter less for the results. To examine this further, we have excluded every one of the cities from the sample and re-estimated equation (7). In every one of these regressions, the trade estimate β_2 is significantly negative at a 1% level or less. The average of the β_2s across these regressions is -3.35. In the case of the institutions effect β_1, the mean is positive at 0.33, however the estimate is only significant in 8% of the cases. Moreover, we have explored the influence of the unbalanced nature of our sample by including only city-pairs which have observations for at least 75% of the sample period. This brings the sample down by about 40%. Then the trade coefficient is -4.19 (p-value of 0.004), while the direct institutions effect is insignificant and close to zero.

The remaining results shown in Table 6A explore the robustness of the findings in a number of dimensions. If we give more weight to the relatively larger cities, this tends to decrease the trade estimate somewhat, while the institutions point estimate increases (column 8), but the latter remains insignificant at standard levels. Eliminating systematically influential observations as measured by Cook's Distance raises the institutions effect even more, but the coefficient remains imprecisely estimated (column 9).

Overall, these results are consistent and point to a significant impact of trade on city population, which as we have seen in section 4.2 above is partly driven by institutional change. The direct, non-trade effect of institutions on city population is typically estimated to be close to zero and insignificant. We can also use the TSLS estimates to assess quantitative magnitudes as follows. In section 4.1 we have estimated the (direct) effect of institutions on population of about 1.0 (column 1). This means that if the typical city pair abolishes guilds (i.e., $\Delta Inst = 0.5$), log city population increases by about 0.5, or about 1,600 people. The average city in the sample has a population of about twenty-nine thousand people, so this means an increase of population of 5.6 percent. How does this compare with the estimated indirect effect of institutions through trade? The same $\Delta Inst$ of 0.5 changes $Trade$ according to Table 4A, column 1 by -0.192. The impact of that on population according to Table 6A, column 2 has a coefficient of about -2.2. Therefore, the indirect effect of institutions through trade is about $(-0.192) \times (-2.2) = 0.42$ log points, or about 1,500 people. This confirms that the trade-mediated institutions effect is close to the entire effect that institutions have on city size.

Given that we may also drop the direct institutions effect $Inst$ from equation (7):

$$\log(pop_{jkt}) = \mathbf{X}'\boldsymbol{\beta} + \beta_2 Trade_{jkt} + e_{jkt},$$

where $Trade$ is instrumented by French occupation as well as railway costs. This estimation leads to the results shown in Table 6A, column 10. The trade effect is estimated somewhat larger when institutions are not included in the equation. This is consistent with the indirect channel of institutions through trade somewhat compensating for the small (and insignificant) direct institutions effect that is left out.

Our finding of an effect of institutions through trade which dominates the direct institutions effect may not be that surprising to some readers. After all, trade is the more proximate source of growth compared to institutions, and one might expect that generally the more proximate explanation of growth will be the more powerful one in a statistical sense. Note that the significance of our findings lies in establishing the link between fundamental and proximate factors, at the same time when we establish their effects on growth in a unified framework.

We now turn to some concluding remarks.

5 Conclusions

In the setting of 19th century central Europe, we have examined the link between institutional change as a fundamental driver of development and more proximate factors, specifically steam train adoption and trade. Institutions have a strong effect on city size, and institutions have also a large impact on improving trade. Institutional improvements do not necessarily increase the speed at which steam trains are adopted, but they make it more likely that trains are privately operated, and that leads to relatively large improvements in trade. Finally, the paper has shown that once the institutions effect on trade is accounted for, institutional change has no independent impact on city size. Put differently, institutions affect growth predominantly through trade.

While the findings are consistent and robust to a range of other factors, at this point we cannot claim that they would carry over quite generally. Further, the analysis was limited to one fundamental and two proximate factors. Some of the reduced form results suggest that going further might produce additional insights, although to estimate causal effects can be challenging enough already with one variable, and in practice there are limits to this approach. At the same time, the paper has suggested a way to include more structure and testable implications that we hope will prove useful in studies of comparative development.

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A The Impact of Institutions: Reduced Form Results

To address the possibility that the institutional changes we consider were endogenous, we exploit the fact that institutional change was driven at least in part by the French expansion into German areas from the years 1794 to 1815. This led to the adoption of institutional reforms, including the abolition of guilds and equality before the law, that for the most part had originated in revolutionary France. While in principle the German states did not have to keep these reforms in place after Napoleon's defeat in 1815, in some cases the reforms did stick, and the extent to which the reforms were in place in German states during the 19th century is positively related to the length of a given state being French-occupied during the wars with France. Denote the length of French occupation during this era of city j as Fr_Occ_j; this is the instrument for institutions.

Is the instrument valid? While exogeneity is certainly plausible given the forced nature of French expansion, it remains possible that French occupation is correlated with other features that affect city size, which would weaken and perhaps prevent identification. In order to assess the likelihood of this in the following we examine the robustness of the reduced form regression. The reduced form, from above, is given by

$$\log(pop_{jkt}) = \gamma_{jk} + \gamma_t + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log\left(fr\_occ_{jk}\right) + \sum_{s=1}^{3} \gamma_{s2} I_{st} \log\left(Dist_{jk}\right) + u_{jkt}$$

Here, the expression I_{st} is an indicator variable for each of three twenty-year windows, denoted s, that span our sample period (1820 to 1840, 1840 to 1860, and 1860 to 1880). The variable fr_occ_{jk} is the log of the average number of years of French occupation in cities j and k plus one, $Dist_{jk}$ is the bilateral distance between cites j and k, the γ_t are time fixed effects, and u_{jkt} is an error term. We include bilateral distance, $Dist_{jk}$, in all specifications given the city-pair nature of our data (the main results are similar when the bilateral distance terms are omitted). Employing this specification allows not only the influence of French occupation to vary over time but it also enables us to include city-pair fixed effects, γ_{jk} that account for time-invariant influences.

The results of estimating equation (2) by OLS are shown in column 1 of Table A1, with 1860 to 1880 being the omitted period. The coefficients on French occupation are positive, and the chi-squared test reported at the bottom gives strong evidence that French occupation belongs in the equation. These results indicate that cities with a longer French occupation have subsequently experienced higher population growth. It is consistent with the idea that French occupation triggered institutional reforms that led to growth. In the following, we add to the reduced form equation (2) other potential determinants, X, of city population, especially those that might be correlated with institutional change:

$$\log(pop_{jkt}) = \gamma_{jk} + \gamma_t + \sum_{s=1}^{3} \gamma_{s1} I_{st} \log\left(fr\_occ_{jk}\right) + \sum_{s=1}^{3} \gamma_{s2} I_{st} \log\left(Dist_{jk}\right) + \sum_{s=1}^{3} \gamma_{sx} I_{st} X_{jk} + u_{jkt}. \tag{8}$$

The first variable is the population of the cities in the year 1800, before the begin of the sample period. This is an important check to see whether the observed population trends are simply due to mean reversion. From column 2 in Table A1, the negative coefficients on population in 1800 give indeed evidence for convergence in city size, and as the test at the bottom of Table A1 indicates the effect is significant. At the same time, the size of the French occupation coefficients is not much affected, and the test statistic for the inclusion of French occupation at the bottom actually increases.

