The Trade Impact of the *Zollverein*[*]

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Abstract

The *Zollverein* was arguably the most important free-trade agreement of the 19th century. This paper investigates the economic impact of the *Zollverein* on trade in Germany. Although 1834 is the official date of the *Zollverein*'s establishment, member states in fact joined in a non-random sequence over several decades. This was because the benefits of becoming a member increased, both as the size of the union increased, and as membership in the union became increasingly important for accessing foreign markets. Our key innovation in this paper is to incorporate the endogenous effects of accession into an estimate of the economic impact of the *Zollverein* customs union. We find these effects are important–our estimated effects are several times larger than the simpler estimates that do not take these effects into account. The paper discusses the implications of this for Germany's economic history as well as for other studies of trade liberalization.

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1 Introduction

The German *Zollverein* of 1834 was arguably the most important regional free trade agreement of the 19th century. It was the first time that politically independent states removed trade barriers between themselves and delegated tariff-setting authority to a higher body. Although other treaties can be found, none were as encompassing or long-lived. Jacob Viner called it the "classic example" of a customs union (Viner 1950, 97). The importance of the *Zollverein* was magnified by the fact that prior to union German states were divided into an extraordinary number of small economic zones that implemented their own tariff collection at the borders. Figure 1 shows an area of about 90 by 60 miles around Frankfurt in the year 1789, with a patchwork of sovereign states (the different shading), each state having its own customs barriers. Clearly, the task for a trader was difficult. The system was simplified after the Napoleonic era, but in the early 19th century there still existed over 300 different administrative political borders in the German states.[1]

The economist Friedrich List, head of the Union of Merchants (*der Deutsche Handels- and Gewerbeverein*), described in colorful terms the scale of the problem in a petition to the German parliament in the year 1819:

> 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

[1] For historical overviews of the Zollverein, see Henderson (1959), Dumke (1976), and Hahn (1984).

spends his days among hostile tax-gatherers and customs house officials. He is a man without country."[2]

The main consequence of the Zollverein was the abolishment of tariff barriers among member states, and the implementation of a single external tariff for non-members. Over time, this reduction of trade barriers may have also had an impact on growth and development in the region (Henderson 1959, Pollard 1982, Bairoch 1989). Given the importance of the *Zollverein* to the history of trade agreements, it is surprising that we still know little on its actual trade impact. While there are studies on overall trends in Europe towards deregulation and trade reform of the 19th century in France, Sweden, and Italy (Federico and Tena 1998, Persson, 1999), the case of the German *Zollverein* is not yet well understood. Its importance in historical accounts overshadows any solid empirical support that would back up the claim that the Zollverein had significant economic consequences for trade. This paper aims to fill the gap.

How much did the *Zollverein* contribute to trade in the customs union? We study the Zollverein's effect on trade in terms of the convergence of wheat prices across forty cities located in fourteen different German states.[3] A simple approach to examine the impact of trade policies compares the fall in price differentials after the official date of the formation of the trade agreement. This approach, we argue, is flawed for the reason that states that joined the customs union early on were likely to be different from states that would join later, and the differences could systematically change the amount of price convergence. Moreover, if part of what allowed the customs union to have the momentum to become established had to do with

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

[3]Price data contains important information on trade and its effects (e.g., Stolper and Samuelson 1941) and has been extensively applied in the literature (O'Rourke and Williamson 1999). Information on the volume of trade becomes available for these areas only at a later time (Wolf, Schulze, and Heinemeyer 2011).

the fact that price gaps were falling anyway in the 19th century due to an intervening, omitted variable, then reverse causation could be a concern.

These observations are important. First, failure to take them into account results in biased estimates of the impact of the *Zollverein* on price convergence. Secondly, the systematic differences between early joiners and late joiners also provide the key for our correction of the biased regression estimate. We take advantage of the unique historical setting in 19th century Germany by predicting the propensity to join the *Zollverein* with market access motives. Although the year 1834 is the official date of the beginning of the German *Zollverein*, the actual process occurred gradually over most of the 19th century. In addition, we show that members joined in a non-random sequence. While there are a range of potential motives that might explain why states joined the *Zollverein*, we find the most compelling explanations consistent with recent studies that have emphasized market access reasons. As the membership of the *Zollverein* under the leadership of Prussia increased in numbers, some German states feared that remaining outside the *Zollverein* would severely reduce their access to the Northern German sea coast and the gains to international trade available from that location (Keller and Shiue 2008, Ploeckl 2010a).[4] In a nutshell, the external border of the *Zollverein* imposed higher costs on the states in Germany's south than those in the north, because the latter did not have to cross the *Zollverein* customs border in order to trade internationally from Germany's coast.[5]

Our main finding is that bilateral price gaps between cities came down significantly with the implementation of the *Zollverein*, by about one-third of the initial price gap, which we quantify

[4]For example, the 1831 accession of Hesse-Cassel to the Prussia-led customs union meant that all goods shipped between southern Germany and the northern ports of Hamburg and Bremen had to pass the external barrier of the Prussian-led customs union (Keller and Shiue 2008); see also Figure 4 below. Ploeckl (2010a) presents a bargaining model for the formation of the *Zollverein* as well as additional evidence on the importance of international trade access.

[5]On endogeneity in a closely related setting, see also Ritschl and Wolf (2003).

as the causal effect of the customs union on price gap declines.[6] We compare this estimate to the naive estimate that does not take into account the endogenous relationship between trade and *Zollverein* membership, finding that the naive estimate would lead to a severe underestimation of the impact of the *Zollverein*.

Our paper contributes to a large literature on market integration using information on grain prices. Most closely related to our paper are studies of Germany (Kopsidis 2002, Shiue 2005) and Europe (Persson 1999, Federico 2011), but also research on market integration between North America and Europe (O'Rourke and Williamson 1999, Federico and Persson 2010). Some of this research has examined a wide range of factors behind the observed increase in market integration, including transport improvements and changes in monetary regimes (Jacks 2005, 2006) and the European demand for wheat from the United States (Uebele 2010). While this paper lacks a comparable scope, our more narrow focus makes it particularly suited for studying the causal impact of customs liberalization.

Taken together, existing studies on Germany in the 19th century have emphasized the importance of lower transport costs to trade and market integration, and that there existed regional differences in the extent of integration. The price study of Fremdling and Hohorst (1979) suggests that a substantial part of the integration of German grain markets occurred already by 1820, whereas Kopsidis (2002) shows that much of the integration in the German state of Westphalia occurred after 1850, with the building of railways. In contrast to these studies, it is not our goal to apportion the relative contribution of different explanations to falling price gaps and trade.[7] Rather, we are after an accurate estimate of the trade effect of the

[6] This overall beneficial effect for trade of the *Zollverein* is consistent with the finding that within individual German states, some regions benefited more while others benefited less (Ploeckl 2010b, 2012).

[7] Other work that has considered in particular railroads includes Keller and Shiue (2008, 2013).

Zollverein through appealing to the historical account of how the *Zollverein* came to be. More generally, this paper has implications for studies on the impact of trade liberalization policies in other countries and time periods in that our results highlight the importance of accounting for the motives underlying the policies. We will return to this issue in the concluding section.

The remainder of the paper is as follows. Section 2 provides the historical background for this study and discusses the motives of different states to join the *Zollverein*, which is crucial to our empirical strategy. The data and our empirical approach is introduced in section 3, where we also present initial evidence on the convergence of prices in Germany during the 19th century. Section 4 expands on the market access motive for joining the *Zollverein*, leading to the specific instrumental variable that is employed in this analysis; we also provide evidence on the validity of our approach before turning to the main results. The concluding section 5 summarizes our results and puts them into perspective for future research.

2 Germany's nineteenth century trade: the *Zollverein* treaties

In this section we give a brief historical account of the *Zollverein*.[8] After the defeat of Napoleon in 1815, Germany's political structure was divided into the thirty-nine states of the German Confederation (*Deutscher Bund*); Figure 2 shows the borders in the year 1820. 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 trade barriers that were imposed were complicated.

[8] On the following, see also Henderson (1939), Hahn (1984), as well as Dumke (1976), Ploeckl (2010a).

Economists and businessmen were typically opposed to the existing customs system with numerous trade barriers, but they were not the only ones. Goethe, for example, told an acquaintance that he would look forward to a time when his luggage would pass unopened through all thirty-six German states.[9] The idea that Germany's numerous customs borders were a hindrance to trade and economic development, as well as political unity, was widely held. Figure 3 illustrates the prevailing popular sentiments towards customs liberalization at the time: a man axing a customs barrier. Observers frequently drew unfavorable comparisons with other European countries. In the words of Friedrich 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 [i.e., France], from the Canal to the Mediterranean 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."[10]

Although in the minority, there were also some voices opposing economic liberalization in Germany, especially in the early 1800s. They included political progressives, who typically did not oppose liberalization *per se* so much as liberalization under the leadership of Prussia, which they considered politically undesirable.[11] Also the nobility of the smaller and mid-sized German states was often hesitant about economic liberalization, mostly because they feared the possibly accompanying political changes that might result in a loss in their personal power. However, at times the economic disadvantages of not joining the *Zollverein* even for the nobility were overwhelming. For example, Ludwig I, the king of Bavaria, strongly supported customs

[9] Johann Wolfgang von Goethe in conversation with Eckermann in the year 1828; see Goethe (1828).
[10] Reprinted in German in von Eisenhart Rothe and Ritthaler (1934), pp. 320-324.
[11] Several German states had by then adopted constitutions, which Prussia had not.

