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ABSTRACT

We study the importance of technology and institutions in determining the size of markets in five different countries and fifteen different German states. The setting of 19th century Europe presents a unique opportunity to address this issue, since it witnessed fundamental change in both dimensions. At the beginning of the century, numerous customs borders, separate currencies with different monetary systems, and poor transportation facilities were major obstacles that held back trade. Important institutional change, through the Zollverein customs treaties and currency unification, and major technological innovations in the steam train all had a role in increasing market size as measured in terms of the spatial dispersion of grain prices across 68 markets. However, we find that the impact of steam trains is substantially larger than the effects from customs liberalizations and currency agreements in increasing market size, where correcting for the potential endogeneity in institutional and technological changes are crucial for this result. We also find that a state's institutions influence the rate of adoption of steam trains, thereby identifying an important indirect effect from institutions on economic performance. The institutional and technological changes account for almost all of the decline in price gaps over this period.

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1 Introduction

The growth of Europe, and indeed of the world, has been linked to the scope of market transactions. In fact, it has been said that economic development *is* the spread of markets.[1] As the market size increases, trade raises economic welfare due to the efficiency gains of greater specialization, the division of labor, and potential additional gains through scale economies. Moreover, the welfare gains from increases in market size can be compounded manifold if they usher in an era of higher sustained per-capita growth for an economy.

Institutions that reduce transactions costs are one obvious factor that determines the size of the market, yet little is known about the importance of institutions relative to other means by which market expansion occurs.[2] This paper analyzes these issues by examining the spatial dispersion of grain prices in 68 European markets. These markets are located in five different countries and fifteen different German states, including Prussia. The area corresponds approximately to the location of today's Austria, Belgium, Czech Republic, France, Germany, Italy, the Netherlands, Poland, and Switzerland (See Figure 1). The goal of this paper is to assess the contributions of the key institutional and technological innovations to trade in 19th century Europe, when the scope of markets was increasing at a speed faster than at any point in the past.

The first question we ask, and answer, is how much of an impact did institutional agreements have on market size compared to that of a key transport technology of the 19th century, the steam train. Transactions costs to trade were high in the late 18th century. The steam train dramatically altered the transportation system in which overland transport was often

[1] Braudel (1992, 225).

[2] We borrow North's (1990, 3) notion of institutions; they are "the rules of the game in a society or, more formally, are the humanly devised constraints that shape human interaction".

achieved by horsedrawn carriages. There were about 1,800 customs borders in Germany alone at this time.[3] Moreover, a vast number of different currencies existed, which had the effect of reducing market size even further. Fundamental change arrived in the 19th century. The main mechanism bringing down customs borders was an institution called the *Zollverein*, the "classic example of a customs union".[4] Starting in the year 1828, the *Zollverein* treaties successively liberalized trade among some thirty-five member states that would later become Germany. The first half of the 19th century witnessed also the creation of the first major monetary agreements in Europe. The institutional framework provided by these laws governing commodity and foreign exchange transactions was a clear break from centuries of relatively chaotic conditions, and so one would expect their impact to be large. We show, however, that accounting for the endogeneity of these changes is critical for obtaining proper estimates of the impact of institutions on the size of the market.

Secondly, we consider whether the timing of the adoption of technology could be an indirect outcome of the different institutions that prevailed in 19th century Europe. Over the last several years the literature has emphasized fundamental institutions in bringing about a major impact on economic outcomes (Hall and Jones 1999, Acemoglu, Johnson, and Robinson 2001, Rodrik, Subramaniam and Trebbi 2002). In this line of research, institutions may have a direct effect on economic outcomes, as well as an indirect effect, because they influence the form and rate of technological progress and they shape the incentives of agents (Helpman 2008). We shed new light on this question by examining whether the impact of steam trains on market size is affected either by proximate or by more fundamental institutional factors. As a proximate determinant, we ask whether trains have a stronger impact when they are run by private agents, as opposed

[3] Henderson (1959, 21).
[4] Viner (1950, 97).

to the state.[5] We also examine whether steam trains had a stronger impact on market size in states that abolished serfdom relatively early, hypothesizing that early abolition of serfdom is a general sign of institutional quality.[6]

Our setting has a number of advantages. First, by covering the entire 19th century, we can observe many episodes of change. Moreover, the economies are going through both institutional and technological change, so that the impacts for the same set of economies can be compared. Second, the economies of 19th century Europe also proceeded at different speeds in the implementation of customs, currency, and transportation changes. This is crucial since one can look for corresponding effects on market size at different times across economies. Third, these economies also had different institutional fundamentals, and this allows us to make some headway towards separating the direct and indirect effects of institutions on market size.

We find that both institutional change through currency agreements and customs liberalizations and technological change through the adoption of steam trains were important in increasing the size of the market in 19th century Europe, accounting for almost all of the decline in price gaps. However, the impact of steam trains is found to be larger than the customs and currency effects. We also find support for both proximate and fundamental institutions affecting technological change, thereby providing some initial evidence for indirect effects of institutions via their impact on the rate technological change.

Transportation technology improvements have often been seen as a crucial factor for reshaping the geography of trade. The arrival of more efficient transatlantic shipping led to important

[5] This question is often emphasized in the literature, with the general presumption that private railways would be more efficient; for the case of Germany, see, e.g., Fremdling (1975, 109-132).

[6] Acemoglu, Cantoni, Johnson, and Robinson (2008) study the impact of the French Revolution, showing that the existence of serfdom, as one of the core feudal institutions, is a good indicator for institutional quality, and negatively correlated to economic outcomes.

changes in the late 19th century (Harley 1988, O'Rourke and Williamson 1999), while container shipping and air freight may have increased offshoring during the late 20th century (Hummels 2007). While many writers would agree that steam railways revolutionized transport technology during the 19th century, not all earlier studies bear out the large impact of rails (Fogel 1964, Fishlow 1965, and Williamson 1980).[7] One advantage of our approach to transport technology is that it employs economy pair-specific information on the establishment of rail connections, and moreover, this includes crucial geographic factors affecting steam train availability.[8] This analysis is unique in comparing causal effects from customs liberalization, currency agreements and transportation innovations using economy-specific information on all three mechanisms.

Even if there is general agreement that declining transport costs, both now and in the 19th century, have a bearing on world trade, the recent emphasis has shifted to non-transport cost factors, especially payment frictions (Alesina and Barro 2002, Eichengreen and Irwin 1995, Flandreau and Maurel 2001, Frankel and Rose 2002, and Rose 2000).[9] Several authors have also simultaneously examined, as do we, the impact of customs liberalizations.[10] In this respect, our work is closely related to Estevadeordal, Frantz, and Taylor (2003) and Lopez-Cordova and Meissner (2003), who estimate that bilateral trade in the late 19th century was substantially higher if both partners were signatories of a monetary agreement, the gold standard, than if

[7] Cameron and Neal (2003, 199); they go on to write "[Railroads] were both the symbols and the instruments of industrialization. Before the railways inadequate transportation facilities constituted a major obstacle to industrialization in both continental Europe and the United States."

[8] Estevadeordal, Frantz, and Taylor's (2003) main information on transport cost is Isserlis' (1938) freight cost index, which is based on British rates. For a global sample, this introduces measurement error; as noted in Isserlis, e.g., British ships carried less than 10% of trade between two foreign ports at the time (p.139). Jacks and Pendakur (2008) suggest to combine the maritime trade cost information with overland rates, noting that O'Rourke and Williamson (1994) show that much of the decrease in price differentials between the UK and US after 1850 was due to lower transport costs between the Midwest and the East Coast of the US.

[9] Glick and Taylor (2008) document the disruptive effects of wars on trade. See also Jacks (2005, 2006) who studies a broad range of transportation and non-transportation factors.

[10] On customs liberalizations, see also Baier and Bergstrand (2007), Rose (2004), and Subramanian and Wei (2006).

they were off the gold standard. Our study differs from theirs in that we consider a period starting 70 years earlier. We also conduct an instrumental variable analysis that is compelling, we believe, in picking up on key historical facts to establish that the timing of institutional change in 19th century Europe was systematically related to the economies' specific costs and benefits. The results are substantively different from what comes out of treating institutional change as exogenous.[11]

The remainder of the paper is as follows. Section 2 highlights the convergence of prices in 19th century Europe, and presents preliminary evidence on the importance of institutional and technological change in this. We also provide some historical background for customs liberalization, currency agreements, and steam train adoption during this period. Our data is decribed in section 3. The empirical results on their importance for increases in market size are presented in section 4, where we also report evidence for the impact of institutions on the rate of technological change. The concluding section 5 summarizes our results and suggests directions for future research.

2 Price Convergence in 19th Century Europe: the *Zollverein*, Currency Agreements, and Steam Trains

Consider the European cities Berlin, Brussels, and Munster. The first was the capital of Prussia, and later, of the German *Reich*. The second is the capital of Belgium (founded in 1830), and

[11] Estevadeordal, Frantz, and Taylor (2003) find instrumental variable (IV) estimates that are similar to OLS estimates, while Lopez-Cordova and Meissner's (2003) find no evidence for endogeneity. Ritschl and Wolf's (2003) focus is on showing that a high propensity to trade leads to currency agreements rather than estimating the causal impact of them on trade. For the later part of the 20th century, Barro and Tenreyo's (2004) IV analysis shows that the strong effect of currency unions on trade found in Rose (2000) and others is not subject to endogeneity bias.

Munster was the capital of the Prussian province of Westfalia. Figure 1 shows the location of all three cities on a map. Between Berlin and Munster, customs borders were removed in the year 1831, while between Berlin and Brussels, customs borders remained. Does this customs liberalization explain why between 1830 and 1855, the Berlin-Munster relative price gap for wheat fell by 0.12, while for Berlin-Brussels it fell only by about 0.02?

A priori, it is plausible that customs liberalization did indeed play a role. However, the period 1830-55 witnessed not only customs liberalization, but also, in the year 1848, the arrival of a steam train connection between Berlin and Munster, while a train connection between Berlin and Brussels was not available until 1859. Hence, it would be hard to argue that the difference in price gap reduction of 0.10 (0.12 minus 0.02) is due to customs liberalization alone.

There are also the German cities of Nurnberg, and Parchim. The former is a major city in Bavaria, while Parchim was a smaller town in the state of Mecklenburg-Schwerin, in Germany's far North (see Figure 1). Between Berlin and Nurnberg, it was possible to transport wheat on trains from the year 1851 on, while between Berlin and Parchim, such transport was possible only from the year 1880 on. Does this difference in the transport options explain why between 1830 and 1855, the Berlin-Nurnberg relative price gap for wheat fell by 0.18, while for Berlin-Parchim it fell only by about 0.01?

Trains may have brought down the Berlin-Nurnberg price gap faster than for Berlin-Parchim. However, Berlin and Nurnberg also became members of the *Zollverein* in 1834, while Parchim joined it only in 1867. Moreover, it is surely no accident that trains connected the relatively important cities of Berlin and Nurnberg three decades before it was possible to transport grain by steam train from Berlin to the less important town of Parchim. The marginal contribution from trains on relative price gaps may therefore be different than what these figures indicate.

As a first cut, then, the evidence is consistent with customs liberalization, train transport, and also currency agreements each expanding market size. At the same time, it is also instructive to review what is known about the perceptions of those who lived in these areas at the time.

It was recognized early on that internal trade in Germany was hampered by the multiple customs borders. The economist Friedrich List, head of the Union of Merchants (*der Deutsche Handels- and Gewerbeverein*), expressed this in a petition to the German parliament in the year 1819 as follows: the numerous customs barriers

> "cripple internal trade and produce the same effect as ligatures which prevent the free circulation of blood. The merchants trading between Hamburg and Austria, or Berlin and Switzerland must traverse ten states, must learn ten customs tariffs, must pay ten successive transit dues. Anyone who is unfortunate enough as to live on the boundary line between three or four states spends his days among hostile tax-gatherers and customs house officials. He is a man without country."[12]

The customs situation in Germany was also unfavorably compared with that in other major countries, such as France. In the words of List, the situation is

> "depressing for [German] men who want to act and trade. With envious eyes they look across the Rhine river, where a large nation, from the Canal to the Mediteranean Sea, from the Rhine to the Pyrenees, from the border with the Netherlands to Italy, engages in trade on open rivers and roads without ever encountering a single customs official."[13]

[12]The petition is printed in German in von Eisenhart Rothe and Ritthaler (1934, 320-324).
[13]Ibid.

The support for a removal of customs borders was broad and went beyond merchants, agriculturalists and industrialists.[14] For example, Goethe emphasized both the importance of currency agreements as well as customs liberalization. He said that he would look forward to a time when

> "the German *Thaler* and *Groschen* will have the same value throughout the entire country and my luggage may pass unopened through all thirty-six German states."[15]

There were also some voices opposing economic liberalization in Germany, especially in the early 1800s. They included political progressives, who would however typically not oppose liberalization per se, but liberalization under the leadership of Prussia, which they considered as politically undesirable.[16] These views waned over time, with the increasing recognition that there was no way to German economic liberalization other than under Prussia's leadership.

Also the nobility leading the smaller and mid-sized German states was often hesitant about economic liberalization in Germany, mostly because they feared the possibly accompanying political changes that would result in a loss in their personal power. However, at times the economic imperatives were overwhelming. For example, Ludwig I, the king of Bavaria, strongly supported customs liberalizations with Prussia in the year 1833, because he expected that the economic costs imposed by customs borders would fuel political unrest in the population, thereby leading to a revolution and a loss of his legitimacy (Hahn 1984, 73-75).

The following gives a brief overview of how customs and currency agreements as well as steam trains changed the economic landscape in 19th century Central Europe.

[14] See Henderson (1959), Hahn (1984). This is not to say that merchants, agriculturalists and industrialists everywhere were in favor of customs liberalization. For example, industrial producers in the South German states of Baden, Bavaria, and Wurttemberg complained about the new competition from Prussian and Saxon products after the 1834 *Zollverein* treaty (Hahn 1984, 94).

[15] Goethe in conversation with Eckermann in the year 1828; see Goethe (1828).

[16] Several of the Southern German states had by then adopted constitutions, which Prussia had not.

2.1 The *Zollverein*

The *Zollverein* was the most important institutional development in the move towards trade liberalization in 19th century continental Europe. Other treaties can be found, but none were as encompassing or long-lived. The main economic impact of the *Zollverein* treaties was the abolishment of tariff barriers among member states, and the implementation of a single external tariff for non-members. As of 1815, Germany's political structure was divided into the thirty-nine states of the German Confederation (*Deutscher Bund*), see Figure 1. The confederation consisted of sovereign states in which joint action depended upon unanimity. Austria was initially the most powerful of the German states, followed by Prussia. Individual states tended to be highly protectionist and the tariffs that were imposed were complicated. There is no reliable information on enforcement, but it was likely that it was costly especially for the many small states to each monitor its own borders.