Next, we turn to geographic variables. While the differences in climate or disease environment in this sample are arguably too small to matter, geography may matter for the length of French occupation. Napoleon's push was mainly to the East and South in areas adjacent to France. We include latitude, longitude, and the distance to Paris as alternative variables in equation (8), see columns 3, 4, and 5, respectively. There are smaller coefficients on French occupation with the inclusion of longitude and distance to Paris. This may not be too surprising because higher costs for more distant occupation should reduce the occupation length for areas further east. If this would be strictly proportional it would be impossible to tell whether French occupation as such mattered, versus the ease of conquest of capturing more near by regions. However, the tests show that French occupation remains a strong predictor of population size even with the inclusion of longitude or distance to Paris. Thus French occupation mattered for city population independent of geographic factors.

We also consider culture, which is often seen as another fundamental driver of growth. In particular, we examine Weber's (1930) argument that Protestantism is a major driver of growth. In column 6 of Table A1, the share of the population in the states to which j and k belong is included. Consistent with Weber' argument, a higher share of Protestants is associated with a larger population, however, the relationship between city size and French occupation is largely unchanged.

Differences in the infrastructure might also have led to diverging population trends between the sample cities. For one, some cities might be advantaged relative to others in terms of domestic transport, including but not limited to steam railways, which we will analyze further below. Another key innovation of the 19th century was the telegraph, which was widely used in German states from around the 1850. In column 7 of Table A1, we ask whether cities in areas with a relatively high telegram per capita usage exhibited different population growth from cities that did not, and whether that was correlated with the length of French occupation. As the test at the bottom of column 7 indicates, while a high telegram intensity was positively correlated with city size, this effect does not affect the correlation between city size and French occupation.

Further, in column 8 we examine with the public debt per person whether the state of public finances at the beginning of the sample is associated with different city growth trajectories, as a basic macroeconomic indicator. It turns out that on average high-debt states had higher population growth, and this was largely orthogonal to the length of French occupation. Another factor that features prominently in analyses of the 19th century is the improved *international* integration of markets, in particular between Europe and the United States (O'Rourke and Williamson 1999). The possible impact of access to grain from the United States, or also machinery from England, is explored in column 9. The results show that cities that had because of their proximity to the coast relatively good access to international trade exhibited no drastically different population trajectories from the average city in the sample (and this effect is not strongly related to French occupation). In sum, looking at the test results for the French occupation variables across the different columns of Table A1, for a wide range of factors we find no evidence that suggests the length of French occupation is not a valid instrument for institutions.

B The Growth Effect of Institutions: Additional Results

In this section we discuss additional results for the effect of institutions on city growth, see Table B1. Recall that the estimating equation is given by

$$\log(pop_{jkt}) = \beta_{jk} + \beta_t + \beta_1 Inst_{jkt} + \sum_{s=1}^{3} \beta_{s2} I_{st} \log\left(Dist_{jk}\right) + e_{jkt},$$

where the institutions variable $Inst_{jkt}$ is instrumented by the length of the French occupation. We begin with results that are weighted by the average population of each city-pair so that developments in large cities such as Berlin have a stronger influence on the results than smaller cities, for example Bamberg. As seen from column 1 of Table B1, it turns out that weighting has no major impact on the results. The first-stage is strong, with an Angrist-Pischke F-statistic of around 30, the TSLS estimate is close to 1, and the preferred LIML estimate is about 0.72.

Next, we report results from clustering in both the cross-sectional and time dimension, employing methods proposed by Cameron, Gelbach, and Miller (2008), see column 2 of Table B1. In the first stage regression, the French occupation variables remain significant at a 10% level, and the first-stage F-statistic's p-value is now 7%. In the second stage, the institutions effect is estimated at about 1.1 with the LIML estimator, significant at a 5% level. One would expect lower significance with two-way clustering because allowing for additional dependence in the data is equivalent to less data. If that would be a sign of weak instruments, OLS and TSLS results would be similar (see Bound, Jaeger, and Baker 1995)–but they are not. Also the differences in the estimates compared to one-way clustering are relatively small.

In the remaining three columns of Table B1 we present results from alternative sample restrictions. In column 3, we exclude cities in Bavaria from the sample. Bavaria was the second largest German state at the time, and moreover, Bavarian cities are highly represented in the sample. Excluding Bavarian cities leaves the first-stage coefficients largely unchanged, and the F-statistic is above 28. In the second stage, the LIML estimate of the institutions effect β_1 is somewhat lower compared to the full sample.

We also examine the robustness of the results with respect to the city states in the sample, which are Hamburg, Frankfurt, Luebeck and Bremen). Economic development in these relatively urban areas might proceed quite differently during the 19th century than in area states such as Prussia or Saxony. However, as column 4 of Table B1 shows, the LIML estimate of β_1 is quite close to the corresponding estimate with the full sample. Finally, we have systematically excluded influential points using a standard measure, Cook's distance. The results of this are shown in column 5. Excluding about 7.5% of the data with the most leverage does not qualitatively change the first-stage results, while the second-stage LIML estimate is somewhat higher than with the full sample. All in all, these results suggest that our findings reported in the text are robust.

C Details on Data Sources and Construction

Institutional Change The data on institutional change in Germany during the 18th and 19th century comes from Acemoglu, Cantoni, Johnson, and Robinson (2011). Summary statistics for our sample are shown in Table 1. 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.

Wheat Prices The two most important sources for information on wheat prices are Shiue and Keller (2007) and Seuffert (1857). The former covers markets in Bavaria and Mecklenburg, while the latter provides information on markets 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). Additional sources to include the coverage are Fremdling and Hohorst (1979), Gerhard and Kaufhold (1990) for Prussia, and Vierteljahrshefte (1935) for Berlin, Cologne, Hamburg, Leipzig, and Munich.