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). Thus, opposition to liberalization waned over time as there came to be an increasing recognition that Prussian leadership offered the only viable solution for German customs liberalization.

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 from the Prussian standpoint 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.[12]

Hesse-Darmstadt was the first territorially separate state to join the Prussian Customs

[12]However, throughout, Prussia reserved the right to negotiate with foreign countries for itself.

Union in the year 1828; Hesse-Cassel became the next to join in 1831. The latter was significant because for the first time the east and west portions of Prussia were joined together without an intervening customs border in between; Figure 4 shows the location of Hesse-Cassel ("*Kurhessen*"), linking the east and west portions of Prussia. Now British goods could no longer reach Frankfurt and Germany's south without crossing a Prussian external tariff border. In the year 1834, the Thuringian states, the Kingdom of Saxony, and the 1828 formed South German Customs Union (consisting of Wurttemberg and Bavaria, see Figure 4) 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.

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. Of the imports from foreign countries, a substantial share were goods that were not produced in Europe (such as tobacco, sugar, and spices). In fact, during the period of 1833 to 1842, more than 50% of the Zollverein revenue was due to such colonial goods (*Kolonialwaren*; see Dumke 1976, p.92). Therefore, the trade diversion effect of the *Zollverein* was rather limited, and the basic character of the *Zollverein* should be viewed as trade-liberalizing.

By stages, other states entered. Three states joined the *Zollverein* between mid-1835 and early 1836: Baden, Hesse-Nassau, and the Free City of Frankfurt. The entry of Baden, in Germany's far southwest, was important because with the entry of Baden the two separate areas of Bavaria were joined without custom borders (see Figure 4). The entry of Frankfurt was significant because it created the possibility 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 of customs union enlargement materialized over a large part of the century (the years 1828 to 1888).[13]

Motives for Joining the Customs Union The expectation of efficiency gains, lower prices, and higher welfare might have been a strong *a priori* argument for the formation of the *Zollverein*. It was the welfare of the feudal lord that counted, however, not that of the population at large. States retained political sovereignty, and so Prussia could not force individual states to join against the will of the sovereign. Moreover, it is well-known that even in fully-fledged democracies the gains from free trade might be hard to reap, either because losers oppose the move to free trade or because gains from trade are dissipated in the political process through lobbying or rent-seeking.

What were the primary motives for when individual states joined the *Zollverein*? Some of them were clearly idiosyncratic. For example, Hanover, joined relatively late in part because it was governed in personal union with England, which had no interest in a Prussian led customs union dominating the center of Europe. Other motives that have been proposed are systematic but inconsistent with the evidence. For example, we can easily rule out the argument that the non-joiners did so because they preferred higher tariff rates than Prussia was proposing. In fact, Prussia's tariffs on a range of goods, especially colonial goods such as tobacco, tea, and

[13] Austria-Hungary did not become a member of the *Zollverein*.

sugar, were higher than the tariffs of many German states before they joined the *Zollverein*. Bavaria and Wurttemberg right before they entered the *Zollverein* in 1834, for instance, had *ad-valorem* tariff rates of 47% on *Genussmittel* (non-essential consumption goods), whereas the tariff rate of the Prussia-Hesse customs union for these goods' was 73% (Dumke 1976, Table 3.16). Therefore the desire for more protection can hardly be the main reason for not joining the *Zollverein*.

We also discount the possibility that fiscal reasons played the most important role. Certainly, it was noted at the time that it may be prohibitively costly for some of the smaller states to establish and enforce tariff borders on their own–smaller states have a higher border length to area ratio (Kuehne 1836). If this is the case we should expect smaller states, and highly indebted states, to prefer joining the Prussian-led customs union in exchange for receiving some fraction of the joint tariff revenue, and to some extent they did (Dumke 1976, Chapter 1). These fiscal reasons, however, cannot be the full explanation because there were several highly indebted and small states that joined the *Zollverein* relatively late. A different fiscal reason may have mattered, namely that the benefits of joining would get larger as the customs union expanded.

We argue that market access was the major motive. In Figures 2 and 4, for simplicity let us distinguish only three regions: the North with states such as Hannover and Mecklenburg, the Center with Prussia, and the South including Baden and Bavaria. There are some reasons why *both* the North and the South had an incentive to join the *Zollverein*, most importantly that joining would give tariff-free access to the large market of Prussia, which included the leading industrial areas of Germany at the time. However, there were also reasons that were *specific* to the South for joining the *Zollverein*, because if it did not traders would have to pay

hefty Zollverein tolls in order to reach the Baltic or North Sea coast. The coast was important for a number of reasons. First, machinery and equipment goods from the emerging industrial power, England, landed here. Moreover, the Baltic and North Sea coast was the main port of sea access for the Southern states', since the Alps effectively blocked off trade to ports in the south. Thus, the Southern German states of Baden, Württemberg and Bavaria had all joined the *Zollverein* by 1836; this is seen from Figure 5. In contrast, Mecklenburg and the city states of Hamburg and Bremen, which relied heavily on international trade, joined only in 1867 and 1888, respectively. In sum, the key explanation for when states joined the *Zollverein* is market access. We will build on this observation in the empirical analysis below.

The following section describes the data.

3 Data

We employ information on the price for wheat in Germany to analyze trade as it leads to the convergence of prices. The data set consists of forty city markets in fourteen different German states. Table 1 gives an overview of the data. Generally in Europe in the 19th century, grain prices increased from the South (towards the Black Sea) and East (Eastern Prussia) to the Northwest (Northern Germany, the Canal region, and England). Starting from the average annual wheat prices in each of the forty cities, we have computed the city's percentage price gap to each of the other cities in the sample.[14]

The overall sample period is 1820 to 1880. We focus on the central sixty years of the 19th century because during the years 1800-1820, trade was strongly disrupted by wars, whereas by

[14]For this we first converted the many different quantity and monetary units that were used in 19th century Germany using conversion rates given in Seuffert (1857) and other sources; see the Appendix for details.

the years 1880 to 1900 Germany had become politically unified, which may have generated a new environment for trade altogether. Data is employed every five years (1820, 1825, etc.) to reduce the impact of serial correlation on the results. Within the overall sample period of 1820 to 1880, the range of years for which wheat prices are available varies (as given in Table 1). The table also presents the number of price gap observations for each city. For example, Aachen is included as city j *or* k in 252 cases. Summing across all forty cities there are 7,140 cases, which corresponds to 3,570 bilateral price gap observations in the sample.

Our dependent variable, $Trade_{jk}$, is defined as the absolute value of the percentage price gap for wheat between cities j and k in a particular year. This measure is informative on trade because it is a modified "law of one price". Suppose Nurnberg exports a unit of wheat to Frankfurt, and Frankfurt charges a customs duty of τ_{FN}. In the absence of other costs and with competitive markets, the price of wheat in Frankfurt in this period is going to be equal to that in Nurnberg, plus the customs duty, or, the customs duty is equal to the excess of the price in Frankfurt over that in Nurnberg

$$P_F - P_N = \tau_{FN}. \tag{1}$$

While equation (1) gives the main motivation for studying the impact of customs liberalization- which reduces τ– on price gaps, two points should be kept in mind. First, the price gap captures more than customs duties. In particular, we expect transport costs, d_{FN}, to affect the price gap between Nurnberg and Frankfurt. The institutional setting matters too, such as the strength of contract enforcement for trades between Nurnberg and Frankfurt. Alternatively, if markets are not perfectly competitive there could be mark-ups charged by middlemen; denote the costs

capturing this by i_{FN}. The excess of the price in Frankfurt over that in Nurnberg is then

$$P_F - P_N = \tau_{FN} + d_{FN} + i_{FN}, \tag{2}$$

which shows that changes in customs duties need not change price gaps one for one. Second, we do not have information on whether the wheat in Frankfurt was shipped from Nurnberg, or from somewhere else.[15] The price in Frankfurt is the minimum across all possible sources: $P_F = \min_q \left[P_q + \tau_{Fq} + d_{Fq} + i_{Fq}\right]$, and the information provided by any given price gap, such as to Nurnberg, gives the upper bound of the bilateral transactions costs:

$$P_F - P_N \leq \tau_{FN} + d_{FN} + i_{FN}. \tag{3}$$

In our sample, the mean absolute value of the percentage price gap between two markets is about 0.18 at the beginning of the sample, and around 0.05 towards the end. Thus the average price gap fell by about 70%.[16] This reflects the dramatic extent of price convergence in Germany over the 19th century. There is considerable variation in price gaps across city-pairs in the cross-section. This is in part due to differences in bilateral distance, affecting transport costs, as well as other factors. In the analysis below we will include city-pair fixed effects to address some of these issues. Further, the decline in price gaps is relatively stronger for initially high price gaps than for low price gaps. At the 10th percentile, the price gap fell from 0.026 at the beginning to 0.016 by the end of the sample period, or, by 38 percent, while at the 90th

[15] Donaldson (2010) can infer transactions costs from price gaps because some of the goods he is studying are produced in only one region. Here, this is clearly not the case.