In the aftermath of debts from a decade of war, and new tariffs raised by Britain, Russia, Austria, France, and the Netherlands, Prussia sought to negotiate treaties with her neighbors while reforming internal tariffs. This was particularly pressing because Prussia's territories were divided into two, an eastern portion consisting of seven provinces, and a western portion that included the Rhineland provinces and the Ruhr area. In the year 1818, the Prussian Customs Union was formed. With few exceptions, internal dues were abolished, and by 1821 only a single tariff for the entire Kingdom was levied, while transit dues on goods passing through Prussia were reduced. The importance of the Prussian Customs Union stems from the fact that it served as a model for most of the *Zollverein* treaties that followed.

Enclaves within Prussia were the first to develop agreements with Prussia on how its payment of duties were to be treated—with Prussia deciding to treat the enclaves as her own

territory rather than as foreign states required to pay import duties. As with all of the following treaties, these were based on the principle that states that adopted the Prussian system of tariff received a share of the joint revenue based on population size. Their rights as sovereign states were maintained.[17]

Hesse-Darmstadt was the first territorially separate state to join the Prussian Customs Union in the year 1828, while Hesse-Cassel became the next to join in 1831. The latter was significant because it meant that the East and West Prussian provinces were joined without a customs border for the first time. It also meant that British goods could no longer reach Frankfurt and Germany's south without crossing a Prussian external tariff border; see Figure 1. In the year 1834, the Thuringian states, the Kingdom of Saxony, and the 1828 formed South German Customs Union (consisting of Wurttemberg and Bavaria) joined the augmented Prussian Customs Union to become the German *Zollverein.* At that point the *Zollverein* had an area of about 163,000 square miles and a population of about 23.5 million people.

By stages, other states entered. Three other German states joined the *Zollverein* between mid-1835 and early 1836: Baden, Hesse-Nassau, and the Free City of Frankfurt. The entry of Baden was significant because it meant that the two separate areas of Bavaria were joined without custom borders. The entry of Frankfurt meant that it was possible to trade manufacturing goods from Frankfurt up the Main River to Northern Bavaria in exchange for grain without paying customs duties. Later on, Brunswick became a member of the *Zollverein* in 1841, Hanover in 1851, Oldenburg in 1852, and Mecklenburg as well as the Free City of Lübeck in 1867. Two states became members of the *Zollverein* only after Germany became politically unified in 1871, namely the Free Cities of Bremen and Hamburg in 1888. Thus, the process

[17] Throughout, Prussia reserved the right to negotiate with foreign countries such as France, Belgium, and England for itself.

of customs union enlargement materialized over a large part of the century (the years 1828 to 1888). Austria-Hungary did not become member of the *Zollverein*.

There are a number of possible reasons for states wanting to join the *Zollverein*. Market access was certainly a major motive. For any state considering whether or not to join the *Zollverein,* one advantage was tariff-free access to the large market of Prussia, which included the leading industrial areas of Germany at this time. In general, however, the states located in Germany's South joined the *Zollverein* earlier because not joining implied having to pay hefty tolls in order to reach the Baltic or North Sea coast. This was important, first, because it gave access to trade with the emerging industrial powers, in particular England. Moreover, it was the Southern states' main access to sea ports, since the Alps effectively blocked off trade to southern ports. Thus, the Southern German states of Baden, Württemberg and Bavaria had all joined the *Zollverein* by 1836, whereas the Mecklenburg states, located directly on the Baltic coast, joined only in 1867, and the city states of Hamburg and Bremen, which relied particularly heavily on international trade, joined only in 1888.

Fiscal reasons may well have also been part of the calculus, but it is difficult to find general patterns. For many of the relatively small states, it was prohibitively costly to establish and enforce tariff borders, and they preferred joining the Prussian-led customs union in exchange for a fraction of the joint tariff revenue (Dumke 1976, Chapter 1). At the same time, this cannot be the full explanation since there were several highly indebted and small states that joined the *Zollverein* relatively late. Some of the smaller and mid-sized German states may also have hesitated to join the *Zollverein* because they preferred more trade protection than the external tariff preferred by Prussia provided. However, Prussia's tariffs on a range of goods, especially *Kolonialwaren* such as tobacco, tea, and sugar, were actually higher than the tariffs of other

German states before they joined the *Zollverein*, so the desire for more protection can hardly be the main reason for not joining the *Zollverein*.[18] Other reasons for joining the *Zollverein* were idiosyncratic. For example, Hanover joined relatively late in part because it was governed in personal union with England, which had no interest in an all-inclusive Prussian led customs union in the center of Europe. Overall, the key motive for joining the *Zollverein* was likely market access, and we will return to this question in the empirical analysis below.

Since the *Zollverein* was a customs union, joining it was not identical to a move towards multilateral free trade. Trade diversion was a possible outcome. However, most of trade of the German states at the time was with other German states, and a substantial share of imports were consumption goods that were hardly produced in Europe (such as tobacco, sugar, and spices). Therefore, the trade diversion effect of the *Zollverein* was rather limited, and thus the basic character of the *Zollverein* was trade-liberalizing..

2.2 Currency Agreements

In the first decades of the 19th century, Germany was replete with coins issued by its many different states. The diversity was immense, in sharp contrast to the unified monetary conditions in Great Britain and France, for example.[19] In the Southern states, the dominating currency was the *Gulden*, as it was also called in the empire of Austria-Hungary, while in the Northern states the currency was typically called *Thaler*. Irrespective of the name, each state minted its own currency, and initially currencies did not have legal-tender status outside of a given state. The currencies were linked to silver by the currency unit expressed in equivalent to a certain quantity of silver weighted in Cologne Mark. Comparability of coins even of the same

[18] See Dumke (1976) for more details.
[19] Holtfrerich (1989, 1993).

denomination, like *Gulden*, was difficult because the mints in different states had different coinage fees. This meant that the net silver weight of *Gulden* from different states would actually differ. During the 1820s, the state of Hesse-Nassau for example went as far as to melt down high-silver content coins issued in Bavaria to produce its own low-silver content coins, and pocket the difference (Holtfrerich 1993). The dividing line between full-value specie money and debased coins was therefore fluid.

The South German states put an end to this through the Munich Coin Treaty of 1837.[20] It stipulated that the silver content of the *Gulden* should be the same (nine-tenth of face value), no matter which state minted it. This effectively meant the fixing of exchange rates among the Southern states' currencies from this date on. Importantly, *Gulden* coins minted in any of the Southern states would have legal-tender status in all signatory states. One year later, the Dresden Coin Convention in 1838 effectively led to fixed exchange rates between all *Zollverein* currencies by requiring that each state was obliged to mint coins according to the common metal-content specifications. However, the 1838 Dresden agreement did not give legal tender status to all currencies throughout the *Zollverein*. This created an important barrier to commercial exchange. The Dresden agreement left the Northern *Thaler* bloc and the Southern *Gulden* bloc intact, even though currencies in both blocs were linked to the Cologne Mark at a fixed exchange rate of 1 *Thaler* = 1.75 *Gulden*.

It was recognized at the time that a generally accepted medium of exchange is important for facilitating trade. In fact, the states agreed on the minting of a common coin worth 2 *Thaler* or 3.5 *Gulden* that would have full legal tender status throughout (called the "*Vereinsthaler*"). In part because its denomination was too large for everyday small-scale business, the coin

[20] These Southern states are Bavaria, Baden, Württemberg, Hesse-Nassau, Hesse-Darmstadt, and the Free City of Frankfurt.

never played the role for which it was introduced.[21] Instead, the Prussian one-*Thaler* piece was increasingly used for commercial transactions after 1838, and even gained de facto acceptance in the *Gulden* states of Southern Germany. The need for a generally accepted medium of exchange was remedied only twenty years later, in the Vienna Coin Treaty of 1857, where all *Zollverein* currencies were given full legal tender status throughout the *Zollverein* (even retrospectively to those coins minted between 1838 and 1857). The states that remained outside the Vienna currency agreement of 1857 in our sample are Mecklenburg-Schwerin and the Free Cities of Bremen, Hamburg, and Lübeck (Willis 1896).

Monetary unification was achieved with political unification of Germany soon after the year 1871. The newly created *Reichsmark* had full legal tender status in all German states. Also, Germany moved from the silver to the gold standard after the year 1871, in line with the international trend at the time.[22]

2.3 The Introduction of Steam Trains

European economic growth from the 19th century on also coincided with a series of innovations in transportation.[23] 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

[21] The signatories expected that by the year 1842, the *Vereinsthaler* would account for 1.2% of the total coin circulation in Germany. In fact, the *Vereinsthaler* circulation fell well short of this; Holtfrerich (1993).

[22] In our sample, the Netherlands was on the gold standard by 1875, while Belgium and France were on the gold standard by 1880 (Lopez-Cordova and Meissner 2003).

[23] A good survey is O'Brien (1983). On the debate concerning the contribution of railways in the United States, see Fogel (1964), Fishlow (1965), and Williamson (1980).

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, some 5 years after the initial *Zollverein* treaties came into effect. 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).

Railway building in the five sample countries other than Germany, namely Austria-Hungary, Belgium, France, the Netherlands, and Switzerland, proceeded in quite different ways. In France, railway construction began as early as 1828 with 23 kilometers of track opened, but its pace fell behind that of Germany in part because of resistance to the new technology from owners of other means of transportation. It has been argued that railway building in Germany has been particularly fast because the various politically independent states competed for transport routes through their territitories (Fremdling et al. 1995). At the same time, railway building in Belgium was also very swift. The Belgium railways were designed as a means of international transport from the beginning. This meant that negotiations among different states were necessary. In 1834, the Belgium Parliament planned for a network that allowed connections to Prussia, France, England, and the sea at Anvers, and later, an extension to Holland (Laffut 1983). In Switzerland, both the difficult geography as well as the highly federalistic (cantonal) system slowed down railway building. Also in Austria-Hungary, railway building proceeded at a moderate pace; major reasons for that include relatively little interest in the new technology among the empire's leaders, as well as empty state budgets and lost wars starting around the mid-19th century.

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.[24] 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 Switzerland 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, and while we do not have fully detailed information on this, we know that they differed both across states as well as over time (Hohorst and Fremdling 1979, 64-65). The existence of a train connection therefore does not say everything on the importance of a particular train track for grain trade. The significance of railways for grain trade varied for a number of reasons between different market pairs, and the estimation will take this into account.

[24] 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). At the same time, the availability of paved roads and canals also influenced how important steam railroads were.

We now turn to a description of the data.

3 Data

This study employs the price for wheat across markets in Europe to analyze trade and the size of the market. We have compiled a data set consisting of sixty-eight market locations; Table 1 provides an overview. There are 16 markets, or about 24% of the sample, in Austria-Hungary, Belgium, France, the Netherlands, and Switzerland. The remaining 52 wheat price series are for markets located in fifteen different German states.[25] The prices are averages for an entire year, which is appropriate since we are interested in low-frequency changes of price gaps over an entire century. All prices are quoted in terms of Bavarian *Gulden* per Bavarian *Schaeffel* (about 223 liter of wheat). To arrive at a comparable set of prices we have converted the many different quantity and monetary units that were used in 19th century Europe using the conversion rates given in Seuffert (1857) as well as in the original sources.[26]

The overall sample period is 1800 to 1899, but data availability varies greatly across the series. For example, there are all 100 annual price observations for the city of Brugge during the 19th century, while for the market in Wiesbaden, there is only one single observation. Since the goal is to rely on important time-series variation (before-after comparison), it is clear that

[25] These German states are the Grand Duchy of Baden, The Kingdom of Bavaria, the Duchy of Brunswick, the Free City of Bremen, the Free City of Frankfurt/Main, the Free City of Hamburg, the Free City of Lübeck, the Kingdom of Hannover, the Electorate of Hesse-Cassel, the Grand Duchy of Hesse-Darmstadt, the Duchy of Hesse-Nassau, the Grand Duchy of Mecklenburg-Schwerin, the Kingdom of Prussia, the Kingdom of Saxony, and the Kingdom of Württemberg. Some of these territories changed their name during the 19th century, for instance the Kingdom of Hannover, which was an Electorate until 1814. All of these territories became part of the German *Reich* after the year 1871.

[26] The rates of inflation might have differed across markets, but information on inflation rates or exchange rates for all currencies and all years are unfortunately not available. Both the levels as well as differences in inflation rates across states have tended to fall over the 19th century, and the currency agreements have likely played a role in this. We include state-pair fixed effects in the analysis to reduce any bias.

more weight should be placed on markets where prices are observed for a long time. Table 1 reports the number of observations for each market as well as the year of the earliest price observation during the 19th century.

Grain prices in Europe at the time generally increased from the South (the Black Sea area) and East (Eastern Prussia) to the Northwest (Northern Germany, the Canal region, and England). The average percentage price gap between two markets in our sample is about 0.18 over the entire 19th century. For the subset of price gaps for which we have information for the entire century, the average price gap in the first decade of the 19th century is about 0.32, while in the last decade of the 19th century it is, with 0.10, less than one third of that. This decline reflects that dramatic extent of price convergence over the 19th century.

To understand the roles of customs liberalization, currency agreements, and steam railways in price convergence, we coded data on each. For each city-market, we have recorded the year in which it became part of the *Zollverein*; this year is listed in Table 1.[27] Important accession dates are 1834 and 1836, as well as the years 1841 (Brunswick), 1854 (Hanover), 1867 (Mecklenburg and Lübeck), and 1888 (Bremen and Hamburg). Generally, joining the *Zollverein* meant that barriers for grain trade between any two of its markets would be equal to zero. Unfortunately, there is no comprehensive information on the levels of tariffs on grain before liberalization. Some available figures suggest that the duties on wheat may have been on average the equivalent of about 10 percent *ad valorem*.[28] Instead of exploiting the size of the tariff change, we rely on the timing of the move towards zero trade barriers through *Zollverein*

[27] Customs liberalizations that did not involve *Zollverein* accession are discussed in section 4.

[28] At the time, mostly specific duties were charged, so that the *ad valorem* duty varied with the price of wheat. In the year 1831, the augmented Prussian customs union charged a specific duty equivalent to about 7% for wheat. The *ad valorem* equivalents for "products of agriculture" before the formation of the Zollverein in 1834 were about 16% in Prussia, 9% in Bavaria and Wurttemberg, 8% in Baden, and 3% in Saxony (Dumke 1976, Tables 3.16, 3.17).

membership.[29]

Even though within states tariffs were generally abolished in the very early 1800s, there could still have been customs borders faced by agents trading within the same state. This is in part because the territory of several states consisted of several non-contiguous parts, such as the Eastern and Western provinces of Prussia, or the Bavarian Palatinate area that was separate from core Bavaria around Nurnberg and Munich. For each market pair in our sample, we have established using historical maps whether a direct trade route would involve passing any customs borders. If the number of customs borders to be crossed is greater than or equal to one, CU_{ijt} is coded as 0, otherwise it is 1, for each market pair ij and year t. For any relationship between a German and a non-German market, or between two markets in different European countries, CU_{ijt} is equal to 0 for all years. For example, the customs variable CU_{ijt} turns to 1 for the pair Berlin-Nurnberg in 1834 (the year of the *Zollverein* foundation), it changes from 0 to 1 for Berlin and Parchim in 1867 (with Mecklenburg-Schwerin's *Zollverein* accession), and between Berlin and Brussels, CU_{ijt} takes on the value 0 throughout the sample period (see Figure 1).