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 *Schaeffel*. The conversion factors are taken from the original sources (see Shiue and Keller 2007) as well as from Seuffert (1857). Specifically, from the latter we have (page 351):

State	Quantity unit	Conversion factor into Bav. *Schaeffel*	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

City population Information on the city population size comes mainly from eKompendium-hgisg.de (Kunz 2012). This is combined with data from Bairoch, Batou, and Chevre (1988) and de Vries (1984), used to benchmark in the years 1800 and 1850, as well as the population histories of individual cities and towns (accessed through www.wikipedia.com). The city size data for non-benchmark years (1816, 1850) especially for the smaller towns in the sample are our own estimates. They are based on the population developments at the *Regierungsbezirk* level (roughly, county level), from Kunz (2012). For example, in the case of Prussian cities such as Madgeburg and Aachen, our estimates are based on the official population figures for the *Regierungsbezirke* of Madgeburg and Aachen that were collected during our sample period every three years (in 1822, 1825, etc.). We have also compared our estimates with other sources whenever possible, and there are no differences that would lead to a major change in the estimation results.

Railway data

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 IEG's web site at the University of Mainz, http://www.ieg-maps.uni-mainz.de/. At times a train connection existed but it was highly circuitous and thus hardly the least-cost route. In our coding we assume that in that case the train connection in fact did not exist. Our results are robust to alternative assumptions in this respect within a reasonable range.

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 gives the following data, p.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 and for going at a speed of 8 to 12 miles per hour. This information is for going 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 same 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 tent 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 (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 module 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. Bodies of water (lakes) are blocked out, 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 (using the Haversine formula) to arrive at $rail\_cost_{jk}$, the average gradient cost of terrain between j and k in terms of foregone railway freight capacity. Summary statistics for this data is given in Table 1.

State versus Private Railways We derive the state versus private railways indicator employed in Tables 4A and 4B at the level of each city from the histories of the individual rail companies given in Fremdling, Federspiel, and Kunz (1995) as well as Fremdling (1975). At times a given city is served by railways owned by several railway companies, in which case we classify the city as having a state- or privately owned railway depending on the ownership for the railway company that was most important in the particular city.

Table 1

City	**State**	**Data Range**	**Population (Mean '000)**	**Population (Av growth 1820-80)**	**Population (Av growth sample**	**Wheat Price Gap (Mean abs perc diff)**	**French Occupation (Years)**	**Abolition of Guilds (Year, last time)**	**Equality before law (Year, last time)**	**Railway Cost (Mean)**	**Railway Cost (Max/Min)**
Aachen	Prussia	1820-1860	47.8	0.809	0.860	0.178	19	1795	1804	790329	4.85
Augsburg	Bavaria	1820-1855	33.5	0.341	0.278	0.161	0	1868	1900	879575	1.90
Karlsruhe	Baden	1820-1840	19.7	1.823	1.886	0.261	0	1862	1810	931409	2.79
Bamberg	Bavaria	1820-1855	17.6	0.287	0.140	0.154	0	1868	1900	918738	1.45
Bayreuth	Bavaria	1820-1855	13.2	0.501	0.507	0.154	0	1868	1900	925457	1.59
Berlin	Prussia	1820-1860	369.1	2.853	2.193	0.119	0	1810	1900	749848	2.87
Boizenburg	Mecklenburg	1820-1870	3.2	0.636	0.683	0.147	0	1869	1900	739046	8.15
Braunschweig	Brunswick	1820-1850	36.1	1.473	0.851	0.128	6	1864	1900	859684	6.79
Bremen	Free City	1840-1845	48.3	1.946	1.250	0.179	0	1861	1900	752743	17.85
Dresden	Saxony	1835-1850	84.2	2.350	2.230	0.134	0	1862	1865	854026	2.25
Erding	Bavaria	1820-1855	4.0	1.129	1.245	0.162	0	1868	1900	868282	1.66
Frankfurt	Free City	1840-1845	49.0	2.214	0.318	0.106	6	1866	1900	946409	1.57
Goettingen	Hanover	1820-1865	10.9	0.480	0.264	0.120	3	1809	1900	930056	5.31
Grabow	Mecklenburg	1820-1870	3.4	0.634	0.701	0.133	0	1869	1900	745067	5.96
Hamburg	Free City	1820-1880	203.4	1.841	1.841	0.141	0	1862	1900	703684	10.73
Hannover	Hanover	1820-1850	22.7	3.279	1.776	0.128	3	1809	1900	876265	12.01
Kassel	Hesse-Kassel	1825-1845	33.1	0.594	0.970	0.175	6	1869	1900	973468	5.85
Kempten	Bavaria	1820-1855	7.0	0.985	1.381	0.147	0	1868	1900	872375	2.37
Cologne	Prussia	1820-1880	91.3	1.231	1.231	0.120	19	1795	1804	806150	5.11
Landshut	Bavaria	1820-1855	9.4	0.593	0.600	0.185	0	1868	1900	876861	1.66
Leipzig	Saxony	1835-1880	68.2	2.525	1.153	0.130	0	1862	1865	785432	2.59
Lindau	Bavaria	1820-1855	6.0	1.200	1.750	0.148	0	1868	1900	881423	2.99
Luebeck	Free City	1840-1845	41.7	0.884	0.456	0.144	0	1869	1900	763064	7.05
Mainz	Hesse-Darmstadt	1840-1845	41.6	0.801	0.847	0.224	6	1798	1900	916428	1.67
Memmingen	Bavaria	1820-1855	6.8	0.223	0.076	0.151	0	1868	1900	865485	2.42
Munich	Bavaria	1820-1880	103.2	2.398	2.398	0.124	0	1868	1900	461065	21.15
Muenster	Prussia	1820-1860	21.1	0.425	0.472	0.146	6	1809	1900	1172194	8.36
Noerdlingen	Bavaria	1820-1855	6.7	0.341	0.278	0.193	0	1868	1900	914334	1.79
Nurnberg	Bavaria	1820-1855	50.1	1.518	1.061	0.169	0	1868	1900	915761	1.50
Parchim	Mecklenburg	1820-1870	2.5	0.600	0.660	0.141	0	1869	1900	747159	5.39
Regensburg	Bavaria	1820-1855	21.7	0.178	0.025	0.218	0	1868	1900	888453	1.82
Rostock	Mecklenburg	1820-1870	20.9	0.634	0.701	0.150	0	1869	1900	759509	4.46
Schwerin	Mecklenburg	1820-1870	18.1	0.634	0.701	0.160	0	1869	1900	745803	6.01
Straubing	Bavaria	1820-1855	8.2	0.772	0.907	0.243	0	1868	1900	870977	1.92
Stuttgart	Wurttemberg	1850-1855	49.7	2.732	0.908	0.095	0	1862	1900	976190	4.48
Ulm	Wurttemberg	1850-1855	19.9	0.536	-0.329	0.088	0	1862	1900	926946	7.58
Wismar	Mecklenburg	1820-1870	10.9	0.637	0.721	0.151	0	1869	1900	757602	5.53
Wuerzburg	Bavaria	1820-1855	24.2	0.235	0.316	0.134	0	1868	1900	959884	1.47
Zweibruecken	Bavaria	1820-1855	6.7	0.634	0.681	0.158	19	1795	1804	978455	2.84
Zwickau	Saxony	1835-1850	11.6	1.802	1.546	0.213	0	1862	1865	898095	2.24
Mean			41.2	1.143	0.913	0.155	2.325	1854	1888	854593	4.90
Standard Dev.			64.9	0.842	0.649	0.037	5.23	26.25	29.35	112524	4.34