[16] We compute the absolute value because the local price of wheat is affected by weather, and from year to year it might change which city is the low-cost producer.

percentile the price gap fell from 0.39 to 0.09, or by 76 percent.[17]

For each city we have recorded the year in which it became part of the *Zollverein*; this year is listed in Table 1.[18] Generally, joining the *Zollverein* meant that barriers for wheat 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. However, in the year 1831 the augmented Prussian customs union charged a specific duty on wheat equivalent to about 7% *ad valorem* (Dumke 1976, Table 3.15). Moreover, the *ad valorem* equivalents for "products of agriculture" immediately 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). Based on the available information, we estimate that the duties on wheat may have been on average the equivalent of about 10 percent *ad valorem* before they were reduced to zero in the Zollverein liberalizations. Instead of the tariff rates, we will exploit the timing of the move towards zero trade barriers using a dichotomous 0/1 variable.[19]

Eliminated from the sample are the city pairs between which there were never any customs barriers during the sample period. It is well known that trade and price arbitrage *across* states is weaker than *within* states, for reasons that are not fully understood. In the literature, this finding is referred to as the 'border effect' (Shiue 2005 analyzes 19th century Germany).[20] For

[17] Given this difference, we do not necessarily expect that customs liberalizations have the same effect throughout the sample.

[18] For the Prussian cities in the sample, we give the year 1828, which is the earliest year at which another state became part of the Prussian-led *Zollverein*. Customs liberalizations that did not involve *Zollverein* accession are discussed in section 4.

[19] 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 *Zollverein* treaty. We focus nevertheless on the *Zollverein* accession date, because arguably this played the key role in terms of commitment.

[20] These excluded observations are typically within-state market pairs. The exception to this are observations where the territory of states consisted of several non-contiguous parts, such as the Eastern and Western provinces of Prussia.

each market pair in our sample, we have established using historical maps whether a direct trade route would have to cross any customs borders. If the number of customs borders to be crossed is greater than or equal to one, the variable $CustLib_{jkt}$ is coded as 0, otherwise it is 1, for each market pair jk and year t. For example, the customs variable $CustLib_{jkt}$ turns to 1 for the pair Berlin-Nurnberg in 1834 (when Bavaria joined the *Zollverein*), while it changes from 0 to 1 for Berlin and Parchim in the year 1867, when Mecklenburg joined the *Zollverein*.

To illustrate our approach, consider the customs liberalizations of the year 1834. In the following analysis, we distinguish the city-pairs for which tariffs were abolished in the year 1834 from the other observations for which customs were liberalized in another year, or not at all during the sample period. In Figure 6 we show the average price gap for both of these groups during the ten years *before* the 1834 liberalization, versus the ten years *after* the 1834 liberalization.[21]

The figure shows that before the 1834 liberalization, the average price gap between cities that would reduce their customs barriers was around 0.22, somewhat higher than the value of 0.17 for the cities that would not liberalize in 1834. After the 1834 liberalization, the typical price gap for the liberalizers was around 0.15, down by 0.07, whereas the non-liberalizers' price gap fell only by 0.01 to about 0.16 on average. Thus, these difference-in-differences results for the 1834 customs liberalization are supportive of the idea that customs liberalizations had a substantial effect on price convergence and trade in 19th century Germany. The regression analysis below extends the analysis underlying Figure 6 in three important ways. First, instead of focusing on the customs liberalization event of 1834, we pool across many such events during

[21] Recall that we employ data every five years, so the pre-period years are 1820, 1825, and 1830, while the post-period years are 1835, 1840, and 1845.

the 19th century. Second, we focus on price gap changes within each city-pair, which is useful for a number of reasons, including to reduce the influence of changes in the sample composition. Third, and perhaps most importantly the regression analysis below corrects for the non-random sequence with which states became members of the *Zollverein*. As noted above, the net benefit of each state to join the *Zollverein* depended on the state's access to international markets, with and without *Zollverein* membership. To create a measure of this international trade access we calculate the distance between each city and the nearest coastal port. Table 1 gives these distances for each of our forty cities.

We will also consider a number of other factors in the following analysis. Information on these variables, as well as further data details are given in the Appendix. We now turn to our results.

4 Empirical Results

In this section we begin by considering the formation of the *Zollverein* as an endogenous process, followed by the introduction of our market access instrumental variable (IV). Next, we produce reduced-form results that shed light on the importance of other determinants of 19th century German trade, as well as on the validity of our IV. Finally, we show IV estimates of the causal effect of the *Zollverein* on trade.

Our goal is to obtain a valid regression estimate of the impact of customs liberalization ($CustLib$) on trade ($Trade$). Let ε be the regression error. Correlation between ε and customs liberalization would lead to a biased OLS estimate. For example, if the states that expect the largest trade benefits from joining the *Zollverein* do it while others do not (or join later),

OLS will overestimate the gains from customs liberalization. This would be reverse causation, where the expected gains in $Trade$ determine the decision to liberalize customs. Even in the absence of this, endogeneity through omitted variables is a concern. For example, if *Zollverein* accession was more likely for relatively small states at the same time when the potential customs liberalization gains for these states would be relatively small, OLS would underestimate the impact of the *Zollverein*.

The instrument For the instrument to be valid, it cannot be correlated with the regression error ε. This could either arise from a direct effect of the IV on $Trade$ or from a correlation of the IV with another determinant of $Trade$. Because ε is unobserved the validity of the IV cannot be proven, however, below we will provide evidence on instrument validity by estimating variants of the reduced form regression. The instrument for customs liberalization is the relative market access variable $RDistCoast$, defined as follows:

$$RDistCoast_{jkt} = \frac{DistCoast_{jk}}{[\varnothing DistCoast|Not\_ZV_t]}, \forall jk, t \tag{4}$$

where $DistCoast_{jk}$ is defined as the average distance to the nearest coast for cities j and k

$$DistCoast_{jk} = 0.5 \times (DistCoast_j + DistCoast_k). \tag{5}$$

The expression $[\varnothing DistCoast|Not\_ZV_t]$ is the average distance across all market pairs lq to the nearest coast that are *not* yet part of the Zollverein customs union, as of year t:

$$[\varnothing DistCoast|Not\_ZV_t] = \left[\frac{1}{N_t}\sum_{lq=1}^{N_t} I_{lqt} \times DistCoast_{lq}\right], \tag{6}$$

where I_{lqt} is an indicator variable that is one if markets l and q in year t are not yet both part of the Zollverein (and so customs borders do still exist).

Note how this IV builds on the motives to join the *Zollverein* that is stressed by historical accounts. First, the numerator of equation (4) captures the fact that a state's accession to the *Zollverein* was related to the distance to the coast because that gave access to international markets. Markets more distant to the coast joined earlier. Consequently, 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*, recall Figure 5. As one would expect, based on this there is a strong negative relationship between the distance to the nearest coast of a city and the year of *Zollverein* accession. For the first-stage regression–the potentially endogenous variable $CustLib$ regressed on the instrument $RDistCoast$–this means we expect a positive coefficient.

Second, the denominator of equation (4) captures the fact that as the *Zollverein* became larger the net benefit of joining *increased* over time. Clearly, a larger *Zollverein* meant more potential customers to which to sell customs-free. Moreover, a larger *Zollverein* raised the chance of having to pay customs duties even when selling to non-*Zollverein* members because a larger *Zollverein* meant that the external *Zollverein* border would cover a wider geographic area. In this way the instrumental variable picks up the fact that Nassau, Frankfurt, and Baden joined in 1836, only two years after the *Zollverein* was founded: it is plausible that the leaders of these states had come to the conclusion that staying outside of the *Zollverein* had just become prohibitively costly. This rising cost of staying outside the *Zollverein* is captured

by the IV because the denominator of *RDistCoast* declines over time (states far from the coast tend to join early), so that even for a given own distance to the coast the propensity to join the *Zollverein* is increasing over time.

Reduced-form results We now explore the reduced-form regression to shed light on the validity of this instrumental variable. The reduced-form is given by

$$Trade_{jkt} = \gamma_1 rdistcoast_{jkt} + \mathbf{X}'\boldsymbol{\gamma} + u_{jkt}, \tag{7}$$

where vector $\mathbf{X}$ includes city-pair fixed effects and time fixed effects, γ_{jk} and γ_t, and $rdistcoast$ is equal to the log of $RDistCoast$, plus one. The fixed effects reduce omitted variable concerns. In particular, the γ_{jk} imply that identification comes from the changes in price gaps over time within each city-pair.

The relative distance variable is based on geographic characteristics of city markets j and k, as well as on the *Zollverein* accession decisions of all states. Each individual state's decision has only a small impact on (the denominator) of the instrument, and geography is quite plausibly exogenous, so we estimate equation (7) by OLS. The results are shown in Table 2, first column. A high relative distance to the coast leads to lower price gaps. This is consistent with the idea that city pairs that are relatively distant from the coast join the *Zollverein*, and the customs liberalizations of the *Zollverein* bring down the bilateral price gap.