Turning to monetary agreements, a major step was for currencies to have full legal tender status in other states. As discussed above, this occurred between the Southern states in 1837 with the Munich Coin Treaty. For all *Zollverein* currencies, full legal tender status was agreed upon with the Vienna Coin Treaty of 1857. Thus, for example, the variable LT_{ijt} for the pair of Munich (in Bavaria) and Stuttgart (Wurttemberg) up to the year 1837 is equal to 0 and 1 afterwards. In contrast, the variable LT_{ijt} for the pair Berlin (Prussia) and Stuttgart is 0 up to

[29] In a few cases, the time of the *Zollverein* accession does not coincide with the year in which tariffs on grain were eliminated. For example, the tariffs between Bavaria and the augmented Prussian customs union were eliminated in 1829, four years before the initial *Zollverein* treaty. We focus nevertheless on the *Zollverein* accession date, because this played the key role in terms of commitment.

the year 1857, and 1 afterwards. For relations between a German and a non-German market, LT_{ijt} is always 0. Table 1 gives the year in which the currency used in a particular city had for the first time full legal tender status in another state.[30]

Table 1 gives also the year in which a particular city-market had its earliest bilateral rail connection in our sample. For example, the rail track between the Saxony cities of Dresden and Leipzig was completed in the year 1839, and since this was the earliest connection in the sample for both cities, Table 1 lists this year for Dresden and Leipzig. The trains variable TR_{ijt} for the Dresden-Leipzig pair is 0 until the year 1839, and 1 thereafter.

This coding is not based on when a particular city became part of the railway network by getting its railway station, which in itself may not be particularly important. Instead, we code the TR variable specific to bilateral connections in our sample. Moreover, since it clearly matters for the choice between different modes of transport how circuitous the route between two markets is, we have set TR_{ijt} only equal to one once a direct and non-circuitous train connection existed. This has been determined by analyzing historical maps that give the precise geographic location of the historical train tracks in Europe. For example, Figure 2 shows the train connections in the year 1850, as well as the four cities Strassbourg, Munich, Hamburg, and Cologne. In the following year, 1851, the North-South connection between Munich and Hamburg was established, and the variable TR_{ijt} switches from 0 to 1 in our analysis.

The TR variable also incorporates other relevant elements of Europe's topography, such as the existence of bridges across rivers. For example, the railway line between Cologne and

[30] The two Alsatian cities of Mulhouse and Strassbourg are special cases, since they were part of France until 1871 and part of Germany from 1871 to 1918. Thus, the value of LT_{ijt} between Mulhouse and Toulouse, e.g., goes from 1 to 0 after 1871. Moreover, we could in principle take into account the fact that the unified Germany and other countries in our sample went on the gold standard in the 1870s. We have not done so mainly because being on the same commodity standard is not identical to mutually agreed upon legal tender status. We have also considered the effects of fixing exchange rates on the price gaps. Incorporating this into our analysis does not qualitatively change our findings.

Aachen was an early one in Europe, completed in the year 1841, and as early as 1843 this line connected internationally to the Belgian cities of Brussels and Brugge. Grain from the relatively low-price areas of Prussia could be shipped via Hanover to the emerging industrial areas of Cologne by the year 1847 via the *Köln-Mindener* line. But that was only the Cologne-Deutz part of Cologne, located on the east side of the Rhine—the railway bridge across the Rhine was completed only in the year 1859, and until then, Aachen as well as the Belgian markets could effectively not be supplied by rail with the relatively cheap Eastern European grain.[31]

Below we also employ information on when states formally abolished serfdom as an indicator of institutional quality. Late abolition of serfdom is taken as a sign of institutions that are not conducive to economic efficiency and growth, consistent with Acemoglu, Cantoni, Johnson, and Robinson (2008) who show that prosperity in 19th century Europe was lower when the prevalence of feudal institutions increased. Such institutions might also affect the efficiency of steam trains. Table A6 of the appendix reports the year of the initial decree abolishing serfdom is presented for our markets. The average year is 1809, with the earliest year being 1783 (Grand Duchy of Baden) and the latest year being 1848 (Austria-Hungary).

Finally, we analyze whether state and private railways had different impacts on trade and the size of the market. On the one hand, private railways might emerge whenever efficiency gains outweigh the cost of adoption, whereas state railway might serve also purposes other than economic efficiency. On the other, there might be market failures, for example imperfect capital

[31] We have also experimented with another railway variable that incorporates information on how much freight traffic was present on a given rail line in a given year, based on information in Fremdling et al. (1995). This takes into account differences in the relative importance of rail connections for freight traffic. Moreover, the actual freight traffic figures also reflect differences across rail lines and over time in terms of freight charges per ton-kilometer. Results with this alternative variable were found to be similar.

markets, that the state railways might be able to overcome. We have evaluated the historical record to determine whether state or private railways were most important for each of the 68 markets in our sample, and the result is shown also in Table A6 of the appendix.[32]

Data sources Major sources for the wheat price data are Shiue and Keller (2007) as well as Seuffert (1857). The information on trains comes mainly from IEG (2008) and Putzger (1997). The customs liberalization and currency agreement variables are based on the accounts in Henderson (1959), Hahn (1984), and Willis (1896), as well as historical maps at IEG (2008). For population data, we draw on Bairoch et al. (1988), de Vries (1984), Kunz (2008) and Mitchell (1980). The institutional variables are based on Blum (1978) and Fremdling, Federspiel, and Kunz (1995) for the abolition of serfdom and state versus private railways, respectively. Additional details can be found in the appendix.

We now turn to the empirical analysis.

4 Empirical Results

To assess the importance of customs liberalization in bringing down price gaps between markets, consider the following regression:

$$pdif_{ijt} = \beta_0 + \beta_1 CU_{ijt} + \gamma X' + \varepsilon_{ijt} \quad (1)$$

[32] The key criteria is whether the state had an important role in financing and in operating the railway early on. Since private operators typically needed a concession (license) awarded by the government, the state had always some role in the decision to adopt steam trains. While typically states adopted either private or state railway systems, this is not the case for Prussia and Bavaria, which had mixed systems. Our analysis takes this into account.

which relates the log absolute bilateral price gap between markets i and j in year t to the dichotomous customs liberalization variable CU_{ijt} and a vector of control variables X. We are interested in estimating β_1. The concern is that customs liberalizations were not exogenous, so that CU_{ijt} is correlated with ε_{ijt} and OLS estimates are inconsistent. To address this, we adopt an instrumental variable (IV) approach:

$$\begin{aligned} pdif_{ijt} &= \beta_0 + \beta_1 CU_{ijt} + \gamma X' + \varepsilon_{ijt} \\ CU_{ijt} &= \delta_0 + \delta_1 DistCoast_{ij} + \delta_2 ZollPop_{ijt} + \theta X' + v_{ijt} \end{aligned} \tag{2}$$

where $DistCoast_{ij}$ and $ZollPop_{ijt}$ are two instruments for customs liberalization. The first captures the distance of markets to the coast. Almost all of the customs liberalizations between cities in this sample were due to the enlargement of the *Zollverein*. Moreover, the date of a state's accession to the *Zollverein* is clearly related to the distance to the coast, with more distant markets joining earlier. Not being a member of the *Zollverein* mattered more for the states in the South of Germany, since the external tariff of the *Zollverein* prevented customs-free access to the coast, which gave relatively low-transport access to distant markets. It is thus not surprising that by the year 1836, all German states to the south of Prussia had joined the *Zollverein*, see Figure 3. As one would expect, based on this there is a strong cross-sectional relationship between the distance to the coast and the year of *Zollverein* accession (R^2 of 0.48, see Figure A1 in the Appendix).

The bilateral variable $DistCoast_{ij}$ equals the minimum of the distance to the coast for market i and for market j. If at least one of the markets is located near the coast, customs between i and j would tend to be not liberalized. To gain precision, we add a second instrument

based on the distance-weighted *Zollverein* population in that particular year, $ZollPop_{ijt}$; it is defined as the log average of the distance-weighted *Zollverein* populations in market i and j, $ZollPop_{it}$ and $ZollPop_{jt}$, respectively, where

$$ZollPop_{it} = \sum_{s \epsilon S} \frac{I_{st}^{ZV} \times Pop_{st}}{d_{is}}, \forall i, t. \tag{3}$$

Here, Pop_{st} is the population of state or country s in year t, d_{is} is the geographic distance between market i to the capital of state s, and I_{st}^{ZV} is an indicator variable that is equal to one if state s in year t was part of the *Zollverein*, and zero otherwise. A larger *Zollverein* population means a larger customs-free internal market in the customs union, and thus the greater is the incentive to join. Note that this variable changes over time while the distance-to-coast variable does not.

We adopt analogous IV approaches for trains and currency agreements, with the following instrumental variables. The size of the markets that the railway would connect was an important consideration for rail construction. There is a strong cross-sectional relationship between city population in the year 1800, which pre-dates railway construction anywhere, and the earliest date at which a city-market had a railway connection (shown in the Appendix, Figure A2). This indicates that on average, larger cities adopted railways earlier than smaller cities. The first bilateral instrumental variable for TR is the average of the population sizes in city i and city j in the year 1800 (denoted $Size1800_{ij}$).

Also the market potentials of locations i and j are employed to predict whether there exists a train connection between them, where the market potential of location i is defined as $MP_{it} = \sum_{s \epsilon S} \frac{Pop_{st}}{d_{is}}$, $\forall i, t$. A stylized fact in regional economics is that a location's market

potential is a strong predictor of its economic potential (Harris 1954). Since a city's market potential is computed from the distance-weighted sizes of all states in the sample, the influence of the city's size itself on its market potential is negligible. This is relevant for the exclusion restriction, because it reduces the likelihood that the instrument varies systematically with characteristics that determine the price gap between markets i and j. The market potential of markets i and j at time t, $Market\_Potential_{ijt}$, is defined as the log average of MP_{it} and MP_{jt}, for all pairs ij and years t.

We construct also two instruments for the currency agreement variable. The first is based on the extent to which monetary systems imposed differential transactions costs for trade between market pairs. In the Southern German states, transactions costs were particularly high because coins from different states had highly varying silver contents even though they had the same face value, one *Gulden*. These states had relatively more to gain from currency agreements, and the Southern German states formed currency agreements before other states did. The instrument for the bilateral LT_{ijt} variable is a dichotomous variable that is equal to one if both markets i and j belong to the *Gulden* area, and zero if at least one of the markets is not part of the *Gulden* area (the instrument is denoted by *Gulden1754*). This *Gulden* area was formed about half a century before the sample period begins, in the year 1754. A second instrument for currency agreements is created as the distance-weighted log average population covered by full legal tender status, denoted by $CurrPop_{ijt}$. It is defined as the *Zollverein* population variable above, except that an indicator variable for currency agreement membership, I_{ijt}^{CA}, plays the role of the *Zollverein* indicator I_{ijt}^{ZV} in equation (3).[33]

[33]Both of our currency agreement instrumental variables are related to the factors that raise the gains from a currency union stressed by Alesina and Barro (2002). First, the reason why the gains from currency agreements for the Southern German states (of the *Gulden1754* bloc) were relatively high had to do with these states financing the wars following the French Revolution in ways that were more detrimental to the value of their

We employ only a subset of the data in the empirical analysis, namely observations at five-year intervals (1800, 1805, ..., 1895). Since in the case of wheat, shocks to prices often affect the crop for several years, using annual information would yield relatively little additional information while at the same time creating serial correlation.[34] Table 2 presents summary statistics of the data. The average price gap in our sample is 0.15, with a standard deviation of 0.12. The table also shows that the fraction of observations where customs was liberalized is somewhat higher than the fraction where a train connection existed (30% versus 21%, respectively). This reflects the fact that the customs liberalizations started in the late 1820s, which is at least a decade before the building of railway tracks gained momentum in Germany. About 13% of the sample had the *Gulden* currency in the year 1754, and the smaller distance to the coast of the two locations in a market-pair is on average 181 kilometers. The final row of Table 2 shows that the average number of observations per market-pair is 11.4, or an equivalent period of 57 years during the 19th century. We now turn to estimating the effect of customs liberalizations, train connections, and currency agreements on bilateral price gaps.

currencies than the Middle and Northern German states (see Rittmann 1975, 467-469; Holtfrerich 1993, 521). Thus, the commitment value of currency agreements for the Southern states was relatively high. And the *CurrPop* variable reflects in part the idea that the propensity to join an existing currency agreement rises, the larger is the size of the economy governed by the currency agreement.

[34] For the most part, we also restrict our attention to observations that lie in a window of twenty-five years before or after the institutional and technological changes that we consider; this gives 6,990 observations. Results from employing all available observations are presented in the Appendix, Table A1.

4.1 Market Expansion and the Relative Costs versus Benefits of Change

In Table 3, specification (1) presents results for the following two-stage least squares (TSLS) regression

$$\begin{aligned} pdif_{ijt} &= \mu_{s(i)s'(j)} + \pi_t + \beta_1 CU_{ijt} + \varepsilon_{ijt} \\ CU_{ijt} &= \mu_{s(i)s'(j)} + \pi_t + \delta_1 DistCoast_{ij} + \delta_2 ZollPop_{ijt} + v_{ijt} \end{aligned} ,$$

where $\mu_{s(i)s'(j)}$ are state-pair fixed effects, and π_t are time fixed effects.[35] Our sample includes 68 markets in 20 states and countries. Each of the more than two thousand market pairs ij belongs to a particular state-pair, with the corresponding bilateral fixed effect denoted by $\mu_{s(i)s'(j)}$.[36] These fixed effects control for unobserved heterogeneity at the state-pair level: for example the geographic distance between states which typically will affect transport costs, and the location of rivers, which may affect the importance of steam trains in bringing price gaps down.[37]

The first three specifications in Table 3 are included to assess the performance of the instrumental variables; the first-stage results are shown in Panel B, while the second-stage results are presented in Panel A. In column (1), the positive coefficient on the distance to the coast

[35] Reported are robust 2-step efficient GMM standard errors, which are clustered at the state-pair level. This is preferred since customs liberalizations typically occur at the state-pair level, so that trade barriers between any markets for a given state-pair often fall away in the same year. The analogous is often the case for currency agreements as well. Also train connections were frequently established so that the opening of a particular train connection also connected other markets for the same pair of states.