Table 2: The Effect of Institutions on City Growth

	(1) Robust	(2) Clustered City-Pair	(3) Clustered City	(4) No Prussia
Second Stage: TSLS				
Institutions	1.010	1.010	1.010	0.610
	[<.001]	[0.036]	[<.001]	[<.001]
Second Stage: LIML				
Institutions	1.003	0.622	0.713	0.550
	[<.001]	[0.085]	[<.001]	[<.001]
First Stage				
(1820-1835) x	0.084	0.084	0.084	0.127
Years French Occupation	[<.001]	[0.001]	[<.001]	[<.001]
(1840-60) x	0.086	0.086	0.086	0.128
Years French Occupation	[<.001]	[0.001]	[<.001]	[<.001]
F-statistic	19.58	5.81	52.77	20.51
	[<.001]	[0.003]	[<.001]	[<.001]
OLS				
Institutions	-0.153	-0.153	-0.153	0.201
	[0.029]	[0.087]	[<.001]	[0.014]
Kleibergen Paap F	19.58	5.814	52.77	20.507
City-par Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Clustering		city-pair	city	city-pair
Number of obs	3,570	3,570	3,570	2,814
Number of city-pairs	642	642	642	538
Number of clusters		642	39	538

Dependent variable: log of average population size in city-pair; p-values in parentheses

No Prussia: Observation is dropped if city j is in Prussia

Table 3: Reduced form results for price gap on French occupation and railway cost

	Base	RR Plan List	Pop. In 1800	Culture Protest't	Macro Pub. Debt	Latitude	Longitude	Dist. To Paris	Int'l Trade Coastal	Railroad Freight	Tele-grams	Macro Fixed ER
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1820-1835) x	-0.0264	-0.0263	-0.0277	-0.0243	-0.0277	-0.0293	-0.0298	-0.0325	-0.0284	-0.0286	-0.0307	-0.0257
French Occupation	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1840-1860) x	-0.0219	-0.0219	-0.0232	-0.0224	-0.0218	-0.0230	-0.0275	-0.0266	-0.0231	-0.0224	-0.0250	-0.0236
French Occupation	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1820-1835) x	-0.0282	-0.0280	-0.0286	-0.0259	-0.0288	-0.0287	-0.0267	-0.0259	-0.0283	-0.0290	0.00107	-0.0285
Railway Cost	[0.018]	[0.018]	[0.016]	[0.027]	[0.016]	[0.015]	[0.026]	[0.030]	[0.017]	[0.015]	[0.922]	[0.017]
(1840-1860) x	0.0153	0.0142	0.0197	0.00728	0.0207	0.0180	0.0158	0.0189	0.0168	0.0233	0.0415	0.0128
Railway Cost	[0.268]	[0.314]	[0.162]	[0.612]	[0.142]	[0.210]	[0.259]	[0.185]	[0.236]	[0.112]	[0.005]	[0.352]
(1820-1835) x		-0.00146										
On List Plan		[0.910]										
(1840-1860) x		-0.00647										
On List Plan		[0.581]										
(1820-1835) x			-0.0142									
Population in 1800			[0.031]									
(1840-1860) x			-0.00707									
Population in 1800			[0.299]									
(1820-1835) x				0.000182								
Protestant				[0.703]								
(1840-1860) x				-0.000523								
Protestant				[0.242]								
(1820-1835) x					-0.000515							
Public Debt					[0.077]							
(1840-1860) x					0.000334							
Public Debt					[0.284]							
(1820-1835) x						-0.00864						
Latitude						[0.238]						
(1840-1860) x						-0.00548						
Latitude						[0.435]						
(1820-1835) x							-0.00967					
Longitude							[0.075]					
(1840-1860) x							-0.0146					
Longitude							[0.004]					
(1820-1835) x								-0.000449				
Distance Paris								[0.001]				
(1840-1860) x								-0.000407				
Distance Paris								[0.001]				
(1820-1835) x									-0.0302			
Coastal									[0.149]			
(1840-1860) x									-0.0142			
Coastal									[0.392]			
(1820-1835) x										-0.0734		
Rail Freight										[0.000]		
(1840-1860) x										0.00664		
Rail Freight										[0.722]		
(1820-1835) x											-0.0520	
Telegrams											[0.004]	
(1840-1860) x											0.00641	
Telegrams											[0.748]	
(1840-1860) x												-0.0376
Fixed Exchange Rate												[0.001]
(1820-1835) x	0.0290	0.0293	0.0240	0.0296	0.0274	0.0301	0.0288	0.0298	0.0177	0.0112	0.0289	0.0279
Bilateral Distance	[0.002]	[0.001]	[0.014]	[0.011]	[0.012]	[0.002]	[0.003]	[0.001]	[0.143]	[0.388]	[0.033]	[0.003]
(1840-1860) x	0.0597	0.0595	0.0586	0.0491	0.0619	0.0597	0.0608	0.0599	0.0515	0.0578	0.0642	0.0587
Bilateral Distance	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
R-squared	0.008	0.008	0.009	0.011	0.011	0.009	0.009	0.009	0.009	0.012	0.011	0.010
Test	35.97	34.69	40.33	29.88	41.64	35.97	44.70	57.96	39.68	42.81	31.44	32.87
incl all 4	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Test add'l var		0.42	6.14	8.65	13.10	1.90	8.83	12.31	2.14	41.76	20.19	10.29
inclusion		[0.811]	[0.046]	[0.013]	[0.001]	[0.386]	[0.012]	[0.002]	[0.343]	[<.001]	[<.001]	[0.001]

Dep. Variable: absolute value of percentage price gap; n = 3,570, 642 city-pairs. P-values based on clustering at city-pair level in parentheses

