

From Political Fragmentation towards a Customs Union:

Border Effects of the German Zollverein, 1815 to 1855¹

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Abstract

Over the first half of the 19th century, the Prussian-German Customs Union known as the Zollverein gradually unified a scattered confederation of sovereign states under an internal free trade agreement. This paper uses grain prices to quantify the differential effect of the Zollverein for market integration among Zollverein members versus European powers that were not part of the Zollverein, including France, Switzerland, and the Habsburg Empire of Austria. Overall, this border effect is consistently and substantially less than border effect estimates from contemporary samples. For the 1834 liberalization round, the implied border effect, calculated as the implied decrease in distance that comes about as the result of the customs border being eliminated, is between 140 and 160 kilometers, with the smaller distance for non-German speaking cities, and the larger distance for German speaking cities. Thus, common language in this sample provides an additional benefit of lowering trade barriers by 11-15% in distance, making border elimination more valuable among German-speaking cities than for mixed-language-speaking cities. The paper offers a few reasons for why I estimate smaller border effects than are found in studies on 20th century economies, and the analysis gives a new historical perspective on what drives trade costs and changes in market integration.

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1. Introduction

Over the first half of the 19th century, the Prussian-German Customs Union known as the German Zollverein gradually unified a scattered confederation of sovereign states under an internal free trade agreement. According to Jacob Viner, “the German Zollverein was the pioneer and by far the most important customs union, and generalizations about the origin, nature, and consequences of unification of tariffs tend to be based mainly or wholly on the German experience.”³ The Zollverein provides a convenient natural experiment that helps shed light on the impact of borders in the 19th century, since the policy effectively dismantled economic borders among member states. By stages, the political borders between member states dissolved as well. Moreover, a comparison of historical and contemporary so-called ‘border effects’ can provide a better understanding of the impact of political boundaries and trade costs over time on economic integration.

The measure of the border effect has been used in numerous contemporary studies to capture the notion that political borders lead to international price dispersion. Typically, the price dispersion of a similarly traded good is larger across different countries relative to an equal distance within borders, and the measure of how much more can be attributed to the additional transactions costs of crossing the border (Feenstra 2004, 151). The factors behind the widely documented border effect are numerous, and could include tariffs or non-tariff barriers, exchange rate variability, nominal price stickiness, unit-shipping costs, and differences in culture and language, among other sources.⁴

Significantly, existing estimates suggests that international border effects are large (McCallum 1995, Engel and Rogers 1996, Parsley and Wei 1996, 2001, Helliwell and Verdier

³ Viner (1950, 97).

⁴ Evans (2003), e.g., evaluates some of the sources of the border effect.

2001). Engel and Rogers (1996), for example, show in a late 20th century sample that crossing the United States and Canadian border has the equivalent effect on price volatility as adding 1,780 miles (or 2,848 kilometers) between cities within national borders, a result that implies transporting a good between cities located directly across the U.S.-Canadian border is as difficult as carrying it between Boston and Houston. What is striking about the estimate is that the effects are so substantial even between countries that share a common language, similar legal systems, market institutions, and where formal trade barriers have been lowered under the North America Free Trade Agreement (NAFTA). Between countries such as the U.S. and Japan, Parsley and Wei (2001) calculate that the “border” is equivalent to 43,000 trillion miles. What estimates on border effects might we expect to find in a historical sample where trade barriers tended to be much higher and international markets more segmented? If studies based on today’s economies are any guide, one might expect that in times past border effects placed only greater constraints on trade opportunities.

In order to assess the impact of borders in 19th century Europe, and to provide a comparison of that to the importance of borders today, this paper examines how national borders affected the process of European integration during the critical years of its economic development from 1815 to 1855. I use grain prices to quantify the differential effect of the Zollverein for market integration among Zollverein members versus European powers that were not part of the Zollverein, including France, Switzerland, and the Habsburg Empire of Austria. If price dispersion was driven by the presence of trade barriers, customs houses, and official borders before the Zollverein agreement, integration should increase when these factors are removed. In addition to estimating the border effect, I also use local indicators of spatial association (Anselin 1995) to assess relative market integration. Local Geary statistics allow us

to take a spatial view of integration and to identify the specific locations where integration was relatively strong or relatively weak.

A comparison of 19th century and contemporary border effects should provide a better understanding of their effects in history and on how borders may have affected long-run economic development paths. The study of the impact of border effects in history may also be helpful for arriving at a better understanding of trade costs today (Anderson and van Wincoop 2004). This paper relates broadly to several other strands of the literature. One is the longstanding empirical research on the Law of One Price (LOP) or Purchasing Power Parity (PPP), the basis of which has been examined under a diverse set of contexts and for a variety of goods. Recent studies include Persson (1999), Kopsidis (2002, 1998), Shiue (2003, 2002), Taylor (2002), Findlay and O'Rourke (2003), and Goldberg and Verboven (2003). None of these studies, however, have measured the border effect in a historical setting. Second, since improved economic integration in the 19th century might have had an impact on the scope of specialization and Germany's path towards industrialization, the findings also relate to the important topic of the implications of trade for growth. See for example, Bairoch (1989, 1972) on the history of European development and free trade, O'Rourke and Williamson (1999) on the impact of trade in goods and factors in the Atlantic Economy, and Frankel and Romer (1999), Sachs and Warner (1995), and Rodriguez and Rodrik (1999) on cross-country correlations between openness and growth in 20th century.

The following section gives a brief overview of the political and economic conditions, starting before 1815. Section 3 discusses the data used in this paper. Section 4 presents the framework of analysis and empirical results, and Section 5 concludes.