[36] With 20 states and countries, there are up to 400 state-pair fixed effects. In the regressions, we estimate close to 300 of those (not reported); the remaining ones cannot be estimated since such state-pairs are not observed in the sample.

[37] For some of the larger states, such as Prussia, state pair fixed effects imperfectly control for bilateral distance between markets. We address this by showing that the results are similar using market-pair instead of state-pair fixed effects. The regressions are also weighted by the number of bilateral price gap observations for a particular pair; unweighted regressions yield quite similar results. See Table A2 in the appendix.

indicates that markets in states that are relatively far away from the seaboard joined the *Zollverein* relatively early. This was expected, given the strong positive cross-sectional relationship mentioned earlier, but the first-stage regression produces this result controlling for unobserved heterogeneity. We also find that *Zollverein* membership became more attractive as the *Zollverein* population grew (coefficient of 0.022 on $ZollPop$). In the train regression of column (2), the market potential and the population in the year 1800 enter positively. This is consistent with the idea that both raise the likelihood that a train connection between markets exists, though only the market potential variable is significant at standard levels. In the currency agreement specification (3), being in the *Gulden* area early on ($Gulden1754$) is a good predictor of whether currency agreements exist, and moreover, the larger is the population covered by currency agreements, the more likely it is that additional states join the agreement.

To sum up, the instrumental variables have the expected impact. What about the power of the instruments? Given the poor performance of instrumental variable estimation with weak instruments (Bound, Jaeger, and Baker 1995, Staiger and Stock 1997), we use two statistics to gauge the power of the instruments. Table 3 reports the p-values of the F-test of the excluded instruments, as well as Shea's partial R^2 for the excluded instruments. The R^2 values are relatively low, ranging from about one to three percent. Moreover, the p-value for the F-test of the train first-stage is one percent, which also suggests that power is limited.[38] However, it is plausible that there is variation in these IV effects. France, for example, was never offered *Zollverein* membership, and thus it is not clear that the *Zollverein* population has the same impact on the propensity of France to join as it does for German states. Thus, we allow the instrumental variables' influence to vary by state. The equations for the customs effect then

[38] This F-statistic is 5.25, below the rule of thumb of a minimum of 10 suggested by Staiger and Stock (1997).

become

$$\begin{aligned} pdif_{ijt} &= \mu_{s(i)s'(j)} + \pi_t + \beta_1 CU_{ijt} + \varepsilon_{ijt} \\ CU_{ijt} &= \mu_{s(i)s'(j)} + \pi_t + \delta_{1s} DistCoast_{ij} + \delta_{2s} Zoll\_Pop_{ijt} + \nu_{ijt} \end{aligned} \quad , \tag{4}$$

where δ_{1s} and δ_{2s} are the first-stage parameters varying by state.[39] The results are shown in columns (4), (5), and (6) of Table 3.[40] The additional instruments raise the explanatory power of the first stage by at least a factor of three, and also the F-tests' p-values are now all lower than 0.001. The possibility that our inferences are strongly affected by weak instruments appears to be remote now.

Looking at the second-stage results, one sees that customs liberalization, trains, and currency agreements all increase market size as evidenced by lower price gaps. The trains effect is the largest, at about -0.22, followed by the customs impact at about -0.13, but also the currency estimate is substantial, at about -0.10. These results are preliminary since the relative impact of institutional and technological change can best be gauged by including both into the same regression, something we will turn to below. For now, it is instructive to compare the IV results with the corresponding OLS results, which are shown in Panel C of Table 3.

According to the OLS estimates, neither customs liberalization nor currency effects have a significant impact on price gaps, which is in sharp contrast to the IV estimates. Moreover, even though both the OLS and the IV estimate point to a significant price-gap reducing effect from steam trains, the IV estimate is more than eight times as large as the OLS estimate. It is thus not surprising that for all three variables specification tests strongly reject the null

[39] With $S = 20$ states and countries, the maximum number of instruments here is 40. The actual number of instruments that are employed depends on the available price data; it is listed in the table.

[40] From now on, the first-stage estimates $\hat{\delta}_{1s}$ and $\hat{\delta}_{2s}$ are not shown to conserve space; they are available from the authors upon request.

hypotheses of exogeneity (second to last line in Table 3).[41] This is the first main result of our paper: accounting for the endogeneity of institutional and technological change is crucial for the estimation results.

There are a number of key questions. First, why are our OLS estimates relatively close to zero whereas others have recently found sizable impacts in OLS regressions? One reason might be that in our sample, there are many factors that affect institutional and technological change, and they outweigh each other to a substantial degree.[42] Second, why are our IV results larger (in absolute value) than the OLS estimates? Take the trains impact, for example. If endogeneity of train adoption induces selection such that those market-pairs expecting large price gap reductions adopt trains before other market-pairs anticipating smaller reductions, the IV estimate would be smaller (in absolute value), not larger than the OLS estimate. However, this is not what we find. Instead, the results suggest that many of the late train-adopting market-pairs are those that would have benefited relatively strongly, and those late-adopters tend to be relatively small cities. In that case, the relatively large effect of trains on shrinking price gaps becomes apparent only once the IV estimation un-does the relation between timing of adoption and market size. In addition, the IV estimation might also address measurement error issues that could bias the OLS estimate towards zero. Specifically, the 0/1 trains variable

[41] These Hausman-style tests are based on the difference between IV and OLS estimates. Since only IV is consistent in the presence of endogeneity, a large difference between IV and OLS results supports endogeneity. The large difference between OLS and IV results also gives another perspective on the possibility of weak instruments. Weak instruments would bias the IV results towards the OLS estimates (Bound, Jaeger, and Baker 1995). Given the substantial differences between OLS and IV estimates in (4) to (6), that appears to be unlikely.

[42] We know from the historical record, for example, that personal animosity between rulers affected the likelihood of institutional change in form of currency or trade agreements between them. Also, the leader of some states were more interested in steam trains than others. See Henderson (1959), Hahn (1984), and Fremdling, Federspiel, and Kunz (1995) on this. Another reason for the OLS estimates close to zero might lie in the bilateral fixed effects we include, which goes beyond sets of importer and exporter fixed effects that are often employed.

is likely measuring the trains impact with error, since as noted above the trains effect would have been dependent also on the train freight tariff structure, among other factors. Similarly, the actual *Zollverein* accession date of certain states was occasionally affected by the personal preferences of the states' leaders.[43] Because the IV estimation, in contrast, predicts *Zollverein* accession with the distance to the coast and *Zollverein* population, it is not prone to this type of measurement error.

Another important issue is whether the instruments are exogenous to price gaps. As usual, there can be no direct test of the exclusion restrictions. Exogeneity of the instruments is however plausible for a number of reasons. First, on *a priori* grounds, $DistCoast$ is based on exogenous geographic factors while $Gulden1754$ and $Pop1800$ are based on pre-sample information. And as noted above, the market potential, *Zollverein* population, and currency agreement population measures by construction do not depend critically on characteristics of the market-pair ij itself. Second, we have employed overidentification tests that ask whether the instruments as a set appear to be valid. The p-value of Hansen's J test statistic is shown in the last row of Table 3, and in none of these tests can we reject the null of instrument validity at standard levels of significance. Third, we have conducted some informal analysis on the exclusion restrictions by computing the sample correlation between price gap, the dependent variable, and the instruments. These correlations are typically quite low. For example, the correlation of $pdif$ with $Pop1800$, $DistCoast$, and $Gulden1754$ is equal to -0.017, 0.013, and -0.024, respectively. Moreover, in exploratory regressions of the price gap on the instrumental variables, the latter are typically not significant.

[43] Or, for that matter, by the preferences of foreign leaders. The decision of Hanover to delay its accession to the Prussian-led Zollverein, for example, was to a significant extent determined by the preferences of the King of England, who ruled Hanover in personal union.

The following section turns to estimating the impact of institutional and technological change in direct comparison to each other.

4.2 Institutional and technological change compared

We first consider customs liberalizations and the establishment of train connections. In Table 4, column (1), the customs effect is estimated at about 7% and the trains effect at about 13%. These estimates are lower by about 40% compared to the results in Table 3, where one variable at a time is included. This suggests that, quite plausibly, customs liberalization and train adoption explain to some extent the same variation in changing price gaps. The first-stage regressions are strong and the Hansen J test indicates that exogeneity of the instruments cannot be rejected at standard significance levels. Comparing the impact from currency agreements with the introduction of steam trains, the former lowers price gaps by about 6% while trains have a 16% effect (column (2)).

How about the impact of trains relative to both customs liberalization and currency agreements? It turns out that it is difficult to estimate jointly the effect from customs liberalization and currency agreements, since markets that benefited from one typically also benefited from the other (the correlation of CU and LT is 0.75). In order to be able to compare the impact from steam trains with a broad institutions effect, we construct a new variable, $INST_{ijt}$ that incorporates both currency and customs information:

$$INST_{ijt} = \begin{cases} 0 \quad if \quad CU_{ijt} = 0 \text{ and } LT_{ijt} = 0 \\ 1 \quad if \quad CU_{ijt} = 1 \text{ and } LT_{ijt} = 0 \quad \text{or} \quad CU_{ijt} = 0 \text{ and } LT_{ijt} = 1 \\ 2 \quad if \quad CU_{ijt} = 1 \text{ and } LT_{ijt} = 1 \end{cases} \tag{5}$$

Specification (3) in Table 4 gives an impact for this institutions variable of about 3%, while the trains effect is estimated at about 13%. The estimate of 3% for $INST$ might appear somewhat low in the light of 7% for customs and 6% for currency agreements. At the same time, given that CU and LT are correlated, one would expect that CU in column (1) and LT in column (2) pick up part of the effect from the other institutional variable, and, if one compares the CU and LT estimates for the impact for $INST = 2$ (both CU and LT), they are similar, namely 6%. Moreover, also diminishing returns to institutional change might explain part of the finding. To sum up, with train estimates of around 14% and customs or currency impacts of about 6.5%, the impact of technological change on market size in the 19th century was about twice as large as that of institutional change. This is our second major finding.

We now turn to a number of important robustness checks, see Table 5. While the baseline results from Table 4 are repeated on the left in columns (3) and (4), the sample is restricted to the years from 1820 to 1880. It is during these 60 years that most of the train connections were established, and when customs borders were liberalized, and currency agreements were formed. Moreover, this period also excludes the early 1800s, where prices and trade may be particularly strongly affected by wars (the Napoleonic Wars ended in 1815). Next, we show results from trimmed price gap samples. Specifically, there we drop the observations that exhibit the 2.5% highest and 2.5% lowest price differences during the sample period. This enables us to see whether our results are strongly driven by a small number of unusual but influential observations. We see that for the years 1820-80, the impact of institutions is slightly larger than in the baseline, and for the trimmed price gap sample, it is somewhat smaller, but overall these results are not very different.

We also examine the robustness of the results in terms of focusing on the customs lib-

eralizations associated with *Zollverein* accession treaties, because there were other customs liberalizations, and these might affect our results through direct or third-country effects. First, there were customs liberalizations among German states outside of the *Zollverein* liberalizations. Second, the *Zollverein* as a whole liberalized trade for some years with non-*Zollverein* countries. And third, countries outside of the *Zollverein* at times liberalized trade between each other.[44] While our analysis incorporates these to some extent, we are far from having complete information on grain protection and liberalization for this 19th century sample.[45] However, the single biggest event in this respect occurred in the third quarter of the 19th century, when many countries liberalized their trade. We know that the *Zollverein* had no external duties on wheat for some time after the year 1853 (Tracy 1989, 87; Henderson 1959, 226). Only with the arrival of grain from the United States about two decades later, pressure for import protection mounted and in 1879, the German *Reichstag* reverted to import tariffs for wheat (Tracy 1989, 89).

In Table 5, we compare the results between our baseline and two alternative treatments of customs liberalizations. For the "Pervasive Liberalization" specification (7), we assume that the other European countries' policies were identical to that of the *Zollverein*, respectively the German *Reich*; we assume that during the years 1853 to 1879, no customs duties existed between any two markets in the sample. For the "Only ZV Liberalization" specification (8), we make the alternative assumption that there were no external customs duties for the *Zollverein* markets,

[44] An example of the first is the South German Customs Union between Bavaria and Wurttemberg. It was formed in the year 1828 and lasted until 1833, when both states became part of the *Zollverein*. Our analysis incorporates the direct but no third-country effects. An example of *Zollverein* customs liberalizations with other countries is the agreement with Belgium in the year 1838. However, these were neither as comprehensive nor as long-lasting as the *Zollverein* liberalizations. An example of the third point are the customs liberalizations throughout Europe, as discussed in the following.

[45] For general information on 19th century trade agreements, see Pahre (2008).

and that tariffs greater than zero remained in place between the non-*Zollverein* markets.[46] The results for specifications (7) and (8) indicate that either treatment leads to similar conclusions as our baseline. This suggests that accounting fully for temporary liberalizations and controlling for third-country effects will not change our main findings.

We have also extended this robustness analysis in other dimensions.[47] This can be found in Tables A1 to A4 in the Appendix. Overall, these results confirm the above findings.

4.3 The direct and indirect effect of institutions

In this section we expand the analysis to consider indirect effects of institutions through their impact on the introduction of steam trains. Figure 4 illustrates the idea: in addition to the direct effect of institutions on market size–the solid arrows from currency agreements and customs liberalizations–we now extend our analysis to indirect effects from institutions on the adoption of steam trains on market size (the dashed arrows). We consider the interaction of two institutions on the adoption of steam trains, one more proximate and one more fundamental. The former is whether the steam trains for a particular market were primarily run by the state or by private agents. There is a general tendency in the literature to assume that private railways would be more efficient (e.g., Fremdling 1975), although as noted above this need not be so if the state provides an important public good. In 19th century Central Europe, both state railways and private railways were significant in size, which enables us to compare the impact of the two on market size.

[46] Even though many European countries had relatively low duties on grain in the period of 1850-80, and in particular between 1866-79, they were typically not equal to zero; see Bairoch (1989) and Jacks (2005).

[47] This includes shorter and longer time horizons (instead of a 25 year window), limited-information maximum likelihood estimation (instead of TSLS), additional controls for unobserved heterogeneity (market-pair fixed effects instead of state-pair fixed effects), unweighted regressions, and robustness analysis by country.