Definitions: see notes to Table A1

Table 4A: The impact of institutions and trains on trade

	1	2	3	4	5	6	7	8a	8b	9a	9b	10	11
	Robust	Clustered city-pair	Clustered City	No Prussia	Two-way clustering	Size weights	No influ'l points	Private RR	Private/ State RR	Only Inst'ns	Only Trains	No State Capitals	Early Con.tns
Second Stage: TSLS													
Institutions	-0.384	-0.384	-0.384	-0.339	-0.384	-0.371	-0.216	-0.580	-0.211	-0.257			
	[0.001]	[0.005]	[0.015]	[0.005]	[<.001]	[0.014]	[0.039]	[0.036]	[0.118]	[0.065]			
Train Connection	-0.166	-0.166	-0.166	-0.264	-0.166	-0.142	-0.082	-1.009	-0.092		-0.231	-0.407	-0.246
	[0.006]	[0.003]	[0.016]	[0.011]	[0.004]	[0.040]	[0.087]	[0.037]	[0.232]		[0.017]	[0.028]	[0.010]
Second Stage: LIML													
Institutions	-0.401	-0.416	-0.401	-0.317	-0.393	-0.390	-0.232	-0.610	-0.215	-0.327			
	[0.001]	[0.005]	[0.014]	[0.006]	[<.001]	[0.012]	[0.034]	[0.039]	[0.118]	[0.029]			
Train Connection	-0.190	-0.186	-0.195	-0.246	-0.173	-0.162	-0.166	-1.059	-0.096		-0.241	-0.437	-0.256
	[0.002]	[0.001]	[0.001]	[0.011]	[0.005]	[0.004]	[<.001]	[0.046]	[0.249]		[0.015]	[0.023]	[0.009]
First Stages													
Institutions F-statistic	12.63	4.64	4.23	9.48	4.91	3.01	5.64	2.24	17.77	3.74			
	[<.001]	[0.003]	[0.011]	[<.001]	[0.174]	[0.030]	[0.001]	[0.126]	[<.001]	[0.033]			
Trains F-statistic	25.00	23.90	20.18	8.13	38.59	14.90	12.28	2.29	4.67		10.12	6.72	13.47
	[<.001]	[<.001]	[<.001]	[<.001]	[0.025]	[<.001]	[<.001]	[0.120]	[0.007]		[<.001]	[0.003]	[<.001]
OLS													
Institutions	-0.021	-0.021	-0.021	0.023	-0.021	-0.032	0.010	-0.024	0.040	-0.012			
	[0.443]	[0.415]	[0.496]	[0.607]	[0.684]	[0.287]	[0.692]	[0.516]	[0.452]	[0.709]			
Trains	-0.041	-0.042	-0.041	-0.051	-0.042	-0.038	-0.019	0.018	-0.045		-0.041	-0.046	-0.080
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[0.003]	[0.034]	[<.001]		[<.001]	[<.001]	[<.001]
Kleibergen Paap F	18.622	17.784	16.247	6.335	31.656	15.03	11.401	2.379	11.752	3.745	10.124	6.724	13.470
City-par Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering		city-pair	city	city	city-pair, time	city	city	city	city	city	city	city	city
Number of obs	3,570	3,570	3,570	2,814	3,570	3,570	3,480	649	2,921	3,570	3,570	3,280	2,147
Number of city-pairs	642	642	642	642	642	642	642	115	527	642	642	642	416
Number of clusters		642	39	39	642, 3	39	39	16	39	39	39	39	33

Dep. Variable: absolute value of percentage price gap for city-pair; p-valuels in parentheses; No Prussia: observation eliminated if city j in Prussia

Table 4B: First-stage coefficients for trade on institutions and trains regression

	1 **Inst'ns** Robust	**1** **Trains** Robust	**2** **Inst'ns** Clust. city-pair	**2** **Trains** Clust. city-pair	**3** **Inst'ns** Cluster city	**3** **Trains** Cluster city	**4** **Inst'ns** No Prussia	**4** **Trains** No Prussia	**5** **Inst'ns** 2-way cluster	**5** **Trains** 2-way cluster	**6** **Inst'ns** Size weights	**6** **Trains** Size weights
(1820-1835) x	0.084	-0.017	0.084	-0.017	0.084	-0.017	0.128	-0.078	0.084	-0.017	0.079	-0.008
Years French Occupation	[<.001]	[0.545]	[0.001]	[0.661]	[0.011]	[0.494]	[<.001]	[0.039]	[0.061]	[0.558]	[0.012]	[0.728]
(1840-60) x	0.086	-0.071	0.086	-0.071	0.086	-0.071	0.130	-0.118	0.086	-0.071	0.081	-0.064
Years French Occupation	[<.001]	[0.016]	[0.001]	[0.076]	[0.011]	[0.005]	[<.001]	[0.005]	[0.059]	[0.111]	[0.013]	[0.013]
(1820-1835) x	-0.001	0.022	-0.001	0.022	-0.001	0.022	-0.012	0.023	-0.001	0.022	-0.001	0.025
Railway Cost	[0.719]	[0.245]	[0.823]	[0.017]	[0.891]	[0.032]	[0.195]	[0.028]	[0.893]	[0.297]	[0.946]	[0.030]
(1840-60) x	0.002	-0.096	0.002	-0.096	0.002	-0.096	-0.010	-0.094	0.002	-0.096	0.002	-0.089
Railway Cost	[0.706]	[0.001]	[0.827]	[<.001]	[0.893]	[0.002]	[0.277]	[<.001]	[0.907]	[0.080]	[0.893]	[0.004]
(1820-1835) x	0.022	-0.035	0.022	-0.035	0.022	-0.035	0.052	-0.020	0.022	-0.035	0.019	-0.035
Bilateral Distance	[0.317]	[0.461]	[0.445]	[0.691]	[0.504]	[0.391]	[0.325]	[0.592]	[0.432]	[0.567]	[0.528]	[0.416]
(1840-60) x	0.025	-0.156	0.025	-0.156	0.025	-0.156	0.054	-0.121	0.025	-0.156	0.022	-0.171
Bilateral Distance	[0.249]	[0.002]	[0.382]	[0.012]	[0.434]	[0.006]	[0.304]	[0.007]	[0.388]	[0.053]	[0.451]	[0.003]
R2	0.188	0.043	0.188	0.043	0.188	0.043	0.320	0.031	0.188	0.043	0.170	0.050