The remaining columns of Table 2 show variants of this reduced-form regression. In each column we introduce a different variable, Z

$$Trade_{jkt} = \gamma_1 rdistcoast_{jkt} + \gamma_2 Z_{jkt} + \mathbf{X}'\boldsymbol{\gamma} + u_{jkt}, \tag{8}$$

where Z is a potential determinant of $Trade$ which might also be correlated with $rdistcoast$. It is important to distinguish these two features. As we will see, there are a good number of variables that affect $Trade$ in the sense that γ_2 is estimated to be non-zero. In itself, that is not a concern for the IV strategy. However, if γ_2 is estimated to be non-zero *and* the coefficient on relative distance γ_1 turns insignificant, that would be evidence that relative distance is strongly correlated with another factor, Z, that affects $Trade$. In that case, relative distance could not be used as IV for customs liberalization because it fails the exclusion restriction: it would not be clear whether the IV picks up the *Zollverein* liberalizations or something related to that other factor, Z.

The first variable Z that we examine is related to the fiscal argument of why states joined the *Zollverein* noted above. It is the log of the average ratio of state border length to state area, which according to contemporary estimates captures the cost-benefit ratio of joining the *Zollverein* from a fiscal revenue perspective (Kuehne 1836; see also Dumke 1976). Given the lack of time variation in this variable, we interact it with dummies for each of the three twenty-year periods in our sample, with the last twenty years (1860-1880) being the omitted category.

Results are shown in column 2 of Table 2. The coefficients on the border-to-area ratio are positive, indicating that price gaps for city pairs located in states with high border-to-area ratios tend to be relatively high. At the same time, the coefficients are not precisely estimated, and the F-test for the inclusion of the border-to-area variables reported at the bottom of Table 2 suggests that border-to-area may not belong into the equation. Importantly, even with the inclusion of the border-to-area variables our proposed instrumental variable, $rdistcoast$, remains highly significant (and of the same sign).

Next we consider institutional change. Many of the German states underwent institutional

change as a consequence of French occupation during the times of the French revolution and in Napoleonic times. These institutional changes tended to be pro-business, in particular in many areas equality before the law was established, and the influence of craft guilds, which typically would restrict the entry of newcomers in an industry, was curtailed (Acemoglu, Cantoni, Johnson, and Robinson 2011). Moreover, one of the consequences of these institutional reforms was to benefit trade (Keller and Shiue 2013). As a consequence, the institutional change in the German states during the early 19th century might affect our IV strategy: what if these institutional reforms determined which state joined the *Zollverein*, and not their relative market access as captured by *rdistcoast*?

A good measure of the depth and the extent of irreversibility of these institutional changes, it turns out, is the length of French occupation (Acemoglu, Cantoni, Johnson, and Robinson 2011). We have added the log of the average length of French of occupation in cities j and k into the reduced-form as the next Z variable. The results are shown in column 3 of Table 2. The negative coefficients indicate that longer French occupation led to lower price gaps, consistent with the idea that French occupation triggered institutional improvements that benefited trade. The F-test at the bottom of column 2 indicates that, in line with Keller and Shiue (2013), French occupation is a significant determinant of price gaps. Importantly, the impact of French occupation is largely orthogonal to that of the relative distance to the coast; the coefficient on *rdistcoast* in column 3 is quite similar to that in column 1. Thus, whatever the impact of institutional change on trade might have been, there is no evidence that it will prevent us from estimating the causal impact of the *Zollverein* on trade using this market access instrument.

According to much of the literature, the introduction of steam trains has been second to none in importance for improving trade and causing economic growth in 19th century Germany

(Fremdling 1975). It has also been noted that the *Zollverein* facilitated railway construction. It was easier to agree on the building as well as the location of the railway tracks when all parties were members of the *Zollverein*, because at a minimum it was not tariff considerations that made individual parties favor different solutions (Hahn 1984, 93). It is therefore not implausible that railway building both affected trade *and* was correlated with *Zollverein* accession. To examine what this means for the IV strategy, we include a railway measure, namely the GIS-based cost of railway building based on the difficulty of the terrain (from Keller and Shiue 2013) in the reduced form. According to column 4 of Table 2, railway costs affect price gaps, however they do not much affect the proposed instrument.[22] We also examine whether city size in the year 1800 significantly affected the speed of price convergence in the 19th century, finding that this has not been the case (see column 5).

In column 6 we include the share of Protestants in the population as additional Z variable. Protestantism has been proposed as a driver of economic performance (Weber 1930), and that may include trade arbitrage.[23] Protestantism is indeed significant as a determinant of price gaps (bottom of column 6). In Germany's North the share of Protestants is larger than in the South (where the majority is Catholic). Including Protestantism changes the coefficient on relative distance to the coast. Also the inclusion of latitude raises (in absolute value) the coefficient on *rdistcoast*, albeit less so (column 7). This is not surprising because the nearest coast for our sample cities is typically in North-South direction to the North- and Baltic Seas, so that *rdistcoast* negatively correlated with Protestantism or latitude. The fact that *rdistcoast* is still

[22] High railway costs have a (imprecise) positive point estimate in the 1840 to 1860 period, consistent with high costs delaying railway building, which itself led to relatively high price gaps. The negative coefficient for the 1820 to 1835 period is less important, because at the time steam railways had not yet been introduced to Germany.

[23] Becker and Woessman (2009) focus on the link between Protestantism and human capital accumulation.

significant upon inclusion of these variables indicates that it contains market access variation beyond what is captured by differences in religion (or latitude). Controlling for the East-West dimension does not much affect the estimate relative distance, as the results for longitude in column 8 show.

We also consider the role of remoteness for our results, defined as the geographic location of a particular city-pair relative to the mean of the sample.[24] Customs liberalization between two relatively isolated markets may matter more than customs liberalization between two markets that each have a multitude of alternative trade partners nearby. Including remoteness also sheds some light on general-equilibrium effects that might be present.[25] Table 2 shows that remoteness is associated with lower price gaps in the sample (column 9). Including remoteness also reduces somewhat the size of the coefficient on *rdistcoast*, however, relative distance to coast remains highly significant.

Next, we examine the role of shipping routes, which may be important because the 19th century saw the widespread adoption of steam ships in Germany (column 10 of Table 2). The variable Shipping NS is equal to one if the drainage areas in a particular state feed into rivers that flow into either the North- or the Baltic Sea (the alternative being that they flow, via the river Danube, into the Black Sea). Including this variable raises (in absolute value) the coefficient on relative distance to coast, which may be due to the geographic flavor of the Shipping NS variable. At the same time, it poses no risk for our IV strategy.

[24] If lat_{jk} and $long_{jk}$ are the average latitude and longitude of city pair jk, then we define $rem_{jk} = \left[\left(lat_{jk} - \overline{lat}\right)^2 + \left(long_{jk} - \overline{long}\right)^2\right]^{0.5}$ as city pair jk's remoteness, where $\overline{lat}$ and $\overline{long}$ are the sample averages across all lat_{jk} and $long_{jk}$, respectively.

[25] Anderson and van Wincoop (2003) have shown in a gravity equation framework that such general equilibrium effects are picked up by so-called multilateral resistance terms, which perform the same function as our remoteness variable.

Another channel that might have affected price gaps is international trade. We know that the 19th century saw the arrival of large grain shipments from the United States (O'Rourke and Williamson 1999). The United Kingdom, in particular, went from importing 0.6% of its wheat from the United States in the period 1841 to 1845 to 54.2% from the United States during the years 1880-84 (Dumke 1976, 231-232). Also imports of industrial goods from England might have had a substantial effect on price gaps. An increase in the integration of international markets should primarily affect the coastal areas in Germany, and in colum11 we include an indicator variable for city-pairs that are relatively close to the coast. We estimate that city-pairs located near the coast tended to have higher price gaps, and controlling for that strengthens somewhat the *rdistcoast* coefficient.

We have also explored the reduced form regression where each variable is interacted with a time trend instead of period fixed effects (for example, the average share of Protestants in cities j and k times *year*). This more structured approach, which assumes that the effect of Z changes monotonically with time, leads to similar results as those of Table 2 for the relative distance to the coast variable.[26] Overall, these results are in support of our strategy of using the relative distance to the coast as the instrument.

In the next section we turn to the instrumental-variable estimation results.

[26] We have also included bilateral geographic distance in the reduced form, interacted with period dummies, as a control for differential changes in transport costs for short- versus long-distance trade. The results are similar.

4.1 The impact of customs liberalizations on trade

This section contains our estimate of the impact of customs liberalization on trade. It is given by β_1 in the following equation:

$$Trade_{jkt} = \beta_1 CustLib_{jkt} + \mathbf{X}'\boldsymbol{\beta} + \varepsilon_{jkt}. \tag{9}$$

Customs liberalization is instrumented by the Relative Distance to Coast variable given in equation (4). The vector X includes year fixed effects (β_t) and city-pair fixed effects (β_{jk}). The method of estimation is two-stage least squares (TSLS). Results are given in Table 3.

The customs liberalization coefficient is negative at about -0.05 (column 1), which indicates that customs liberalization has brought down price gaps and thus improved trade. Inferences in column 1 are based on robust standard errors consistent with arbitrary heteroskedasticity; the p-value of the customs liberalization estimate indicates that it is highly significant at standard levels.