We also use a more fundamental institutional variable, which is the date at which serfdom was formally abolished in a state or country. We take the abolition of serfdom as a general sign that highly discretionary and exploitative activities by the state towards private agents are becoming less likely.[48] To the extent that this general institutional setting affects the impact of steam trains on market size, we expect that states that abolish serfdom relatively early will tend to see stronger gains from train adoption than late-abolishing states. It is important to note that in this analysis we treat the existence of a particular form of railway ownership and serfdom as exogenously given.[49]

The results are shown in Table 6. In specifications (1) and (2), we add two variables to the baseline results: first, there is $Train\_StateRailway$, which is the interaction of the 0/1 trains variable with $StateRailways$, an indicator of the importance of state railways in market pair ij. The latter is a constant for each market pair ij, taking on values between 0 (both markets are private) and 1 (both markets have state railways). This variable is also included by itself to avoid misspecification. In the regression, we treat the interaction variable $Train\_StateRailway$ as endogenous and $StateRailways$ as exogenous. The results suggest that state-run steam trains had a significantly smaller impact on market size than private-run railways. Moreover, with around 70 percent, the efficiency of state railways in reducing price gaps is substantially lower.[50]

[48] It is not crucial for our analysis to know when serfdom in a particular state or country fell out of use (which was typically considerably earlier than the time when it was formally abolished).

[49] We return to this issue in section 5. Note that from Figure 4, one might also think that the existence or non-existence of serfdom might influence the impact of currency agreements and customs liberalization in an economy. We have explored this possibility but found little evidence for it. Further, one might postulate that serfdom (or state railway ownership) would explain the timing of when train connections were built. However, since the correlation of the 0/1 trains variable with either the date of the abolition of serfdom or whether state railways dominated is low, this is unlikely.

[50] A change from $StateRailway = 0$ to $StateRailway = 1$ is associated with a 85% lower trains effect in specification (1), and with a 55% lower impact according to specification (2).

In columns (3) and (4), we include another interaction variable $Train\_LateAbolition$. Here, *LateAbolition* is a 0/1 variable taking the value of 1 if serfdom was present in at least one of the markets i and j after the year 1831. The rationale of using the maximum year in pair ij is that low institutional quality in one market is enough for it to be unlikely that trains will be important in leading to trade. We find that the impact of trains on market size tends to be lower in states that abolished serfdom relatively late, compared to states that abolished serfdom earlier. The findings are consistent with theories in which the establishment of non-absolutist institutions (no serfdom), or reliance on private railways, lead to greater increases in market size than when railways are state-operated, or the state is feudal. These results are broadly confirmed in a number of robustness checks (see Table A5 in the appendix).

At the same time, one needs to be cautious in interpreting these results. First, the size of the reduction in the trains effect from late-serfdom-abolishers is smaller than that from state railways (20 versus 70 percent). Second, the coefficient on the linear *LateAbolition* variable in (3), as well as on *StateRailway* in (1) and (2), is negative. Relatively low price gaps among late-abolishing market-pairs (or with state-run railways) is not what one expects if early abolishment of serfdom is a general sign of economic efficiency-oriented institutions. In addition, the simple correlation between *StateRailway* and *LateAbolition* is negative, that is, state railways dominated where serfdom was abolished relatively early. It appears, therefore, that while there is evidence that the technology effect of railways indeed is influenced by the specific institutional setting, more work is needed to determine the precise relationship between proximate and fundamental indicators of institutional quality on the one, and their relationship with technological change on the other hand.

The following section presents some concluding discussion.

5 Conclusions

Do institutional and technological change affect the size of the market? To answer this question, we examined systematic deviations from the Law of One Price in Western and Central Europe over the 19th century. This setting is particularly interesting since at that time national and international markets were first emerging. To examine systematic deviations from the Law of One Price, we focus on the market price of wheat in cities in large parts of Europe. The analysis embodies three determinants of trade and the size of the market: customs liberalizations, currency agreements, and train transportation. Admittedly, this list may be incomplete. However, the analysis covers the *Zollverein* liberalizations, monetary unification in Germany, and transportation improvements in form of steam trains, which are *a priori* all of first-order importance for European economic development during this time. Our empirical results strikingly demonstrate that both institutional and technology change must figure prominently for any understanding of the expansion of markets and economic development.

It would have been impossible to arrive at these results without extensive data. To this end, we constructed a unique database on wheat prices, geographic, city, state, and country characteristics, customs and currency agreements, and train connections for 68 markets in a major part of Europe over the entire 19th century. This detailed data lets us address the reality that institutional as well as technological change are endogenous responses to economic conditions at the time. We find that accounting for this endogeneity gives vastly different results than not doing so.

Quantitatively, while there is some variation our estimates are for the most part highly consistent. We find that the introduction of steam trains reduced price gaps by about fourteen

percentage points; customs liberalizations lowered price gaps by about seven percentage points and currency agreements by about six percentage points. Since there appears to be diminishing returns, the combined impact of currency agreements and customs liberalization for the typical market would still be substantially below what the introduction of steam trains would do. Thus, we find that technological change had a larger effect on market size than institutional change in 19th century Europe. With a decline in price gaps of about twenty-two percentage points in the sample, the introduction of trains together with currency and customs agreements account for most of the overall decline in price gaps over this period.

We have also asked whether there exist additional indirect effects from a state's institutions. In particular, we were interested to see whether a state's institutions influence the rate of technological change, in this case the adoption of steam trains. There are a number of possible channels of why this might be so, and correspondingly, the paper examines whether either proximate or more fundamental institutions have played a role. Both the former, measured by the ownership of railways, as well as the latter, captured by whether serfdom existed, are found to have influenced the efficiency of steam trains in raising the size of the market. In particular, for markets that were served primarily by state railways, or that were located in states that abolished serfdom relatively late, the introduction of steam trains reduced price gaps by subtantially less than when railways were privately run or when serfdom had been abolished early. Thus our analysis identifies an important indirect effect from institutions on economic performance.

This research suggests a number interesting areas of future research. One is the relationship between institutions and technological change. Recently, economists have been quite successful in explaining which countries are rich and which are poor with variation in institutional quality.

There is also plenty of evidence that institutional quality affects the rate of technical change.[51] At the same time, we still know too little about what exactly determines whether an economy's institutions will foster technological change, and how fundamental institutions are linked to more proximate ones in their effect on economic efficiency and growth. The analysis in this paper is only a first step in this direction.

Our results also indicate that studies of the impact of technological change today might have a very high return. One example is the impact of advances in information and communications technologies—the transactions costs of technological knowledge—on offshoring and the associated relocation of both jobs and production, as well as the resulting changes in global trade flows. The analysis in this paper suggests more generally that with a good measure of technological change, it is not only feasible to estimate the impact of technology on economic outcomes, but such studies may also yield dramatic new insights on the economics of development.

[51] Recent work includes Acharya and Keller (2008) and Coe, Helpman, and Hoffmaister (2008).

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Table 1: Summary Statistics

Overall sample period: 1800 - 1899

No	City	State/Country	Number of price obs.	Mean price	Year of Earliest Obs.	Year of Zollverein Accession	Year of Earliest Rail Connection	Year of Earliest Legal Tender Status
1	Prague	Austria-Hungary	8	19.47	1836		1845	
2	Salzburg	Austria-Hungary	4	29.02	1849		1860	
3	Venice	Austria-Hungary	7	15.57	1836		1856	
4	Vienna	Austria-Hungary	86	20.57	1820		1845	
5	Baden	Baden	28	16.29	1818	1836	1846	1837
6	Augsburg	Bavaria	41	16.92	1815	1834	1840	1837
7	Bamberg	Bavaria	41	16.32	1815	1834	1844	1837
8	Bayreuth	Bavaria	41	16.82	1815	1834	1853	1837
9	Erding	Bavaria	41	16.33	1815	1834	1859	1837
10	Kempten	Bavaria	41	18.81	1815	1834	1852	1837
11	Landshut	Bavaria	41	15.58	1815	1834	1854	1837
12	Lindau	Bavaria	41	19.14	1815	1834	1852	1837
13	Memmingen	Bavaria	41	18.00	1815	1834	1858	1837
14	Munich	Bavaria	100	18.69	1800	1834	1840	1837
15	Noerdlingen	Bavaria	41	16.14	1815	1834	1849	1837
16	Nurnberg	Bavaria	45	16.42	1811	1834	1844	1837
17	Regensburg	Bavaria	41	15.09	1815	1834	1859	1837
18	Straubing	Bavaria	41	14.65	1815	1834	1858	1837
19	Wuerzburg	Bavaria	41	16.41	1815	1834	1854	1837
20	Zweibruecken	Bavaria	38	16.57	1818	1834	1857	1837
21	Brugge	Belgium	100	20.62	1800		1838	
22	Brussels	Belgium	91	22.45	1800		1838	
23	Braunschweig	Brunswick	50	16.50	1800	1841	1844	1857
24	Bar-le-Duc	France	30	18.08	1825		1851	
25	Chalons sur Marne	France	30	18.55	1825		1851	
26	Luneville	France	30	19.03	1825		1851	
27	Mulhouse	France	76	22.41	1800		1841	
28	Strassburg	France	76	21.63	1800		1841	
29	Toulouse	France	100	21.40	1800		1859	
30	Bremen	Free City	11	20.53	1837	1888	1847	1871
31	Frankfurt/Main	Free City	14	22.57	1816	1836	1840	1837
32	Hamburg	Free City	100	19.68	1800	1888	1846	1871
33	Luebeck	Free City	9	17.58	1837	1867	1851	1871

Table 1, cont'd

No	City	State/Country	Number of price obs.	Mean price	Year of Earliest Obs.	Year of Zollverein Accession	Year of Earliest Rail Connection	Year of Earliest Legal Tender Status
34	Goettingen	Hannover	68	17.12	1800	1854	1854	1857
35	Hannover	Hannover	50	17.81	1801	1854	1844	1857
36	Kassel	Hesse-Cassel	27	14.22	1822	1831	1849	1857
37	Bingen	Hesse-Darmstadt	1	20.34	1840	1828	1858	1837
38	Giessen	Hesse-Darmstadt	1	19.12	1840	1828	1850	1837
39	Mainz	Hesse-Darmstadt	3	23.68	1840	1828	1853	1837
40	Worms	Hesse-Darmstadt	1	20.68	1840	1828	1853	1837
41	Wiesbaden	Hesse-Nassau	1	18.13	1840	1836	1840	1837
42	Grabow	Mecklenburg	71	18.45	1800	1867	1846	1871
43	Boizenburg	Mecklenburg	71	18.30	1800	1867	1846	1871
44	Parchim	Mecklenburg	71	17.43	1800	1867	1880	1871
45	Rostock	Mecklenburg	71	17.57	1800	1867	1850	1871
46	Schwerin	Mecklenburg	71	17.67	1800	1867	1847	1871
47	Wismar	Mecklenburg	57	16.65	1800	1867	1848	1871
48	Nijmegen	Netherlands	93	21.46	1800		1856	
49	Utrecht	Netherlands	15	30.66	1800		1856	
50	Aachen	Prussia	61	18.88	1800	1834	1841	1857
51	Berlin	Prussia	61	18.14	1800	1834	1841	1857
52	Cologne	Prussia	100	18.25	1800	1834	1841	1857
53	Hamm	Prussia	20	20.86	1800	1834	1847	1857
54	Herdecke	Prussia	20	23.23	1800	1834	1848	1857
55	Minden	Prussia	13	21.49	1800	1834	1847	1857
56	Muenster	Prussia	64	18.91	1800	1834	1848	1857
57	Saarlouis	Prussia	20	17.70	1800	1834	1858	1857
58	Soest	Prussia	20	17.71	1800	1834	1850	1857
59	Wetzlar	Prussia	20	19.27	1800	1834	1862	1857
60	Xanten	Prussia	20	18.48	1800	1834	1880	1857
61	Dresden	Saxony	21	16.78	1832	1834	1839	1857
62	Leipzig	Saxony	68	20.15	1832	1834	1839	1857
63	Zwickau	Saxony	21	18.44	1832	1834	1845	1857
64	Basel	Switzerland	10	24.75	1845		1844	
65	Lucerne	Switzerland	9	23.94	1845		1856	
66	Rorschach	Switzerland	14	20.79	1824		1856	
67	Stuttgart	Wurttemberg	5	23.68	1850	1834	1850	1837
68	Ulm	Wurttemberg	6	22.81	1850	1834	1850	1837

Prices in Bavarian Gulden, per Bavarian Scheffel (about 223 liter)

Table 2: Sample Summary Statistics

Variable	Mean	Std. Dev.	Variable Description
Price Gap	0.15	0.12	Absolute value of the log difference of wheat price in market i and market j (pdif)
Train Connection	0.21	0.41	0/1 variable; 1 if train connection exists between markets i and j in year t, 0 otherwise (TR)
Currency Agreement	0.23	0.42	0/1 variable; 1 if currencies of i and j are legal tender in both markets in year t, 0 otherwise (LT)
Customs Liberalization	0.30	0.46	0/1 variable; 1 if customs are liberalized between markets i and j in year t, 0 otherwise (CU)
City Population in 1800	0.39	0.40	Average of the population of city i and city j in the year 1800; in 100,000 people
Market Potential	13.93	1.47	Log distance-weighted state population in year t
Gulden in 1754	0.13	0.34	0/1 variable, equal to 1 if both markets i and j had Gulden as its currency in the year 1754
Currency Population	7.97	8.19	Log distance-weighted population of states that gave each other legal tender status in year t
Distance to the Coast	1.81	1.67	Minimum of market i and market j's distance to the nearest coast, in 100 kilometers
Zollverein Population	10.29	4.26	Log distance-weighted population of states that belonged to the Zollverein in year t
State Railways	0.68	0.33	Average of share of railway in markets in and j that is run by the government
Late Abolition of Serfdom	0.24	0.43	0/1 variable indicating that one or both markets i and j abolished slavery after the year 1831
Length of Observation	11.40	4.71	Number of observations per market-pair

Summary statistics for the baseline sample of twenty-five years before and after train connection, currency agreement, or customs liberalization
Statistics weighted by number of observations of market-pair; Number of observations: 6,990

Table 3: Market Expansion and the Relative Costs versus Benefits of Change

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-Stage Results						
Customs Liberalization	-0.173*			-0.128#		
	(0.072)			(0.019)		
Train Connection		-0.211*			-0.219#	
		(0.092)			(0.034)	
Currency Agreement			-0.158#			-0.099#
			(0.051)			(0.030)
Panel B: First-Stage Results						
Zollverein population	0.022#					
	(0.006)					
Distance to Coast	0.014#					
	(0.004)					
Market Potential		0.011*				
		(0.005)				
City Population in 1800		0.064				
		(0.050)				
Currency Agreement Population			0.020#			
			(0.008)			
Gulden in 1754			0.998#			
			(0.004)			
F-statistic p-value	< 0.001	0.01	< 0.001	< 0.001	< 0.001	< 0.001
Shea Partial R-squared (percent)	1.7	1.1	2.9	11.4	3.6	9.1
Number of excluded instruments	2	2	2	36	24	39
Panel C: Ordinary Least Squares						
Customs Liberalization				-0.014		
				(0.010)		
Train Connection					-0.025#	
					(0.009)	
Currency Agreement						-0.002
						(0.015)
Endogeneity test p-value				< 0.01	0.03	< 0.01
Hansen OverID test p-value				0.13	0.37	0.26