	7 **Inst'ns** No infl'l obs	**7** **Trains** No infl'l obs	**8a** **Inst'ns** Private RR	**8a** **Trains** Private RR	**8b** **Inst'ns** State RR	**8b** **Trains** State RR	**9a** **Inst'ns** Only Inst'n	**9b** **Trains** Only Trains	**10** **Trains** No State Capitals	**11** **Trains** Early Con.tns
(1820-1835) x	0.090	-0.006	0.131	0.003	0.113	-0.053	0.084			
Years French Occupation	[0.003]	[0.851]	[0.049]	[0.957]	[0.009]	[0.001]	[0.009]			
(1840-60) x	0.092	-0.054	0.147	-0.093	0.113	-0.094	0.085			
Years French Occupation	[0.004]	[0.086]	[0.028]	[0.063]	[0.009]	[<.001]	[0.009]			
(1820-1835) x	-0.002	0.018	0.015	-0.018	0.005	0.013		0.015	0.015	0.030
Railway Cost	[0.866]	[0.111]	[0.546]	[0.487]	[0.214]	[0.049]		[0.181]	[0.290]	[0.003]
(1840-60) x	0.004	-0.102	0.018	-0.067	0.006	-0.080		-0.125	-0.105	-0.124
Railway Cost	[0.656]	[0.002]	[0.508]	[0.438]	[0.185]	[0.001]		[0.001]	[0.005]	[0.001]
(1820-1835) x	0.014	-0.010	-0.112	-0.004	0.215	0.058	0.022	-0.079	-0.092	-0.021
Bilateral Distance	[0.590]	[0.802]	[0.260]	[0.971]	[0.008]	[0.330]	[0.511]	[0.044]	[0.057]	[0.394]
(1840-60) x	0.018	-0.133	-0.137	-0.114	0.216	-0.018	0.025	-0.187	-0.173	-0.136
Bilateral Distance	[0.488]	[0.017]	[0.170]	[0.151]	[0.008]	[0.728]	[0.447]	[0.001]	[0.004]	[<.001]
R2	0.180	0.038	0.274	0.092	0.280	0.022	0.188	0.025	0.016	0.032

Dep. Variables: Institutions or Trains indicator, as indicated; p-values in parentheses

Table 5: City Population Size as a function of length of French occupation and railway costs - Reduced form results

		List RR Plan	Pop. In 1800	Prot'nt (Share)	Pub.Debt per cap.	Latitude	Longit'e	Dist. To Paris	Drains to NS, BS	Coastal	Price of wheat US	Tele-grams	Fixed ER
	1	2	3	4	5	6	7	8	9	10	11	12	13
(1820-1835) x	0.114	0.109	0.096	0.111	0.115	0.134	0.059	0.090	0.140	0.103	0.114	0.116	0.115
French Occupation	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1840-1860) x	0.0973	0.095	0.080	0.091	0.098	0.109	0.063	0.090	0.121	0.090	0.097	0.098	0.096
French Occupation	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1820-1835) x	-0.0472	-0.052	-0.054	-0.054	-0.044	-0.045	-0.034	-0.042	-0.042	-0.048	-0.047	-0.043	-0.048
Railway cost	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1840-1860) x	-0.125	-0.107	-0.086	-0.148	-0.122	-0.138	-0.100	-0.105	-0.144	-0.116	-0.125	-0.126	-0.127
Railway Cost	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
(1820-1835) x		-0.144											
On List Plan		[0.002]											
(1840-1860) x		-0.0545											
On List Plan		[0.140]											
(1820-1835) x			-0.197										
Population in 1800			[0.000]										
(1840-1860) x			-0.132										
Population in 1800			[0.000]										
(1820-1835) x				0.01000									
Protestant				[0.000]									
(1840-1860) x				0.00885									
Protestant				[0.000]									
(1820-1835) x					0.00120								
Public Debt					[0.252]								
(1840-1860) x					0.00119								
Public Debt					[0.183]								
(1820-1835) x						0.0663							
Latitude						[0.010]							
(1840-1860) x						0.0516							
Latitude						[0.040]							
(1820-1835) x							-0.139						
Longitude							[0.000]						
(1840-1860) x							-0.0914						
Longitude							[0.000]						
(1820-1835) x								-0.00140					
Distance Paris								[0.052]					
(1840-1860) x								-0.000876					
Distance Paris								[0.196]					
(1820-1835) x									0.367				
Drains to NS, BS									[0.000]				
(1840-1860) x									0.287				
Drains to NS, BS									[0.000]				
(1820-1835) x										-0.173			
Coastal										[0.101]			
(1840-1860) x										-0.0802			
Coastal										[0.271]			
(1820-1835) x											-0.295		
U.S. wheat price											[0.000]		
(1840-1860) x											0.295		
U.S. wheat price											[0.000]		
(1820-1835) x												0.203	
Telegrams												[0.004]	
(1840-1860) x												0.150	
Telegrams												[0.015]	
(1840-1860) x													-0.0307
Fixed Exchange Rate													[0.358]
(1820-1835) x	-0.0725	-0.0659	-0.145	0.103	-0.0546	-0.0769	-0.0537	-0.0630	0.0703	-0.137	-0.0725	-0.00271	-0.0733
Bilateral Distance	[0.022]	[0.045]	[0.000]	[0.008]	[0.123]	[0.009]	[0.069]	[0.037]	[0.003]	[0.026]	[0.022]	[0.956]	[0.021]
(1840-1860) x	-0.0970	-0.0823	-0.134	0.0601	-0.0793	-0.0966	-0.0893	-0.0970	0.0107	-0.143	-0.0970	-0.0379	-0.0978
Bilateral Distance	[0.000]	[0.001]	[0.000]	[0.064]	[0.006]	[0.000]	[0.001]	[0.000]	[0.596]	[0.008]	[0.000]	[0.352]	[0.000]
R-squared	0.107	0.153	0.322	0.173	0.109	0.126	0.247	0.144	0.198	0.122	0.631	0.120	0.108
Tests of inclusion													
4 IVs	63.12	47.49	64.60	85.41	60.51	77.80	37.94	38.82	89.38	48.27	63.12	65.00	67.94
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Test													
add'tl var		29.00	115.06	49.26	2.38	17.10	63.71	19.61	108.54	3.67	69.90	11.37	0.85
		[<.001]	[<.001]	[<.001]	[0.304]	[<.001]	[<.001]	[<.001]	[<.001]	[0.160]	[<.001]	[0.003]	[0.358]

Dep. Variable: average population of cities in city-pair; n = 3,570, 642 city pairs; p-values in parentheses based on clustering at city-pair level