What about the first-stage results? The Relative Distance to Coast instrument has a positive coefficient. The sign confirms that cities in states that are far away from the coast (such as Munich) tended to become members of the *Zollverein* relatively early compared to cities close to the seaboard (such as Hamburg or Bremen). IV estimation can lead to biases when the instrument is weak in the sense that the correlation of the instrument with the endogenous variable is low. We assess the power of the instrument by the F-statistic of the first stage. Here this F-statistic is larger than 90, which means the instrument is strong.[27]

[27] Staiger and Stock (1997) formulated the rule of thumb that this F-statistic should be at least 10. Angrist and Pischke (2009, pp. 205-218) analyze the IV bias in detail and provide an update on Staiger and Stock's (1997) rule of thumb.

We also report the OLS estimate of β_1, which would be preferred to TSLS (because of lower variance) if customs liberalizations had been exogenous. The OLS estimate is around -0.015, closer to zero compared to the TSLS estimate. This may be in part because the IV approach addresses the measurement error in the 0/1 customs liberalization variable, which does not use the specific size of the tariff cuts in each liberalization (attenuation bias). In addition, the endogenous sequencing of membership accession here led to city-pairs joining first that would end up experiencing relatively small reductions in price gaps. A test of endogeneity indicates that the null of exogeneity can be rejected at standard levels of significance. Because there is evidence that OLS estimates are inconsistent, we will focus our attention on the TSLS estimates.

Clustering, size, and the role of Prussia Table 3 also reports results for clustering at the city-pair level. Allowing for arbitrary heteroskedasticity and serial correlation between cross-sectional observations across panels may be important if specific city-pairs are affected by shocks over the 19th century. Looking at the results in column 2, however, we see that this clustering does not change the inferences. Given the bilateral definition of the price gap variable, some dependence between observations at the city level must be present, because if a shock increases the price of wheat in city j, this will affect the price gap of city j with *all* other cities. The third column of Table 3 shows results for clustering at the level of the city, as opposed to the city pair. Clustering at the city level affects inference somewhat, as the customs liberalization coefficient is now only significant at a 2% level, however qualitatively the results are unchanged.[28]

[28] We also report the more general Kleibergen and Paap (KP) F-statistic in addition to the usual first-stage F-statistic. The KP is often compared with Stock and Yogo's (2005) critical values to gain additional information

Column 4 shows results from weighing each observation by city size, which gives more weight to the relatively large cities. While the customs liberalization estimate is somewhat larger, overall the results are quite similar to before. Further, does the fact that some cities were in Prussia matter for the results? This is an important question because the Zollverein has been seen at times as Prussia's vehicle to not only achieve economic but also political unification. In the final column of Table 3, we drop Prussian cities from the sample. The results are quite similar to before. Thus, Prussian cities do not appear to play a major role for the results.

Because the following analysis essentially confirms the findings so far–customs liberalization led to price convergence, with an IV estimate of around -0.05, we discuss at this point the magnitude of this estimate. IV estimation has sometimes led to somewhat surprising findings, so how reasonable are our estimates? The IV estimate is significantly larger than the OLS estimate, about three times the size. This is a substantial difference. At the same time, the OLS estimate is close to zero, so a tripling of this size is still not a very large number. To put this in perspective, the customs liberalization effect, while sizable, is smaller than the trade impact of steam trains in 19th century Germany (Keller and Shiue 2013). Moreover, as we noted above the mean price gap in the 1820s is around 0.18, which means that on average customs liberalization has brought price gaps by a little less than one third during this period. While this estimate appears to capture more than the elimination of tariffs, its magnitude is arguably reasonable.

We now present evidence on a number of additional issues.

on the strength of the first stage (even though Stock and Yogo's critical values are for the i.i.d. case). In our case, the KP statistic is far larger than Stock and Yogo's critical values, confirming that the first stage is strong.

Third Market and General Equilibrium Effects By focusing on city-pairs our analysis might abstract from sizable general equilibrium effects, both because of the liberalization of third markets and through the general trends towards protectionism in Europe during the 19th century. The *Zollverein* effect might have been different depending on specific circumstances. For one, while the external tariff of the *Zollverein* and its precursors on wheat was constant for the period from 1825 to 1851 (Oechselhaeuser 1851), the benefit from joining also depended on the level of tariffs between non-*Zollverein* members. While we do not have the information necessary to fully trace out these effects, the single biggest event in this respect arguably took place in the third quarter of the 19th century, when many countries and independent states liberalized their trade. 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 order to see the impact of these third-market considerations on the customs liberalization estimate, we present results under the assumption that for the years 1855 to 1875, customs were liberalized between all sample cities, both inside and outside the *Zollverein*. Column 1 in Table 4 shows that this leads to a higher customs liberalization estimate. It may be explained in part by the fact that price gaps tended to be higher in the early period, and given that the recoding for 1855 to 1875 leaves less variation in $CustLib$ for the later period the coefficient rises (in absolute value) because it is primarily identified from the larger changes in price gaps of the early period.

Another way of assessing the importance of third-market effects is to consider the state capitals in the sample. It is reasonable to assume that state capitals tend to be particularly

important for inter-state trade, both because they account for a relatively high share of all trade and because they may serve as hubs for smaller cities. In column 2 we drop all observations between state capitals from the sample. This leads to a slightly smaller customs liberalization estimate, at -0.041 versus -0.055. Overall, while these results suggest that including third-market effects can lead to either a higher *or* lower *Zollverein* estimate, it is unlikely that our analysis gives a gross overestimate of the impact of customs liberalization in the 19th century.

State-level Clustering Because the decision to join the *Zollverein* was a political decision taken by the state, all cities of a state would typically be affected equally.[29] Therefore we have also considered clustering at the level of each state-pair instead of city-pair. As seen from column 3 of Table 4, state-pair clustering does not qualitatively affect the inferences: the p-value on the customs liberalization estimate is about 5%, and the first-stage F-statistic, with a Kleibergen Paap F-statistic of about 44, remains strong. We can also cluster at the state-pair by year-level. The precision of the customs liberalization estimate falls but remains significant at a 10% level, see column 4.

Sample Robustness The remainder of Table 4 report estimates for a number of important sample restrictions. We begin by eliminating observations with a Bavarian city from the sample. Bavaria, the second largest German state, was closest to being a serious rival for Prussia during the 19th century (after Austria-Hungary). Moreover, Bavaria is also highly represented in the sample. In the restricted sample we estimate the customs liberalization effect somewhat

[29] This was not always the case. For example, the South German Customs Union abolished tariffs between Wurttemberg and Bavaria in the year 1828. Nonetheless, the Bavarian town of Zweibruecken continued to face customs borders in its trade with Wurttemberg because Zweibruecken was located in a geographically disjoint part of Bavaria (Palatinate, or "Pfalz"), see Figure 4.

higher than before (column 5). Further, the city states of Bremen, Frankfurt, Hamburg and Luebeck had quite different characteristics than the area states in the sample, and one might be concerned that this might include their response to customs liberalization. It turns out that dropping observations from city states does not change the results by much, see column 6 of Table 4. We have also systematically eliminated the observations that have the most influence on our estimate, as judged by Cook's Distance. Influential observations do not appear to drive our results, see the estimates in column 7. Finally, we have also explored the role of the unbalanced sample for these results. A focus on those city-pairs where price information is available for the majority of years during the sample period leads to a similar customs liberalization estimate as for the sample as a whole, see column 8 of Table 4.

Other Factors We have also explored the influence of the factors that featured in the reduced-form analysis reported in Table 2. Results are given in Table A of the appendix. In the first column, we repeat the baseline results for convenience (from Table 3, column 2). The next column in Table A gives results for the subsample from which observations with relatively *low* Border-to-Area ratios are eliminated.[30] Analogously, in column 3 we drop observations with low Railway Costs while in column 4 observations with low Population are eliminated, and so forth. These results show that the estimation results for the customs liberalization effect are for the most part close to the baseline in column 1.[31]

[30] Specifically, the first quartile of the sample in terms of Border-to-Area ratio is dropped. While we cannot extend this analysis to the three additional factors analyzed in Table 2 (French Occupation, Shipping NS, and Coast) because of the discreteness of these variables, we have confirmed using other methods that the customs liberalization results are robust to the influences of these factors as well.

[31] The exception to this is Border-to-Area, where it appears that the customs estimate is relatively large for high border-to-area observations, compared to the average city-pair in the sample. To the extent that the high Border-to-Area observations are city state observations, though, we have seen in Table 4 that eliminating city state observations does not lead to a very different customs liberalization estimate.

All in all, our analysis confirms a significant impact of customs liberalization on trade. Quantitatively, the preferred IV estimate gives a value of around -0.05.

5 Concluding Discussion

This is the first study, to our knowledge, that incorporates the endogenous effects of accession into an estimate of the economic impact of the *Zollverein* customs union. We find that the estimate is larger than the simple OLS estimate that does not take these effects into account, and may help to explain why the trade effects of the *Zollverein* have not received much emphasis in the literature thus far. Beyond its significance as a trade agreement, however, the *Zollverein* era provides lessons on the impact of economic policy harmonization on the political cohesion between states that are at the core of today's policy debate, not only in Europe but also at a global level.