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level
Sample observations are within a twenty-five year span before and after the establishment of train connection, customs liberalization, or currency agreement; number of observations: 6,990; p-value of F-statistics of all regressions < 0.001

Table 4: Institutional and Technological Change Compared

	(1)	(2)	(3)
Panel A: Second-Stage Results			
Train Connection	-0.129#	-0.162#	-0.131#
	(0.010)	(0.025)	(0.008)
Customs Liberalization	-0.072#		
	(0.010)		
Currency Agreement		-0.059#	
		(0.022)	
Institutions			-0.031#
			(0.004)
Panel B: First-Stage Statistics			
Trains First-Stage			
F-statistic p-value	< 0.001	< 0.001	< 0.001
Shea Partial R-squared (percent)	5.6	4.8	6.1
Insitutions First-Stage			
F-statistic p-value	< 0.001	< 0.001	< 0.001
Shea Partial R-squared (percent)	18.9	9.8	19.4
Number of excluded instruments	74	62	96
Hansen OverID test p-value	0.14	0.28	0.16
Number of observations	6,990	6,990	6,990

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or currency agreement; p-value of F-statistics of all regressions < 0.001
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population (specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table 5: Robustness Analysis

	Baseline		Years 1820 to 1880		Trimmed 95% sample		Non-ZV customs liberalizations	
							Pervasive Lib'n	Only ZV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Second-Stage Results								
Train connection	-0.129#	-0.162#	-0.113#	-0.129#	-0.123#	-0.177#	-0.142#	-0.136#
	(0.010)	(0.025)	(0.011)	(0.022)	(0.009)	(0.022)	(0.011)	(0.012)
Customs liberalization	-0.072#		-0.086#		-0.054#		-0.053#	-0.062#
	(0.010)		(0.011)		(0.007)		(0.008)	(0.008)
Currency agreement		-0.059#		-0.091*		-0.043#		
		(0.022)		(0.037)		(0.016)		
Panel B: First-Stage Summary								
Shea R-squared (%)								
Trains	5.6	4.8	5.3	4.8	5.6	4.9	5.6	5.6
Institutions	18.9	9.8	17.7	7.9	18.9	10.2	15.5	12.6
Number of excluded instruments	74	62	74	62	74	62	74	74
Hansen OverID test p-value	0.14	0.28	0.21	0.32	0.18	0.28	0.13	0.15
Number of observations	6,990	6,990	6,072	6,072	6,642	6,642	6,990	6,990

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. #/*/+ Estimate is significant at the 1%/5%/10% level; all regressions include year fixed effects and state-pair fixed effects
Sample observations are within a twenty-five year span before and after the establishment of a train connection, a customs liberalization, or a currency agreement; instruments as in Table 4; p-value of F-statistics of all regressions < 0.001

Table 6: The Direct and Indirect Impact of Institutions

	State Railways		Abolition of Serfdom	
	(1)	(2)	(3)	(4)
Panel A: Second-Stage Results				
Train connection	-0.222#	-0.231#	-0.138#	-0.212#
	(0.059)	(0.052)	(0.010)	(0.021)
Train * State Railways	0.187*	0.125+		
	(0.096)	(0.067)		
Train * Late Abolition of Serfdom			0.045#	0.026+
			(0.009)	(0.015)
Customs liberalization	-0.084#		-0.067#	
	(0.032)		(0.010)	
Currency agreement		-0.066#		-0.019*
		(0.017)		(0.008)
State Railways	-0.044#	-0.039#		
	(0.013)	(0.013)		
Late Abolition of Serfdom			-0.143#	0.036#
			(0.006)	(0.012)
Panel B: First-Stage Summary				
Shea R-squared (%)				
Trains	4.3	4.7	5.9	5.2
Trains * State Railways	2.4	3.5		
Trains * Late Abolition of Serfdom			11.3	16.4
Customs lib'n or currency agr't	6.0	5.3	18.8	10.7
Number of excluded instruments	37	26	74	63
Hansen OverID test p-value	0.11	0.28	0.16	0.15

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. All regressions include year- and state-pair fixed effects. P-value of F-test of all regressions < 0.001
#/*/+ Estimate is significant at the 1%/5%/10% level. Number of observations: 6,990. Sample of observations within a 25 year window before and after the establishment of train connection, customs liberalization, or currency agreement
Instruments: Population in 1800, market potential (specification (1)-(4)), distance to the coast, Zollverein population (specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (4))
P-value of F-test of excluded instruments <0.001 in all cases

Figure 1

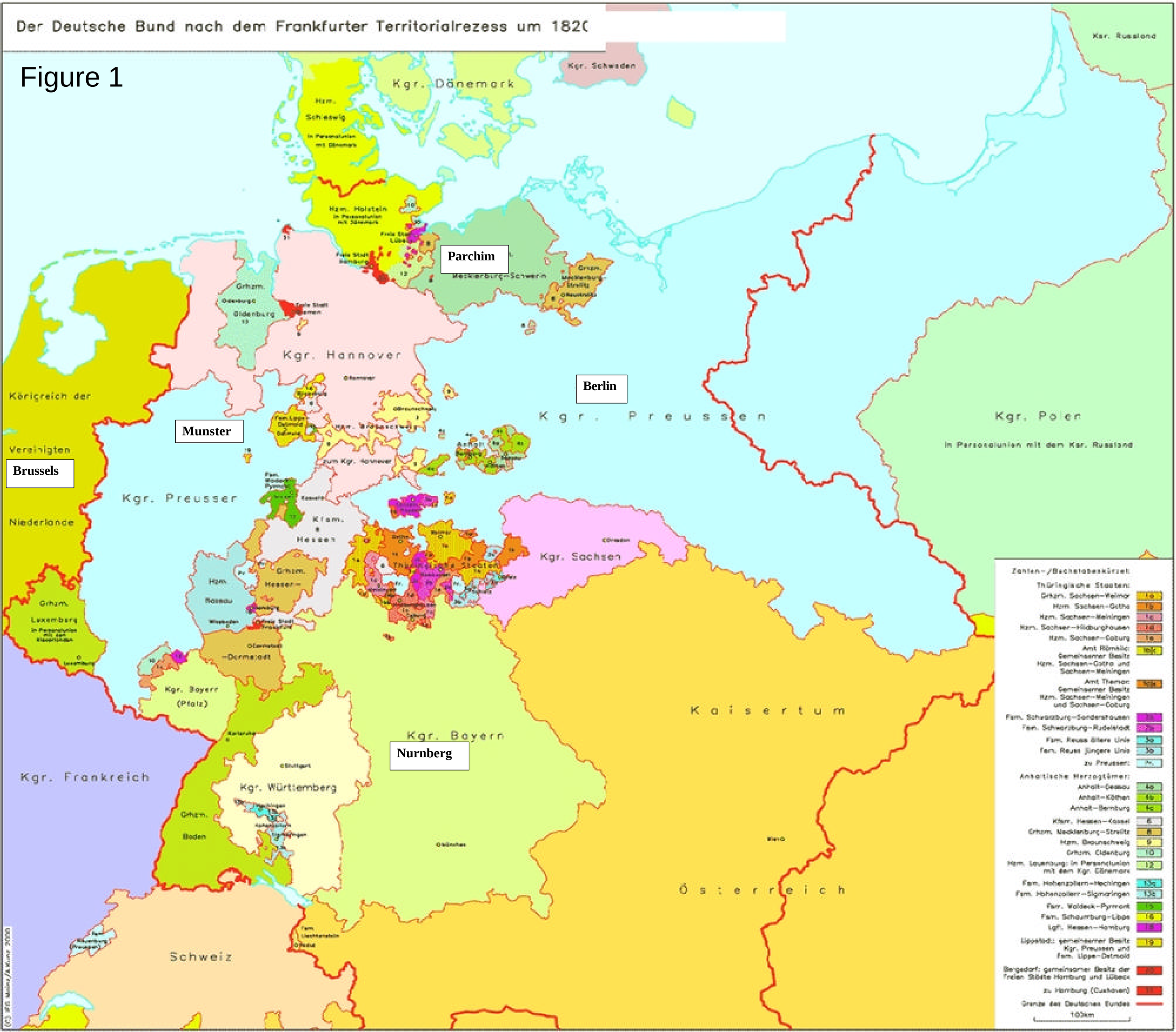

Der Deutsche Bund nach dem Frankfurter Territorialrezess um 182(
Parchim
Berlin
Munster
Brussels
Nurnberg
Kgr. Dänemark
Kgr. Schweden
Kgr. Russland
Kgr. Hannover
Kgr. Preussen
Kgr. Poln
Kgr. Preusser
Kgr. Sachsen
Kgr. Bayern
Kgr. Württemberg
Kgr. Frankreich
Kaisertum Österreich
Schweiz
Niederlande
Königreich der Vereinigten
Kgr. Bayern (Pfalz)
Zahlen-/Buchstabenkürzel
Thüringische Staaten:
Grhzm. Sachsen-Weimar 1a
Hzm. Sachsen-Gotha 1b
Hzm. Sachsen-Meiningen 1c
Hzm. Sachsen-Hildburghausen 1d
Hzm. Sachsen-Coburg 1e
Anhaltische Herzogtümer:
Anhalt-Dessau 4a
Anhalt-Köthen 4b
Anhalt-Bernburg 4c
Grenze des Deutschen Bundes
100km