NS, BS: North Sea, Baltic Sea; other variables, see Notes to Table A1

Table 6A: The impact of trade and institutions on growth

	1 Robust	2 Clustering city-pair	3 Clustering city	4 No Prussia	5 No Bava'ia	6 No City States	7 2-way clustering	8 Size weighing	9 No influ'l obs	10 Only Trade
Second Stage: TSLS										
Institutions	0.447	0.447	0.447	0.563	0.550	0.398	0.447	0.322	1.491	
	[0.115]	[0.264]	[0.119]	[0.002]	[0.151]	[0.109]	[0.255]	[0.403]	[0.148]	
Trade	-2.189	-2.189	-2.189	-1.043	-1.714	-1.981	-2.189	-2.485	-1.246	-2.559
	[0.001]	[0.001]	[0.017]	[0.011]	[0.001]	[0.010]	[0.006]	[0.029]	[0.006]	[<.001]
First Stages										
Institutions F-statistic	13.30	3.92	3.71	10.16	3.54	3.93	3.34	16.58	3.87	
	[<.001]	[0.009]	[0.020]	[<.001]	[0.015]	[0.016]	[0.114]	[<.001]	[0.009]	
Trade F-statistic	4.55	5.44	3.79	3.15	5.86	4.82	7.42	8.72	3.37	8.94
	[0.004]	[0.001]	[0.018]	[0.038]	[0.001]	[0.006]	[0.028]	[<.001]	[0.018]	[<.001]
Second Stage: LIML										
Institutions	0.064	0.243	0.096	0.331	0.840	0.197	0.395	0.354	1.246	
	[0.867]	[0.582]	[0.814]	[0.023]	[0.102]	[0.505]	[0.293]	[0.197]	[0.193]	
Trade	-3.666	-3.499	-3.883	-1.582	-2.811	-3.063	-2.058	-2.158	-1.596	-3.983
	[<.001]	[<.001]	[0.001]	[0.001]	[<.001]	[<.001]	[0.006]	[0.001]	[0.022]	[<.001]
OLS										
Institutions	-0.153	-0.153	-0.153	0.200	-0.137	-0.139	-0.153	-0.235	0.063	
	[0.029]	[0.087]	[0.466]	[0.078]	[0.147]	[0.514]	[0.158]	[0.187]	[0.401]	
Trade	-0.006	-0.006	-0.006	-0.008	-0.010	-0.007	-0.006	-0.003	-0.006	-0.005
	[0.619]	[0.582]	[0.531]	[0.427]	[0.382]	[0.407]	[0.564]	[0.759]	[0.417]	[0.597]
Kleibergen Paap F	3.445	4.001	2.843	2.370	4.347	3.591	4.437	2.996	2.673	8.935
City-par Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering		city-pair	city	city	city-pair	city	city-pair, time	city	city-pair	city-pair
Number of obs	3,570	3,570	3,570	2,814	2,535	3,388	3,570	3,570	3,377	3,377
Number of city-pairs	642	642	642	642	465	590	642	642	642	642
Number of clusters		642	39	35	465	39	642,6	39	642	642

Dep. Variable: log of average population of cities (j,k); p-values in parentheses

Table 6B: First-stage coefficients for city population on trade and institutions regressions

	1	1	2	2	3	3	4	4	5	5
	Institutions	**Trade**	**Institutions**	**Trade**	**Institutions**	**Trade**	**Institutions**	**Trade**	**Institutions**	**Trade**
	Robust	Robust	Clustering city-pair	Clustering city-pair	Clustering city	Clustering city	No Prussia	No Prussia	No Bavaria	No Bavaria
(1820-1835) x	0.084	-0.026	0.084	-0.026	0.084	-0.026	0.128	-0.021	0.096	-0.024
Years French Occupation	[<.001]	[<.001]	[0.001]	[<.001]	[0.011]	[<.001]	[<.001]	[0.018]	[0.002]	[<.001]
(1840-60) x	0.086	-0.022	0.086	-0.022	0.086	-0.022	0.130	-0.012	0.098	-0.015
Years French Occupation	[<.001]	[0.001]	[0.001]	[0.001]	[0.011]	[0.002]	[<.001]	[0.298]	[0.001]	[0.016]
(1820-1835) x	-0.001	-0.028	-0.001	-0.028	-0.001	-0.028	-0.012	-0.038	-0.0003	-0.042
Railway Cost	[0.719]	[0.067]	[0.823]	[0.018]	[0.891]	[0.151]	[0.195]	[0.119]	[0.968]	[0.001]
(1840-60) x	0.002	0.015	0.002	0.015	0.002	0.015	-0.010	0.008	0.003	-0.001
Railway Cost	[0.706]	[0.269]	[0.827]	[0.270]	[0.893]	[0.311]	[0.277]	[0.649]	[0.749]	[0.941]
(1820-1835) x	0.022	0.029	0.022	0.029	0.022	0.029	0.052	0.033	-0.009	0.017
Bilateral Distance	[0.317]	[0.018]	[0.445]	[0.002]	[0.504]	[0.062]	[0.325]	[0.016]	[0.837]	[0.135]
(1840-60) x	0.026	0.060	0.025	0.060	0.025	0.060	0.054	0.068	-0.006	0.046
Bilateral Distance	[0.249]	[<.001]	[0.382	[<.001]	[0.434]	[<.001]	[0.304]	[<.001]	[0.887]	[<.001]
R2	0.188	0.008	0.188	0.008	0.188	0.008	0.320	0.011	0.211	0.010

	6	6	7	7	8	8	9	9	10
	Institutions	**Trade**	**Institutions**	**Trade**	**Institutions**	**Trade**	**Institutions**	**Trade**	**Trade**
	No city states	No city states	2-way clustering	2-way clustering	Size weighing	Size weighing	No influ'l points	No influ'l points	Only Trade
(1820-1835) x	0.088	-0.029	0.084	-0.026	0.079	-0.026	0.040	-0.036	-0.026
Years French Occupation	[0.007]	[<.001]	[0.034]	[0.006]	[<.001]	[0.001]	[0.169]	[<.001]	[<.001]
(1840-60) x	0.090	-0.023	0.086	-0.022	0.081	-0.021	0.041	-0.033	-0.022
Years French Occupation	[0.007]	[0.001]	[0.032]	[0.037]	[<.001]	[0.002]	[0.164]	[<.001]	[0.001]
(1820-1835) x	-0.001	-0.031	-0.001	-0.028	-0.001	-0.028	-0.012	-0.015	-0.028
Railway Cost	[0.901]	[0.111]	[0.866]	[0.021]	[0.892]	[0.167]	[0.010]	[0.282]	[0.018]
(1840-60) x	0.002	0.018	0.002	0.015	0.002	[0.006]	-0.011	0.025	0.015
Railway Cost	[0.811]	[0.250]	[0.876]	[0.428]	[0.769]	[0.846]	[0.024]	[0.118]	[0.270]
(1820-1835) x	0.025	0.026	0.022	0.029	0.019	0.028	0.126	0.073	0.029
Bilateral Distance	[0.554]	[0.174]	[0.624]	[0.062]	[0.248]	[0.020]	[0.004]	[<.001]	[0.002]
(1840-60) x	0.030	0.063	0.025	0.060	0.022	0.058	0.127	0.104	0.060
Bilateral Distance	[0.486]	[<.001]	[0.576]	[<.001]	[0.183	[<.001]	[0.003]	[<.001]	[<.001]
R2	0.204	0.010	0.188	0.008	0.170	0.009	0.300	0.008	0.008