It is fairly typical to observe in the case of trade agreements early joiners and late joiners. For example, there has been a gradual, but marked, expansion in the members of the European Union since it was first established under the core members. Rarely is it the case that all members of the union are established in a single period. As Viner noted, "generalizations about the origin, nature, and consequences of unification of tariffs tend to be based mainly or wholly on the German [i.e., *Zollverein*] experience" (1950, 97). This holds for membership in multi-lateral free trade agreements such as the General Agreement of Tariffs and Trade (GATT) and now the World Trade Organization (WTO) as well. The differences in when and how specific countries decide to join are important aspects that have often been neglected. They might play a role in explaining the finding that trade agreements of the more recent

past has not raised trade (Rose 2004). Recent research has sought to identify the general characteristics of countries that choose to join trade agreements, finding larger trade agreement effects after taking into account income and other overarching characteristics of members into account (Baier and Bergstrand 2007, Egger et al. 2011). None of these characteristics appear to be specific to the particular trade agreement being looked at. In contrast to these studies, our analysis of the case of 19th century Germany captures the motives for joining a specific trade agreement–the Zollverein–at a level of detail that is not only unprecedented, but is also targeted to the specific historical context of the agreement. In this sense, our analysis breaks new ground relative to existing studies.

Our results also recast the debate on the impact of the *Zollverein* for economic growth in Germany. At first the contribution of the *Zollverein* to Germany's industrial take-off was widely accepted as a given (Henderson 1959). Post-war economic history called this into question, although the revisionist thinking was not always backed up by compelling empirics. In this paper we show that historically, market access was fundamentally important to regional incentives, and accounting for it is crucial for uncovering the major *Zollverein* contribution for 19th century German trade. This paper might be the first step towards resurrecting the role of the *Zollverein* for German industrial development more generally. The role of market access for economic performance has been central in recent work on trade and regional economics, following the work of Krugman and others (e.g., Fujita, Krugman, and Venables 1999). Market access can have an important impact on the locations of where manufacturing centers arise. Our finding of a substantial *Zollverein* effect, that is greater than we might have thought suggests that trade policy may have played an important role other economic developments within the German region in the 19th and 20th centuries, including where German manufacturing centers

arose.

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A Details on Data Sources

Wheat Prices The two most important sources for information on wheat prices are Shiue and Keller (2007) and Seuffert (1857). The former covers markets in Bavaria and Mecklenburg, while the latter provides information on markets in Baden, Brunswick, Hesse-Darmstadt, Hesse-Cassel, Hesse-Nassau, Saxony, and Wurttemberg. The wheat prices for Prussian markets were provided by Michael Kopsidis, see Kopsidis (2002). Additional sources to expand the coverage are Fremdling and Hohorst (1979), Gerhard and Kaufhold (1990) for Prussia, and Vierteljahrshefte (1935) for Berlin, Cologne, Hamburg, Leipzig, and Munich.

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

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

Zollverein membership A list with the dates of when states joined the Zollverein is given in Dumke (1976, pp. 98-99). The customs liberalization variable $CustLib$ is constructed using the historical maps at IEG (2013); $CustLib_{jkt}$ is equal to 1 if in year t there was at least one customs border between cities j and k, and 0 otherwise.

Railway cost Our measure is based on how the capacity of a 19th century steam locomotive to haul freight changes as a function of the gradient of the terrain, from Nicolls (1878). Based on that we construct a cost function, and use a 90 meter x 90 meter GIS map of the relevant

area in central Europe and the ArcGIS least-cost distance module to compute the least-cost routes, as well as the associated costs of those routes, from each city to all other cities in the sample. The railway variable in Table 2 is this railway cost divided by the bilateral geographic distance. All geographic distances in this paper use the Haversine formula. See Keller and Shiue (2013) for more details.

French occupation Information on the length of the French occupation during revolutionary and Napoleonic times comes from Acemoglu, Cantoni, Johnson, and Robinson (2011). See the overview in Keller and Shiue (2013), Table 1.

Other data This section provides information on the remaining variables that are employed in Table 2 and Table A.

Border-to-Area: log of average of border length to state area. Source: von Viebahn (1858), page 520, and Dumke (1976), page 97. Mean -0.60, standard deviation 0.76.

Population: log of the average population of cities j and k in the year 1800. Source: Bairoch, Batou, and Chevre (1988), De Vries (1984), and estimates of Keller and Shiue (2013). Mean 2.90, standard deviation 0.92.

Protestantism: average of the share of Protestants in the states where cities j and k are located, in the year 1858. Source: von Viebahn (1862), page 337. Mean 65.05, standard deviation 17.87.

Latitude: maximum of latitude of cities j and k, from www.maporama.com, accessed November 2008. Mean 52.43, standard deviation 1.37.

Longitude: maximum of longitude cities j and k, from www.maporama.com, accessed November 2008. Mean 11.55, standard deviation 1.16.

Shipping NS: This variable is equal to 1 if for both cities j and k all rivers through the states in which j and k are located empty either in the North Sea or Baltic Sea, and 0 otherwise. The third alternative is rivers flowing into the Danube and then the Black Sea. Source: computed from information in von Viebahn (1858), page 256. Mean 0.16, standard deviation 0.37

Coast: This variable is equal to one if both cities j and k have a distance to the nearest coast that puts them into the lowest quartile in the sample. Source: latitude and longitude information of the cities (see above) and the closest points on a coast relative to them, in terms of direct geographic distance (using the Haversine formula). Mean 0.04, standard deviation 0.20.

Table 1: Summary Statistics

Number	City Name	State	Data Range	Number of Observations	Wheat Price Gap (Mean abs perc diff)	Distance to Coast (miles)	Zollverein Accession (Year)
1	Aachen	Prussia	1820-1860	252	0.178	185.4	1828
2	Augsburg	Bavaria	1820-1855	153	0.161	382.7	1834
3	Karlsruhe	Baden	1820-1840	155	0.261	281.1	1836
4	Bamberg	Bavaria	1820-1855	153	0.154	277.3	1834
5	Bayreuth	Bavaria	1820-1855	153	0.154	272.8	1834
6	Berlin	Prussia	1820-1860	299	0.119	80.0	1828
7	Boizenburg	Mecklenburg	1820-1870	236	0.147	45.9	1867
8	Braunschweig	Brunswick	1820-1850	226	0.128	119.1	1841
9	Bremen	Free City	1840-1845	72	0.179	34.4	1888
10	Dresden	Saxony	1835-1850	129	0.134	168.1	1834
11	Erding	Bavaria	1820-1855	153	0.162	386.5	1834
12	Frankfurt	Free City	1840-1845	72	0.106	237.6	1836
13	Goettingen	Hanover	1820-1865	274	0.120	150.6	1854
14	Grabow	Mecklenburg	1820-1870	236	0.133	42.3	1867
15	Hamburg	Free City	1820-1880	299	0.141	0.0	1888
16	Hannover	Hanover	1820-1850	219	0.128	94.6	1854
17	Kassel	Hesse-Kassel	1825-1845	166	0.175	159.3	1831
18	Kempten	Bavaria	1820-1855	153	0.147	429.0	1834
19	Cologne	Prussia	1820-1880	281	0.120	167.9	1828
20	Landshut	Bavaria	1820-1855	153	0.185	371.3	1834
21	Leipzig	Saxony	1835-1880	202	0.130	170.6	1834
22	Lindau	Bavaria	1820-1855	153	0.148	446.0	1834
23	Luebeck	Free City	1840-1845	72	0.144	0.0	1867
24	Mainz	Hesse-Darmstadt	1840-1845	72	0.224	245.5	1828
25	Memmingen	Bavaria	1820-1855	153	0.151	411.9	1834
26	Munich	Bavaria	1820-1880	195	0.124	397.8	1834
27	Muenster	Prussia	1820-1860	252	0.146	98.7	1828
28	Noerdlingen	Bavaria	1820-1855	153	0.193	350.6	1834
29	Nurnberg	Bavaria	1820-1855	153	0.169	307.1	1834
30	Parchim	Mecklenburg	1820-1870	236	0.141	36.0	1867
31	Regensburg	Bavaria	1820-1855	153	0.218	338.1	1834
32	Rostock	Mecklenburg	1820-1870	236	0.150	0.0	1867
33	Schwerin	Mecklenburg	1820-1870	236	0.160	18.8	1867
34	Straubing	Bavaria	1820-1855	153	0.243	349.2	1834
35	Stuttgart	Wurttemberg	1850-1855	56	0.095	331.7	1834
36	Ulm	Wurttemberg	1850-1855	56	0.088	361.5	1834
37	Wismar	Mecklenburg	1820-1870	236	0.151	0.0	1867
38	Wuerzburg	Bavaria	1820-1855	153	0.134	290.5	1834
39	Zweibruecken	Bavaria	1820-1855	257	0.158	302.2	1834
40	Zwickau	Saxony	1835-1850	129	0.213	223.9	1834
Mean					0.155	214.2	1842.9
Standard Dev.					0.037	142.4	17.0
Sum				7140			