Figure 2: Train connections in the German states in the year 1850

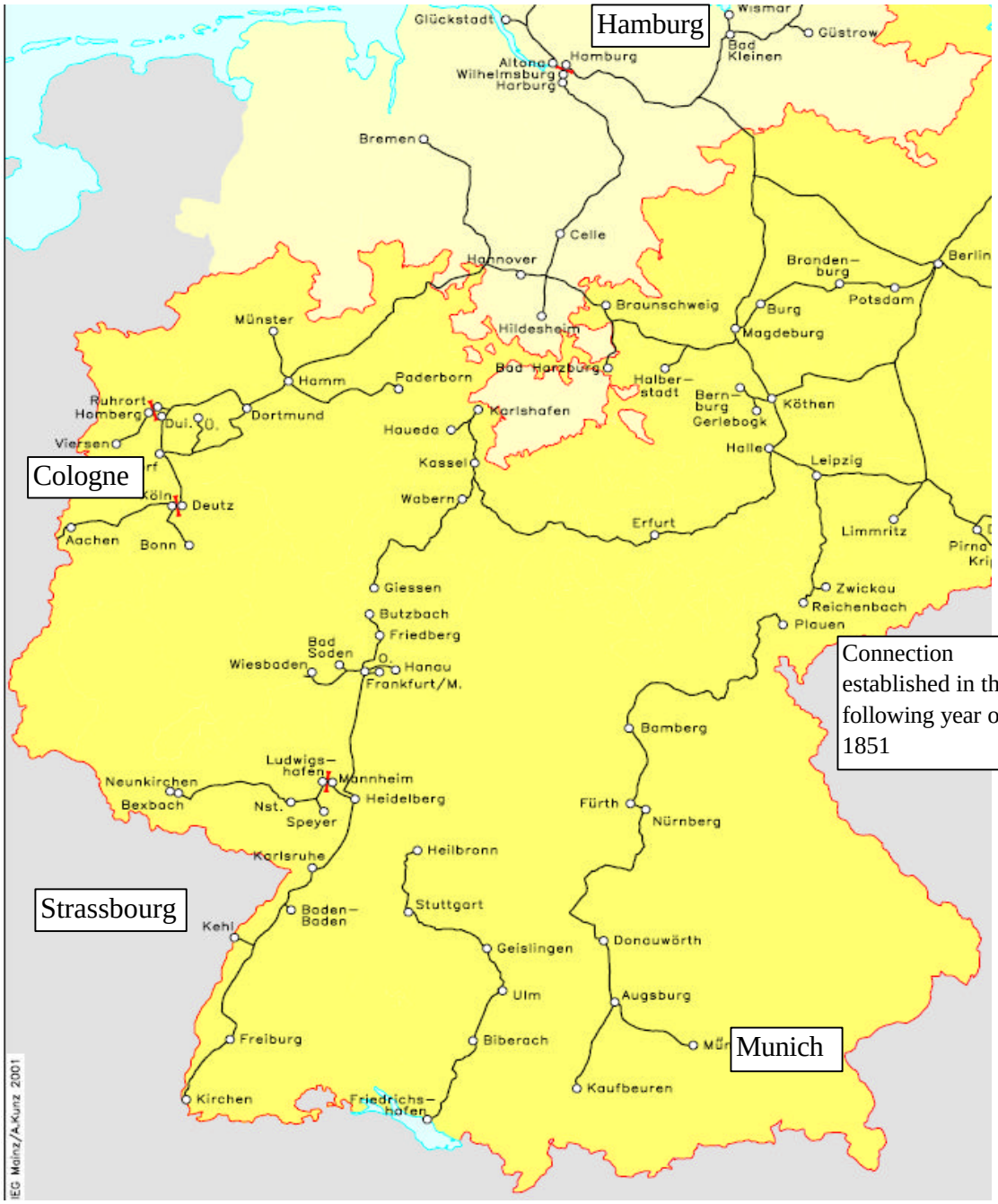


Figure 3: The Southern German States have joined the Zollverein by 1836

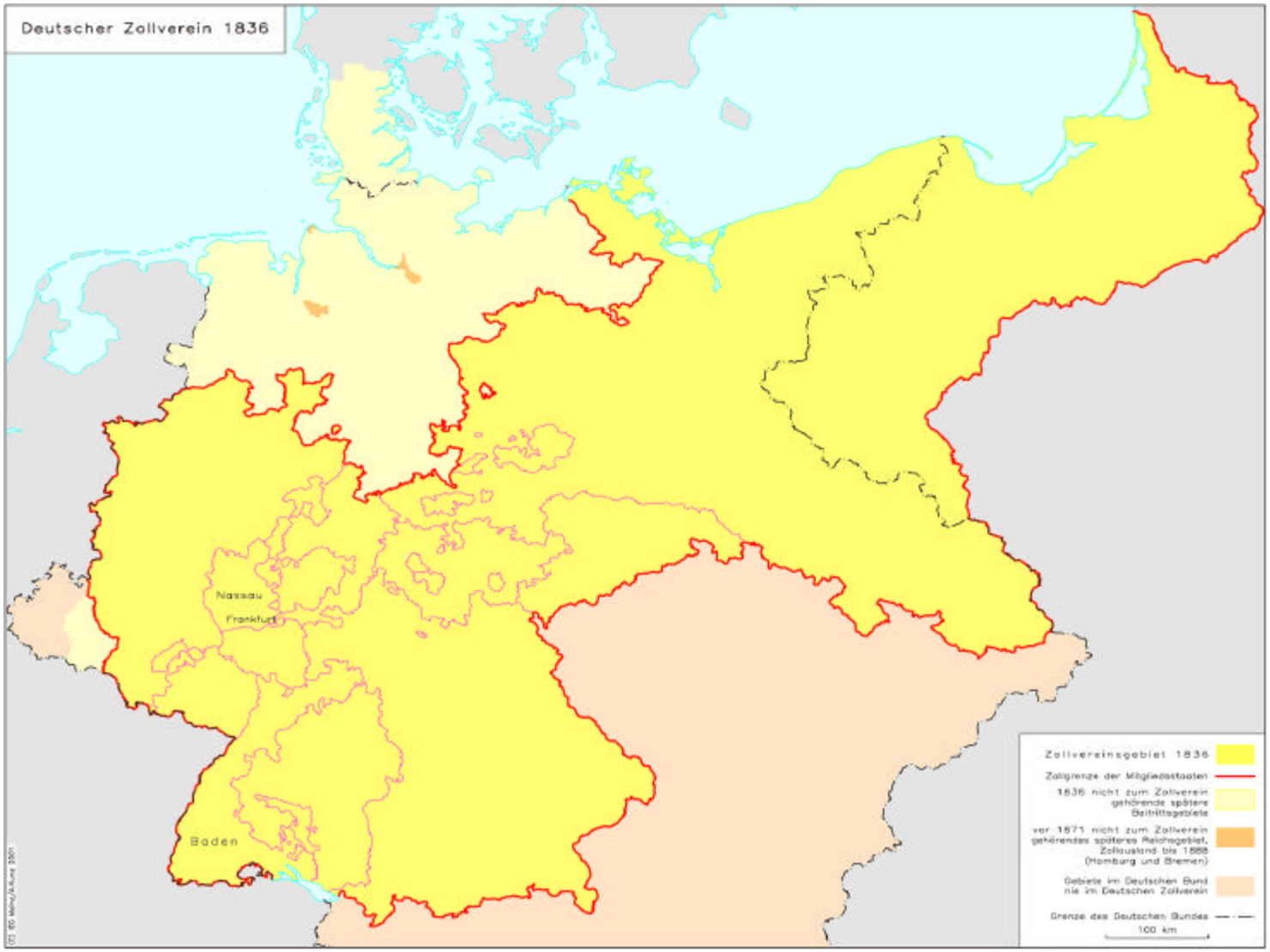
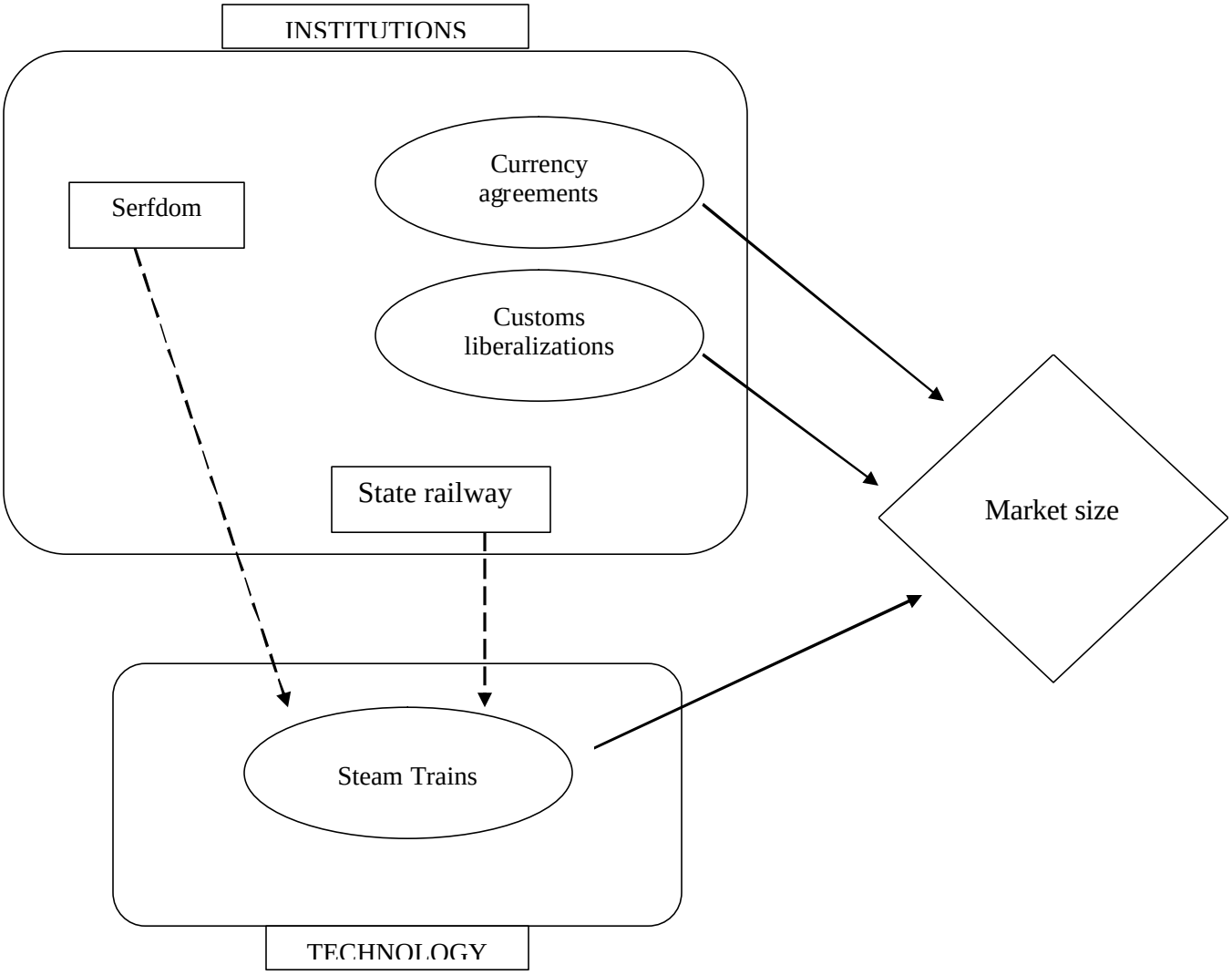


Figure 4: Direct and Indirect Effects from Institutions



Appendix for "Institutions, Technology, and Trade"

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December 2, 2008

1 Robustness analysis

This section presents results that shed additional light on the robustness of our findings. We first turn to the time horizon that is considered in our main specification. Recall that the impact of institutional and technological change on price gaps is estimated from a window of twenty-five years before and after the 'change' (i.e., customs liberalization, currency agreements, or train connection). We report results from varying the size of this window. In general, a larger window will have two consequences. First, it amounts to employing data over a longer time horizon. If there are important adjustment costs that limit the market size in the short-term, then one expects to estimate a larger impact on price gaps with a relatively large window. At the same time, a larger window makes identification more difficult, since in the long-run many other processes may be important. This reduces the signal-to-noise ratio and will typically lead to lower estimates. Which of these effects dominates is not immediately clear.

In Table A1, we see that the customs and currency estimates are similar to the baseline results for the shorter window of fifteen years, and this is the case also for the train estimate. For a longer time horizon of thirty-five years, the estimates for institutional change move towards zero, especially for currency agreements. Using all available data, neither the customs nor the currency effect is significantly different from zero. In contrast, the trains estimate rises, from about -0.14 to -0.21. This however might reflect not only the long-run trains effect but also the weakening of the institution variables (the correlation of TR with CU is about 6%, and the correlation of TR with LT is about 7%). Overall, the results are consistent with the relatively large impact we estimate for trains, and moreover, the twenty-five year window in our baseline appears to be a good compromise between identifying power and time horizon of the estimate.

Next, we present results for market-pair instead of state-pair fixed effects in Table A2. Recall that the analysis is at the level of the market-pair, so using deterministic market-pair fixed amounts to the usual within estimator panel specification. This has the advantage that time-invariant heterogeneity (including bilateral distance) at the market-pair level is controlled for. It may, however, exacerbate measurement error problems. As seen from Table A2, the impact of the institution variables is somewhat lower, around -0.045 instead of -0.065, while the average trains effect is -0.14, as before, but it varies more across specifications (compare (4) and (1)). Overall, our main findings hold up both qualitative as well as quantitatively.

Table A2 also shows results for the estimation with limited-information maximum likelihood (LIML), which have in some settings, in particular when the instruments are weak, better properties than two-stage least squares (TSLS) estimators. In this case, both the currency and the customs point estimates are similar to those with TSLS, although the LIML estimates are less precise (and for currency agreements not significant at standard levels). The LIML trains estimates are somewhat larger than the TSLS estimates, although the difference is not statistically significant. We also show in specifications (3) and (6) of Table A2 that unweighted regressions yield very similar results to the baseline reported in Table 4 (recall that in the baseline we perform weighted regressions, where the number of

bilateral observations for a given market-pair serves as the weight).

We take another step by examining the sensitivity of our results with respect to features specific to particular countries. For example, Austria-Hungary around the year 1820 still had internal customs duties (Bairoch 1989, 6). This would be in contrast to our assumption that there are no customs borders between any two markets of a given state or country with contiguous areas ($CU_{ijt} = 1$, all t). The following analysis is also useful to examine whether our analysis holds even if we restrict the analysis to subsets of states. The six specifications in Table A3 show trains and currency results for restricted samples; "No Austria-H", for example, means that all observations involving a market in Austria-Hungary are dropped from the sample. On the right, we report the average of the six restricted-sample estimates, as well as the baseline estimate from Table 4.

It is clear that individual country- observations have a substantial effect on the currency estimate; the average is only about half the size of the baseline estimate. At the same time, it is reassuring that currency agreements in the fifteen German states, labeled "Only Germany" in Table A3, have a similar effect as in the sample as a whole. This allays concerns that our results are primarily identifying the difference between German and non-German states. Turning to the analysis of the customs and trains effects in Table A4, we find these results to be very robust. Also in the case of customs we estimate a significant price-gap reducing impact from liberalizations if the sample is limited to the German states. Moreover, the "Only Germany" customs impact is somewhat smaller than in the full sample, while the "Only Germany" currency point estimate is slightly larger than in the full sample, so there is no clear pattern of how sample composition affects the results.

Finally, we have also further examined the findings on indirect effects of institutions presented in Table 6. These robustness checks are presented in Table A5. We see that market pairs that were served by state railways see a considerably smaller reduction of price gaps with the arrival of steam trains, and in three out of four cases this effect is statistically significant at standard levels. Also the results for the more fundamental institutional quality variable, late abolition of serfdom, are seen to be robust: steam trains brought price gaps down by less, on average, if they operated in states that abolished serfdom relatively late (Table A5, on the right). The robustness checks also confirm our findings above that the quantitative effect of the proximate variable, $StateRailways$, appears to be larger than that of the fundamental variable, $LateAbolition$.

To sum up, we find the estimates to be robust, more so for the trains and customs estimates than that for currency agreements. Given the generally smaller number of observed changes for currency agreements relative to customs liberalizations or train adoptions, it is difficult to know for sure whether the impact is smaller or less-precisely estimated. In general, however, the robustness analysis confirms the main findings emphasized in the text.

2 Data Sources and Construction

Price data The two most important sources for information on wheat prices are Shiue and Keller (2007) and Seuffert (1857). The former covers markets in Bavaria, Belgium, France, Mecklenburg, and the Netherlands, while the latter provides information on markets in

Austria-Hungary, Baden, Brunswick, Hesse-Darmstadt, Hesse-Cassel, Hesse-Nassau, Saxony, Switzerland, and Wurttemberg. The wheat prices for Prussian markets were provided by Michael Kopsidis, see Kopsidis (2002). Additional sources that improve the coverage are Fremdling and Hohorst (1979), Gerhard and Kaufhold (1990) for Prussia, Hanauer (1878) for the Alsatian cities Mulhouse and Strassbourg, Shiue and Keller's (2007) data for Vienna, and Vierteljahrshefte (1935) for Berlin, Cologne, Hamburg, Leipzig, and Munich.

Since neither quantity nor monetary units were standardized in Europe 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, reported to some extent in 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
Belgium	Hectoliter	0.45	Francs	0.47
Frankfurt	Malter	0.51	Gulden	1.00
France	Hectoliter	0.45	Francs	0.47
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
Netherlands	Zacken	0.45	Gulden Courants	0.99
Austria-Hungary	Metzen	0.27	Gulden	1.22
Prussia	Schaeffel	0.24	Thaler	1.75
Saxony	Schaeffel	0.46	Thaler	1.75
Switzerland	Concordia Malter	0.67	Concordia Francs	0.47
Wurttemberg	Schaeffel	0.80	Gulden	1.00

Other data The main sources of information on railway building are the digital historical maps provided at IEG's website at the University of Mainz, http://www.ieg-maps.uni-mainz.de/ and Putzger (1997). We derive the state versus private railways indicator presented in Table A6 from information in Fremdling, Federspiel, and Kunz (1995). The customs liberalization variable is based on information on the history of *Zollverein* treaties in Henderson (1959) and Hahn (1984), as well as the historical maps at IEG (2008). Also the currency agreement variable is based on information in Henderson (1959) and Hahn (1984), as well as Willis (1896). City population data comes from Bairoch et al. (1988) and de Vries (1984), while state and country-level population figures come from Kunz (2008) and Mitchell (1980). Information on serfdom, as shown in Table A6, comes from Blum (1978).

Finally, the information on cities' latitude and longitude that we employ to compute several variables ($DistCoast$, $ZollPop$, $Market\_Potential$, and $CurrPop$) comes from maporama, http://world.maporama.com/ .