Dep. Variable: Institutions or trade, as noted; p-values in parentheses

Table A1: Reduced Form Institutions on French Occupation

	Base	**Size in 1800**	**Geography** Latitude	**Geography** Longitude	**Invasion Probability** Distance to Paris	**Culture** Protestant	**Infrastructure** Telegrams	**Macro Conditions** Public Debt per pop.	**International Trade** Both cities near coast
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
(1820-1835) x	0.103**	0.0873**	0.118**	0.0462*	0.0751**	0.0972**	0.105**	0.105**	0.0908**
Years French Occupation	(2.83e-07)	(0)	(2.77e-07)	(0.0407)	(0.00237)	(2.41e-07)	(1.37e-07)	(3.35e-06)	(1.87e-05)
(1840-1860) x	0.0816**	0.0686**	0.0938**	0.0476*	0.0742**	0.0744**	0.0827**	0.0838**	0.0761**
Years French Occupation	(5.05e-07)	(0)	(6.61e-07)	(0.0105)	(0.000122)	(2.92e-07)	(1.57e-07)	(5.72e-06)	(9.12e-06)
(1820-1835) x		-0.197**							
Population in 1800		(0)							
(1840-1860) x		-0.127**							
Population in 1800		(7.89e-09)							
(1820-1835) x			0.0586**						
Latitude			(5.51e-07)						
(1840-1860) x			0.0530**						
Latitude			(9.65e-05)						
(1820-1835) x				-0.149**					
Longitude				(0)					
(1840-1860) x				-0.0999**					
Longitude				(1.13e-08)					
(1820-1835) x					-0.00176*				
Distance Paris					(0.0123)				
(1840-1860) x					-0.00114+				
Distance Paris					(0.0550)				
(1820-1835) x						0.00892**			
Protestant						(4.41e-06)			
(1840-1860) x						0.00844**			
Protestant						(3.30e-05)			
(1820-1835) x							0.232**		
Telegrams							(3.60e-09)		
(1840-1860) x							0.180**		
Telegrams							(0)		
(1820-1835) x								0.00143**	
Public Debt								(0.00817)	
(1840-1860) x								0.00177**	
Public Debt								(0.000555)	
(1820-1835) x									-0.193*
Coastal									(0.0167)
(1840-1860) x									-0.0607
Coastal									(0.344)
(1820-1835) x	-0.0431	-0.122**	-0.0432*	-0.0299	-0.0373	0.119**	0.0385	-0.0210	-0.112*
Bilateral Distance	(0.0660)	(1.63e-05)	(0.0204)	(0.302)	(0.183)	(0.00238)	(0.278)	(0.442)	(0.0449)
(1840-1860) x	-0.0548	-0.101**	-0.0524**	-0.0554*	-0.0626**	0.101**	0.0139	-0.0318+	-0.0998*
Bilateral Distance	(0.00151)	(0.000701)	(0.000682)	(0.0191)	(0.00101)	(0.00295)	(0.498)	(0.0866)	(0.0393)
Tests of Inclusion									
French Occupation	26.36	71.59	26.41	11.13	25.75	26.99	27.92	21.66	19.69
	[<.001]	[<.001]	[<.001]	[0.004]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Additional variable		96.05	27.68	63.06	15.28	23.92	47.78	14.71	23.90
		[<.001]	[<.001]	[<.001]	[0.001]	[<.001]	[<.001]	[0.001]	[<.001]
Observations	3,570	3,570	3,570	3,570	3,570	3,570	2,985	3,570	3,570
R-squared	0.055	0.301	0.065	0.213	0.109	0.105	0.069	0.061	0.080
Number of city-pairs	642	642	642	642	642	642	439	642	642

Dependent variable: log of average population of cities in city-pair. **/*/+ significant at 1%/5%/10%; p-values based on clustering at city-pair level in parentheses
Initial size: log of the average city population (j,k) in year 1800; Latitude: max. of latitude of cities (j,k) Longitude: max of longitude of cities (j, k);
Distance to Paris: max. geographic distance to Paris (Haversine formula) of cities (j,k); Protestant: average of share Protestant in states of cities (j,k),
from Viebahn (1862); Telegrams: Number of telegrams sent in Germany, from Mitchell (1975); Public Debt per person in states of cities (j,k), from Viebahn (1862);
Near coast: both cities (j,k) located in first quartile of geographic distance to coast of all cities (Haversine formula)

Table B1: The Effect of Institutions on City Growth

	(1) Size Weights	(2) Two-way Clustering	(3) No Bavaria	(4) No City States	(5) No influential Obs
Second Stage: TSLS					
Institutions	1.003	1.010	0.887	0.921	1.226
	[0.001]	[0.022]	[<.001]	[0.035]	[0.030]
Second Stage: LIML					
Institutions	0.718	0.716	0.457	0.587	1.131
	[<.001]	[0.052]	[<.001]	[0.080]	[0.027]
First Stage					
(1820-1840) x	0.079	0.084	0.096	0.089	0.067
Years French Occupation	[<.001]	[0.034]	[0.002]	[<.001]	[0.020]
(1840-60) x	0.081	0.086	0.099	0.090	0.067
Years French Occupation	[<.001]	[0.032]	[0.001]	[<.001]	[0.019]
F-statistic	31.73	4.74	28.81	6.46	8.65
	[<.001]	[0.070]	[<.001]	[0.002]	[<.001]
OLS					
Institutions	-0.235	-0.153	-0.133	-0.139	0.155
	[<.001]	[0.083]	[0.002]	[0.132]	[0.002]
Endog test	1.765	0.947	0.224	8.610	6.735
	[0.184]	[0.330]	[0.636]	[0.003]	[0.010]
Kleibergen Paap F	4.421	4.735	28.214	6.463	2.023
City-par Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clustering	city	city-pair, time	city	city-pair	city
Number of obs	3,570	3,570	2,535	3,388	3,318
Number of city-pairs	642	642	465	590	642
Number of clusters	39	642,6	24	590	39

Dependent variable: log of av. population size in a city-pair; p-values in parentheses