Table 2: Reduced-Form Results

	Baseline	Border-to-Area	French Occupation	Railway Cost	Population	Protest-antism	Latitude	Longitude	Remote-ness	Shipping NS	Coast
Rel. Distance to Coast	-0.104	-0.129	-0.120	-0.104	-0.097	-0.252	-0.124	-0.109	-0.0851	-0.228	-0.162
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.006]	[0.000]	[0.000]
(1820-35) x Border-to-Area		0.012									
		[0.188]									
(1840-60) x Border-to-Area		0.007									
		[0.372]									
(1820-35) x Fr. Occupation			-0.020								
			[0.000]								
(1840-60) x Fr. Occupation			-0.016								
			[0.004]								
(1820-35) x Railway Costs				-0.032							
				[0.006]							
(1840-60) x Railway Costs				0.005							
				[0.709]							
(1820-35) x Population					-0.006						
					[0.421]						
(1840-60) x Population					-0.006						
					[0.407]						
(1820-35) x Protestant						0.001					
						[0.001]					
(1840-60) x Protestant						6.00E-05					
						[0.866]					
(1820-35) x Latitude							0.011				
							[0.100]				
(1840-60) x Latitude							0.009				
							[0.135]				
(1820-35) x Longitude								-0.001			
								[0.925]			
(1840-60) x Longitude								-0.001			
								[0.298]			
(1820-35) x Remote									-0.014		
									[0.248]		
(1840-60) x Remote									-0.022		
									[0.048]		
(1820-35) x Shipping NS										0.073	
										[0.000]	
(1840-60) x Shipping NS										-0.004	
										[0.776]	
(1820-35) x Coast											0.044
											[0.031]
(1840-60) x Coast											-0.006
											[0.644]
Chi-sq test of inclusion		1.80	13.06	13.33	0.73	35.75	2.71	2.87	6.04	34.56	10.97
		[0.408]	[0.002]	[0.001]	[0.695]	[0.000]	[0.258]	[0.238]	[0.049]	[0.000]	[0.004]

Notes: Dependent variable is absolute value of percentage price gap between cities j and k; n = 3,570; p-values based on clustering at the city-pair level in parentheses
All specifications include year- and city-pair fixed effects

Table 3: The Impact of the Zollverein Liberalizations

	(1) Robust	(2) City-pair Clustering	(3) City Clustering	(4) Size weights	(5) No Prussia
TSLS Second Stage					
Customs Liberalization	-0.055	-0.055	-0.055	-0.063	-0.050
	[0.005]	[0.002]	[0.020]	[0.001]	[0.008]
First Stage					
Rel. Distance to Coast	1.889	1.889	1.889	1.667	1.759
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
F-statistic	92.63	54.84	19.49	53.09	51.45
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Kleibergen-Paap F-stat	92.63	54.84	53.51	53.09	51.45
OLS					
Customs Liberalization	-0.014	-0.014	-0.014	-0.012	-0.007
	[0.069]	[0.030]	[0.212]	[0.072]	[0.319]
Endogeneity test	4.381	4.761	3.463	5.934	4.190
	[0.036]	[0.029]	[0.063]	[0.015]	[0.041]
City-pair Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of obs	3,570	3,570	3,570	3,570	3,245
City-pairs	642	642	642	642	596
No. of clusters		642	39	642	596

Dep. Variable: absolute value of percentage price gap between cities; p-values in parentheses

No Prussia: Observation dropped if city j is from Prussia; city-pair clustering in (4) and (5)

Size weights: weights are log average population of cities j and k in year 1800.

Table 4: Robustness

	(1) Liberalization 1855-75	(2) No State Capitals	(3) Cluster State-Pair	(4) Cluster State-Pair x Year	(5) No Bavaria	(6) No City States	(7) No Influential Observations	(8) Sample Composition
TSLS Second Stage								
Customs Liberalization	-0.166	-0.041	-0.055	-0.055	-0.075	-0.050	-0.048	-0.062
	[0.020]	[0.017]	[0.051]	[0.095]	[0.002]	[0.009]	[0.003]	[0.001]
First Stage								
Rel. Distance to Coast	0.625	2.220	1.889	1.889	1.514	1.853	1.911	1.744
	[0.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
F-statistic	8.87	46.37	16.90	31.16	53.51	45.91	54.21	52.52
	[0.003]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Kleibergen-Paap F-stat	8.87	46.37	44.22	31.16	53.51	45.91	54.21	52.52
OLS								
Customs Liberalization	-0.035	-0.016	-0.014	-0.014	-0.018	-0.014	-0.014	-0.018
	[<.001]	[0.016]	[0.347]	[0.455]	[0.093]	[0.038]	[0.015]	[0.011]
City-pair Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs	3,570	3,280	3,570	3,570	2,164	3,388	3,491	2,551
City-pairs	642	575	642	642	416	590	642	307
No. of clusters	642	575	123	582	416	590	642	307

Dep. Variable: absolute value of percentage price gap between cities; p-values in parentheses
No Bavaria (City States): Observation dropped if city j from Bavaria (from a city state);
No State Capitals: Observation dropped if both city j and city k are state capitals; No Influential Points
based on Cook's Distance; Liberalization 1855-75: Customs Liberalization variable equal to 1 for all city-pairs
for all observations 1855 to 1875; (1), (2), (5), (6), and (7) report p-values in parentheses based on clustering at city-pair level
Sample composition: Only city-pairs with observations for more than 50% of the sample included.

Figure 1: The area around Frankfurt in the year 1789

Source: Thomas Hoeckmann, www.hoeckmann.de

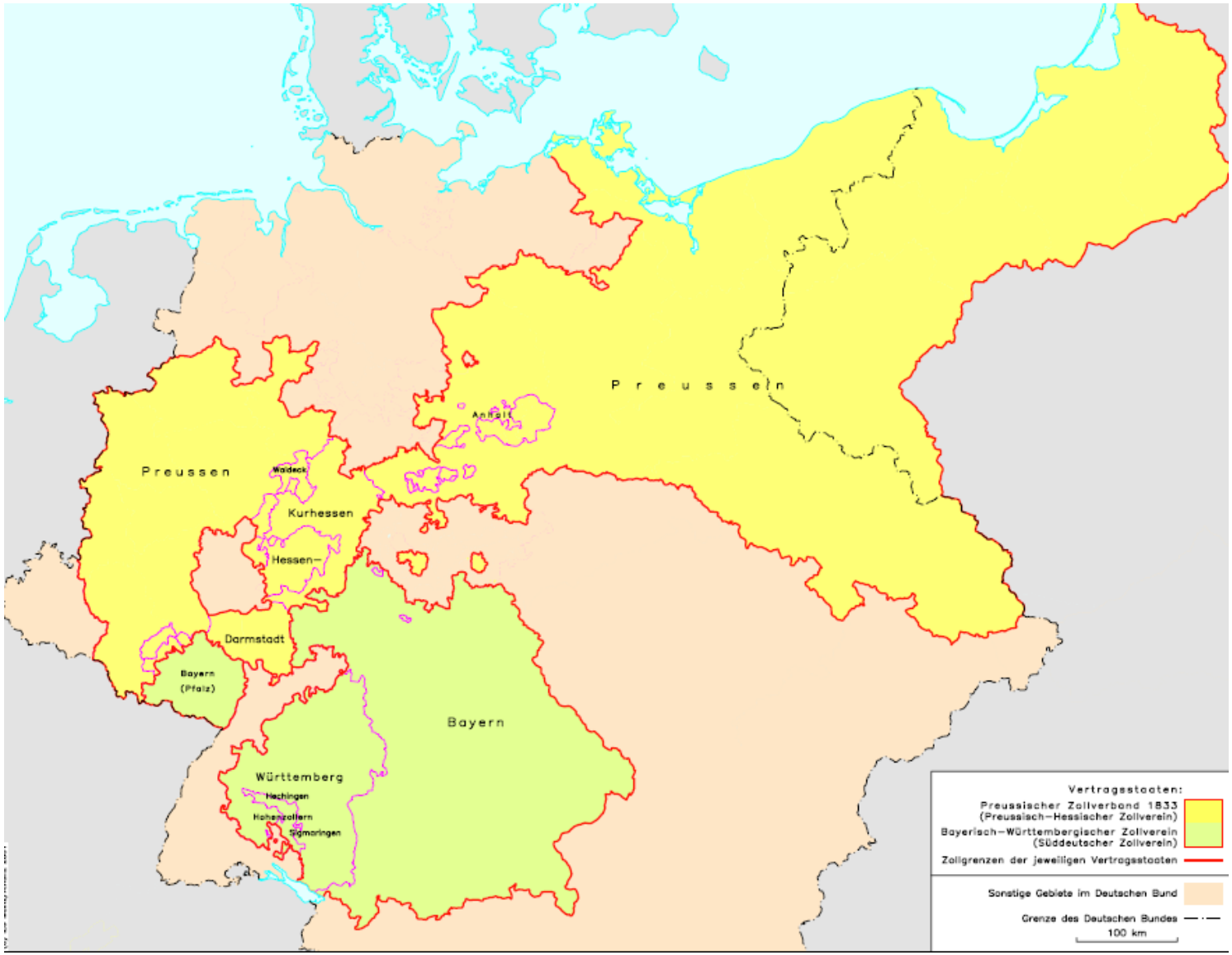
Figure 2: Political Borders in Central Europe in the Year 1820