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Table A1: The expansion of market size over different time horizons

	Customs liberalization and steam trains				Currency agreements and steam trains			
	25 year window	15 year window	35 year window	All Data	25 year window	15 year window	35 year window	All Data
Panel A: Second-Stage Results								
Train Connection	-0.129#	-0.114#	-0.156#	-0.229#	-0.162#	-0.126#	-0.155#	-0.202#
	(0.010)	(0.009)	(0.017)	(0.026)	(0.025)	(0.017)	(0.017)	(0.036)
Customs Liberalization	-0.072#	-0.072#	-0.011	-0.008				
	(0.010)	(0.008)	(0.013)	(0.012)				
Currency Agreement					-0.059#	-0.062#	-0.034#	0.003
					(0.022)	(0.015)	(0.011)	(0.013)
Panel B: First-Stage Statistics								
Trains First-Stage								
Shea Partial R-squared (percent)	5.6	6.6	4.2	3.3	4.8	7.4	3.9	3.5
Insitutions First-Stage								
Shea Partial R-squared (percent)	18.9	12.8	17.8	15.4	9.8	13.3	10.9	11.1
Number of excluded instruments	74	74	74	74	62	61	64	64
Hansen OverID test p-value	0.14	0.32	0.08	0.29	0.28	0.24	0.44	0.62
Number of observations	6,990	4,800	8,403	10,434	6,990	4,800	8,403	10,434

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics of all regressions < 0.001
Sample observations are within a certain window before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population (specificaitions (2) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state; p-value of F-test of excluded instruments <0.001 in all cases

Table A2: Additional controls and alternative estimators

	Customs liberalization and steam trains			Currency agreements and steam trains		
	Market-Pair FE	LIML	Alt. weights	Market-Pair FE	LIML	Alt. weights
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-Stage Results						
Train connection	-0.067^{+}	-0.185$^{\#}$	-0.152$^{\#}$	-0.203$^{\#}$	-0.257$^{\#}$	-0.217$^{\#}$
	(0.039)	(0.056)	(0.015)	(0.060)	(0.084)	(0.026)
Customs liberalization	-0.039$^{\#}$	-0.066*	-0.067$^{\#}$			
	(0.012)	(0.028)	(0.011)			
Currency agreement				-0.054^{+}	-0.059	-0.066$^{\#}$
				(0.031)	(0.057)	(0.015)
Panel B: First-Stage Summary						
Shea R-squared (%)						
Trains	3.1	5.6	4.7	2.9	3.8	4.2
Institutions	22.9	18.9	19.1	15.4	13.9	17.6
Number of excluded instruments	38	74	74	38	60	63
Hansen OverID test p-value	0.51	0.16	0.13	0.22	0.43	0.22
Number of observations	6,593	6,990	6,990	6,593	6,990	6,990

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level; in parentheses.
#/*/+ Estimate is significant at the 1%/5%/10% level; all regressions include year fixed effects; p-values of F-statistics of all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Specifications (1) and (4) include market-pair fixed effects instead of state-pair fixed effects; (2) and (5) employ limited-information maximum likelihood (LIML) instead of two-stage least squares. Specifications (3) and (6) do not weight the regressions by the number of observations for each market-pair

Table A3: Country results - currency agreements

	No Austria-H (1)	No Belgium (2)	No France (3)	No Netherld. (4)	No Switzerld. (5)	Only Germany (6)	Average	Baseline Table 4
	Panel A: Second-Stage Results							
Train connection	-0.158#	-0.169#	-0.184#	-0.212#	-0.218#	-0.174#	-0.186	-0.162
	(0.026)	(0.024)	(0.025)	(0.028)	(0.027)	(0.019)		
Currency agreement	-0.006	0.030*	-0.090#	-0.008	-0.010	-0.068#	-0.025	-0.059
	(0.016)	(0.015)	(0.024)	(0.015)	(0.012)	(0.003)		
	Panel B: First-Stage Summary							
Shea R-squared (%)								
Trains	2.9	4.3	6.1	5.2	5.0	6.7		
Currency agreement	15.4	13.7	11.4	10.5	11.3	32.5		
Number of excluded instruments	60	59	59	59	61	46		
Hansen OverID test p-value	0.15	0.13	0.22	0.20	0.15	0.15		
Number of observations	6,544	6,371	5,656	6,711	6,720	4,348		

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics for all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A4: Country results - customs liberalization

	No Austria-H (1)	No Belgium (2)	No France (3)	No Netherlds (4)	No Switz.ld. (5)	Only Germany (6)	Average	Baseline Table 4
	Panel A: Second-Stage Results							
Train connection	-0.127#	-0.143#	-0.129#	-0.130#	-0.130#	-0.124#	-0.131	-0.129
	(0.012)	(0.008)	(0.008)	(0.010)	(0.010)	(0.005)		
Customs liberalization	-0.057#	-0.044#	-0.077#	-0.071#	-0.071#	-0.037#	-0.060	-0.072
	(0.012)	(0.008)	(0.009)	(0.010)	(0.010)	(0.006)		
	Panel B: First-Stage Summary							
Shea R-squared (%)								
Trains	5.8	4.7	6.7	5.8	5.6	6.8		
Customs liberalization	18.5	20.4	20.0	18.9	18.9	22.3		
Number of excluded instruments	69	67	69	69	69	53		
Hansen OverID test p-value	0.25	0.20	0.14	0.13	0.11	0.24		
Number of observations	6,544	6,371	5,656	6,711	6,720	4,348		

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-value of F-statistics for all regressions < 0.001
Sample observations are within a twenty-five year span before and after the establishment of a train connection, customs liberalization, or a currency agreement
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specificaitions (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A5: Robustness checks on direct versus indirect effects of institutions

	State Railways				Abolition of Serfdom			
	Years 1820 - 1880		Trimmed 95% sample		Years 1820 - 1880		Trimmed 95% sample	
Train Connection	-0.260#	-0.236#	-0.201#	-0.200#	-0.128#	-0.153#	-0.139#	-0.173#
	(0.076)	(0.056)	(0.039)	(0.036)	(0.010)	(0.021)	(0.009)	(0.019)
Train * State Railway	0.304*	0.150*	0.161*	0.094				
	(0.118)	(0.074)	(0.064)	(0.059)				
Train * Late Abolition of Serfdom					0.077#	0.044*	0.054#	0.007
					(0.015)	(0.019)	(0.008)	(0.011)
Customs liberalization	-0.159#		-0.062#		-0.076#		-0.052#	
	(0.059)		(0.029)		(0.010)		(0.008)	
Currency agreement		-0.024		-0.070#		-0.069*		-0.030+
		(0.018)		(0.014)		(0.031)		(0.016)
State Railways	-0.073#	-0.073#	-0.032#	-0.031#				
	(0.015)	(0.014)	(0.009)	(0.009)				
Late Abolition of Serfdom					-0.134#	-0.055	-0.129#	-0.001
					(0.007)	(0.042)	(0.005)	(0.023)
Shea R-squared (%)								
Trains	3.2	4.6	4.5	4.8	5.7	5.2	6	5.3
Trains * State Railways	1.8	3.3	2.5	3.7				
Trains * Late Abolition of Serfdom					7.3	12.7	18.8	16.5
Customs lib'n or currency agr't	2.2	6.2	5.7	5.4	17.7	8.1	11.5	11
Number of excluded instruments	37	26	37	27	74	63	74	63
Hansen OverID test p-value	0.08	0.36	0.10	0.27	0.22	0.22	0.22	0.26
Number of observations	6,072	6,072	6,642	6,642	6,072	6,072	6,642	6,642

Dependent variable: absolute value of percentage bilateral price difference; robust standard errors clustered at the state-pair level in parentheses. Sample as in Table 6.
All regressions include year- and state-pair fixed effects. #/*/+ Estimate is significant at the 1%/5%/10% level; p-values of F-statistics of all regressions < 0.001
Instruments: Population in 1800, market potential (specification (1)-(3)), distance to the coast, Zollverein population
(specifications (1) and (3)), and Gulden in 1754, currency agreement population (in (2) and (3)), all varying by state

Table A6: Measures of instutional quality

Number	City Name	State/Country	Abolition of Serfdom*	State Railway Indicator**
1	Aachen	Prussia	1807	0
2	Augsburg	Bavaria	1808	1
3	Karlsruhe	Baden	1783	1
4	Bamberg	Bavaria	1808	1
5	Bar le Duc	France	1789	1
6	Basle	Switzerland	1789	0
7	Bayreuth	Bavaria	1808	0
8	Berlin	Prussia	1807	0
9	Bingen	Hesse-Darmstadt	1820	0
10	Boizenburg	Mecklenburg	1820	1
11	Braunschweig	Brunswick	1832	1
12	Bremen	Free City	1783	1
13	Brugge	Belgium	1789	1
14	Brussels	Belgium	1789	1
15	Chalons-sur-Marne	France	1789	1
16	Dresden	Saxony	1832	0
17	Erding	Bavaria	1808	1
18	Frankfurt	Free City	1783	1
19	Giessen	Hesse-Darmstadt	1820	0
20	Goettingen	Hannover	1831	1
21	Grabow	Mecklenburg	1820	1
22	Hamburg	Free City	1783	1
23	Hamm	Prussia	1807	0
24	Hannover	Hannover	1831	1
25	Herdecke	Prussia	1807	0
26	Kassel	Hesse-Cassel	1831	0
27	Kempten	Bavaria	1808	1
28	Köln	Prussia	1807	0
29	Landshut	Bavaria	1808	0
30	Leipzig	Saxony	1832	0
31	Lindau	Bavaria	1808	1
32	Lucerne	Switzerland	1789	0
33	Luebeck	Free City	1783	0
34	Luneville	France	1789	1
35	Mainz	Hesse-Darmstadt	1820	0
36	Memmingen	Bavaria	1808	1
37	Minden	Prussia	1807	0
38	Mulhouse	France	1789	1
39	Munich	Bavaria	1808	1
40	Münster	Prussia	1807	1
41	Nijmegen	Netherlands	1789	0
42	Noerdlingen	Bavaria	1808	1
43	Nurnberg	Bavaria	1808	1
44	Parchim	Mecklenburg	1820	1
45	Prague	Austria-Hungary	1848	0
46	Regensburg	Bavaria	1808	0
47	Rorschach	Switzerland	1789	0
48	Rostock	Mecklenburg	1820	1
49	Saarlouis	Prussia	1807	1
50	Salzburg	Austria-Hungary	1848	0
51	Schwerin	Mecklenburg	1820	1
52	Soest	Prussia	1807	1
53	Strassbourg	France	1789	1
54	Straubing	Bavaria	1808	0
55	Stuttgart	Wuerttemberg	1817	1
56	Toulouse	France	1789	1
57	Ulm	Wuerttemberg	1817	1
58	Utrecht	Netherlands	1789	0
59	Venice	Austria-Hungary	1848	0
60	Vienna	Austria-Hungary	1848	0
61	Wetzlar	Prussia	1807	0
62	Wiesbaden	Hesse-Nassau	1812	0
63	Wismar	Mecklenburg	1820	1
64	Worms	Hesse-Darmstadt	1820	0
65	Wuerzburg	Bavaria	1808	1
66	Xanten	Prussia	1807	0
67	Zweibruecken	Bavaria	1808	0
68	Zwickau	Saxony	1832	0

* Year of initial decree of emancipation; source: Blum (1978) and own estimates for missing data: For Belgium (founded in 1830) and the Netherlands, the French date is employed; for the Free Cities, which were urban and relatively democratic, we pick the earliest year in the sample.

** State had a major role in financing and operating the key railway serving this market early on; coding for the German markets based on the descriptions of the rail lines in Fremdling, Federspiel, and Kunz (1995), pp. 20-55. The markets outside Germany are coded based on O'Brien (1983) and sources therein We focus on the early period to make our distinction since most railways were nationalized in the late 19th century

Figure A1: Cities in states further away from the seaboard join the *Zollverein* earlier

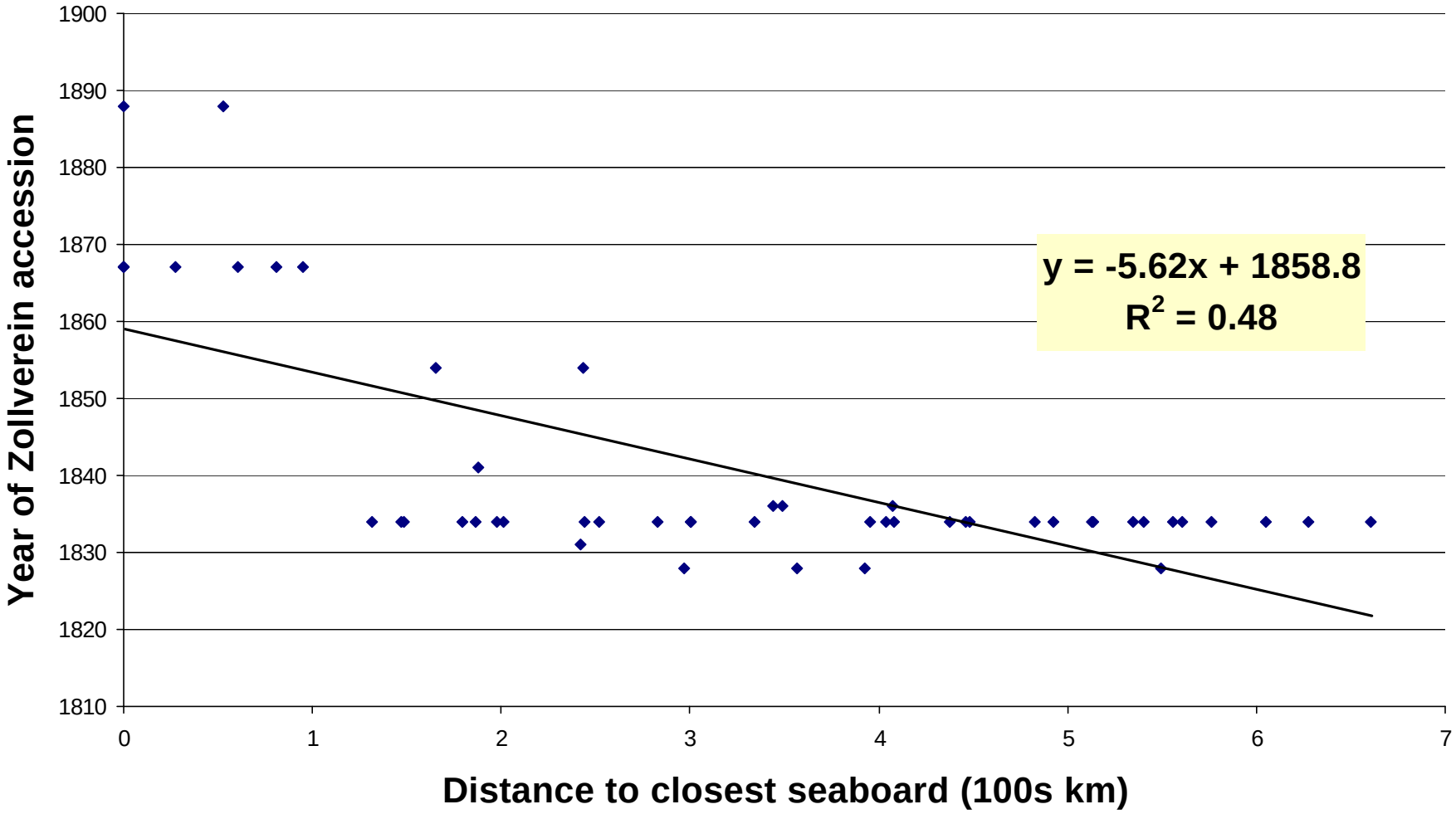

Figure A2: Larger Cities Have Railways Earlier