Source: IEG (2013)

Figure 3: The Zollverein in the Popular Press

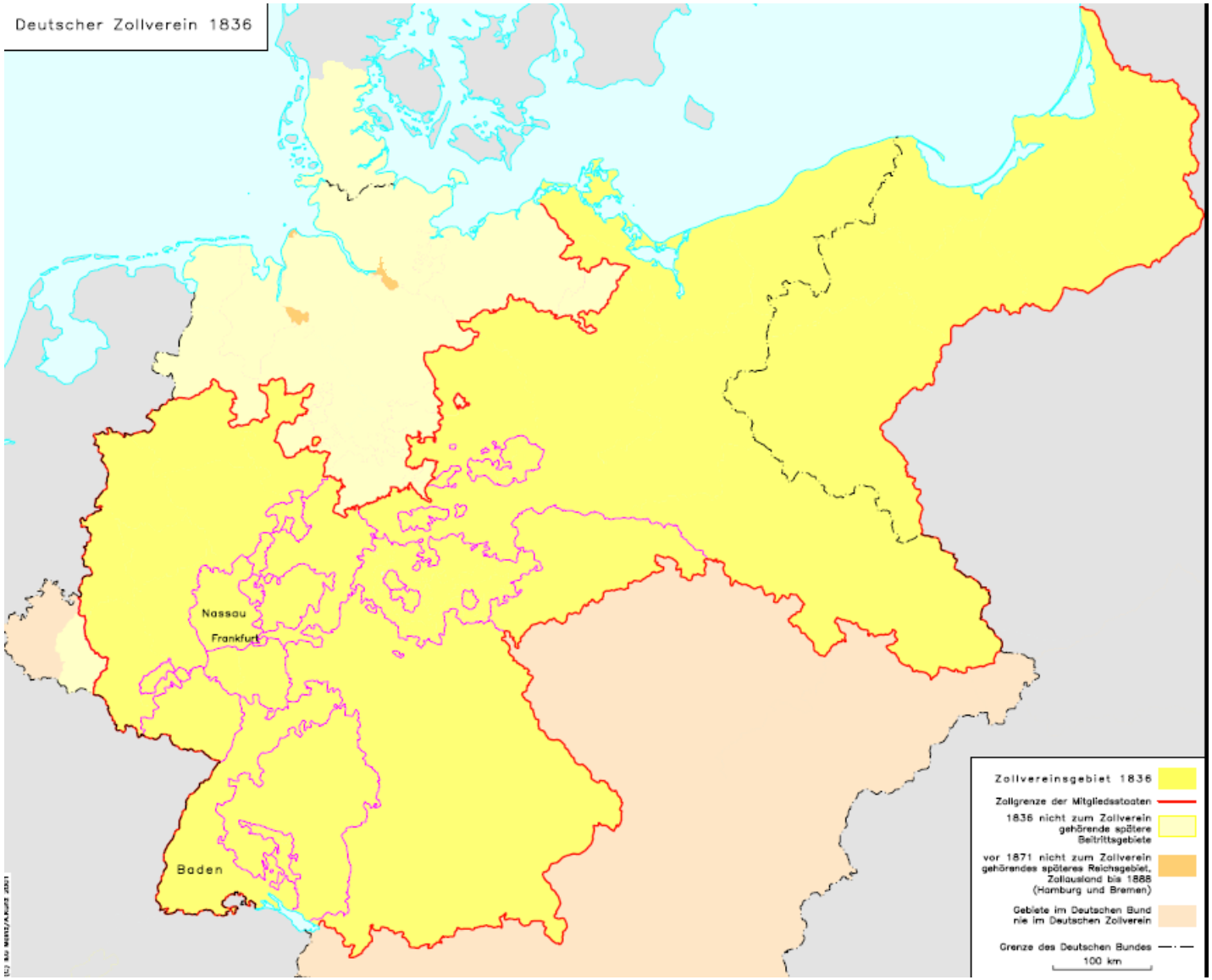
Source: *Fliegende Blätter* , year 1848, volume 6, number 140, page 157; published in Munich

Figure 4: The Prussian-led Customs Union and the South German Customs Union in 1833



Source: IEG (2013)

Figure 5: The Zollverein in the year 1836



Source: IEG (2013)

Figure 6: Price convergence and the 1834 customs liberalizations

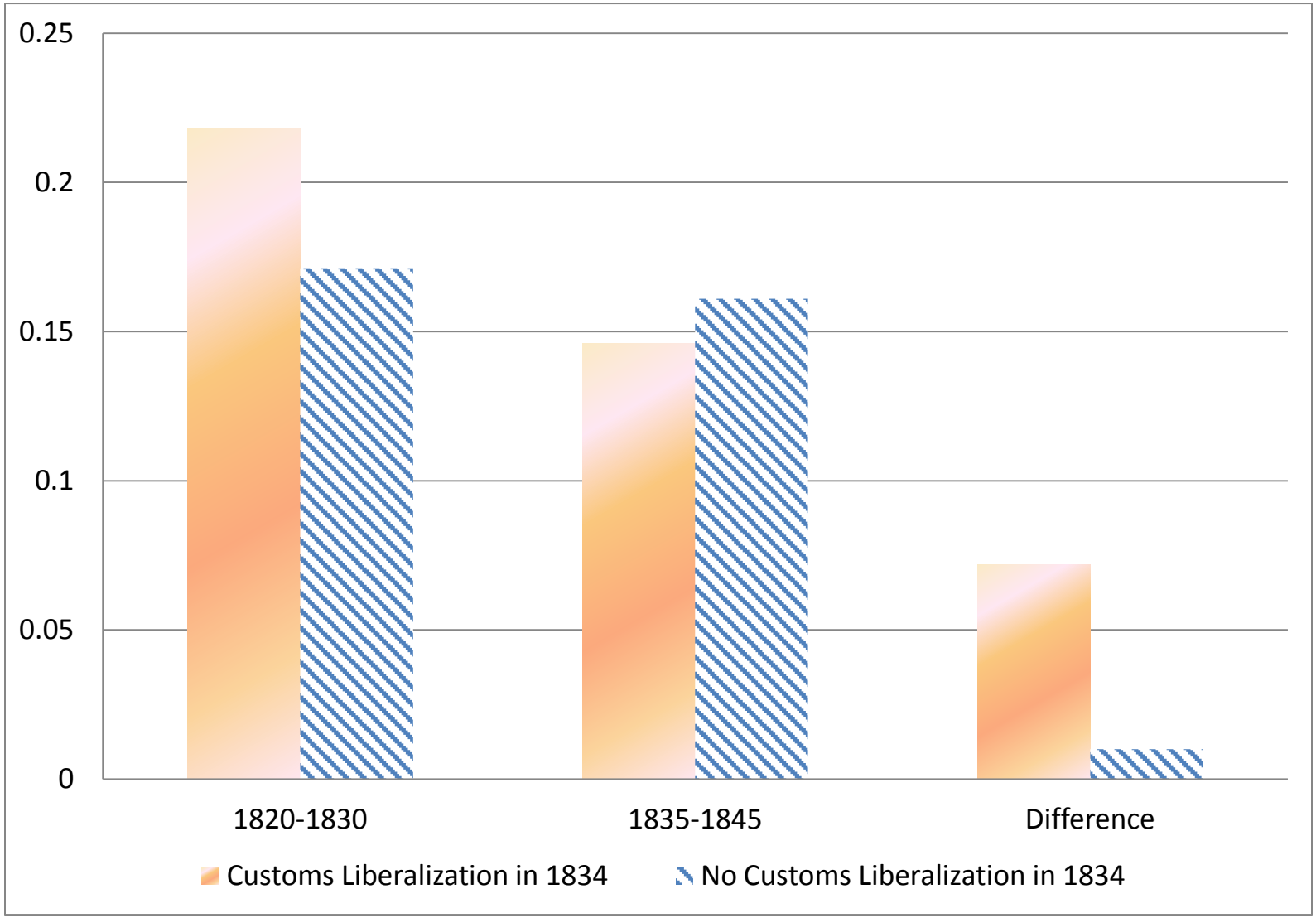


Table A: The Influence of Additional Factors

	Baseline	Border-to-Area	Railway Costs	Population	Protestant	Latitude	Longitude	Remoteness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Second Stage								
Customs Liberalization	-0.055	-0.093	-0.042	-0.061	-0.068	-0.065	-0.063	-0.042
	[0.005]	[0.001]	[0.079]	[0.001]	[0.049]	[0.024]	[0.003]	[0.005]
First Stage								
Rel. Distance to Coast	1.889	1.307	1.871	1.796	1.072	1.215	1.526	2.191
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
F-statistic	54.84	41.16	25.08	55.21	32.09	40.83	52.06	53.97
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	3,570	2,679	2,679	2,709	2,751	2,742	2,765	2,680
Number of city-pairs	642	533	607	516	489	450	467	480

Notes: Dependent variable is absolute value of percentage price gap between cities j and k.
All specifications include time- and city-pair fixed effects; p-values based on clustering at the city-pair level in parentheses