2. Historical Overview⁵

In the 18th century, Germany was divided into over three hundred states—Kingdoms, Electorates, Duchies, Imperial Cities, ecclesiastical territories, and other political administrative bodies—wherein the two major powers were the Habsburg Empire (Austria) and Prussia.⁶ After the defeat of Napoleon in 1814/15, the French withdrew their expansion towards the East, and Germany's political structure became reorganized into the thirty-nine states of the German Confederation (*Deutscher Bund*) (see Figure 1). Austria was the most powerful of the German states in 1815, followed by Prussia, whose territories were separated into two parts. The Eastern part reached from the mouth of the Vistula River to the Harz mountains, and consisted of seven provinces. The Western part consisted of the Westphalia and the Rhineland provinces, and included the Rhine-Ruhr area that later was to become the industrial center of Germany.

The German states of moderate size included Hanover, Saxony, Bavaria, Württemberg, and Baden. There were also a number of independent (free) cities, such as Frankfurt. Constitutionally, the German Confederation was a union of sovereign states in which joint action depended upon unanimity, and states sought to retain their independent status. Thus, even though the German Confederation had a parliament based in Frankfurt, its members would vote according to instructions from their respective governments. As of 1815, Germany still lacked the political unity that characterized the contemporary nation states of, for instance, France or Britain.

At the end of the 18th century, German textile industries in Silesia, Saxony, and Münster,

⁵ See e.g. Hahn (1984), Henderson (1939, 1975), Tilly (1966), Kitchen (1978), Lee (1988), Dumke (1991, 1977), and Bazillion (1990) for additional background information.

⁶ Here, Germany refers to the outlines of the German Confederation of 1820; see Figure 1. Figures 1-4 come from the server for digital historical maps at the Institut für Europäische Geschichte-Mainz (IEG), <http://www.ieg-maps.uni-mainz.de/>.

were among the few industries that were flourishing, and this was due in part to the Continental Blockade system implemented under Napoleon that shut out British competition. Many German industries were still comparatively underdeveloped, and specialization was limited.⁷ Much of the raw materials consumed in Germany were also of domestic origin, and German states generally aimed for self-sufficiency. By contrast, industrialization had already begun in Britain, and Britain was also engaged to a greater extent in foreign trade.

It is plausible that internal German trade before the creation of the Zollverein in 1834 had been hampered by political fragmentation and by the fact that individual states tended to be quite protectionist. A shipper on the Main and Rhine rivers from Bamberg to Mainz (187 kilometers as the crow flies), or from Strassbourg to the Dutch frontier, had to pay more than thirty tolls. Fourteen tolls were charged between Magdeburg and Hamburg on the Elbe. Furthermore, tariffs were complicated: Prussia, for example, imposed over 60 different rates of customs and excise (Henderson 1939, 22-23). These tolls likely reduced internal German trade while encouraging smuggling: the size of the customs area was small relative to its customs border, making the enforcement of customs payment relatively costly on a per-capita (or, per square-mile) basis. Moreover, the German states as a whole were not protected by a high common external tariff as was the case for other European countries.⁸ Given the complexity of the tariff structure, and the frequency with which it changed, it is not surprising that there is no study that is based on a comprehensive measure of transportation costs.⁹

⁷ Henderson (1939, 10), e.g. suggests that “the lack of effective political unity in Germany and the strength of the spirit of particularism in 1815 help to explain the economic backwardness of the country in comparison with Britain or France”. See also Clapham (1936).

⁸ In particular, the tariffs of some of the free cities and ports (Hamburg, Bremen, and Frankfurt) as well as those of states that depended highly on trade (e.g. Baden).

⁹ An interesting contribution along these lines is Ohnishi (1973), who examines the tariff policy of Prussia between 1818 and 1834. At the same time, this study provides limited quantitative details, and moreover, Prussia is only one of several European states that I cover in the present study.

2.3 The Creation of the German Zollverein

Arguably, it was the onset of industrialization and economic growth that made it more costly to retain economic independence, leading to the creation of the German Zollverein. In the case of Germany, economic integration in fact preceded political integration. During the 19th century, Austria and Prussia vied to become the dominant German power. Prussia prevailed in this contest, and the *German Reich*, a politically unified entity, was formed under Prussia's leadership in the year 1871.

Although the year usually given for the inception of the German Zollverein is 1834, in fact the elimination of internal customs barriers in Germany was a gradual process. Moreover, the Zollverein was not so much an agreement created among roughly equally powerful states, but it was rather the product of Prussian efforts to cast an increasingly wider customs union and to include more and more of the German states (ultimately, the only important exception was Austria).¹⁰

When the Prussian Customs Union was formed in the year 1818, Prussia did away with a multitude of national, provincial, local, and private dues in favor of a reasonably simple, unified, and relatively low tariff at its external frontiers. Other German states, notably Bavaria, Württemberg, and Baden, negotiated the formation of a customs union on their part, but initially without success. Then, in 1828, Hesse-Darmstadt decided to join the Prussian Customs Union. It adopted the Prussian external tariff and eliminated internal barriers to Prussia in exchange for a share of the customs revenue. In the same year, Bavaria and Württemberg formed the South German Customs Union, while a number of central German states and cities formed the Middle

¹⁰ On the German and Austrian relationship, see Katzenstein (1976).

German Commercial Union (see Figure 2).¹¹ The latter was not a customs union, but essentially a defensive agreement among members to commit to not joining either the Prussian/Hesse-Darmstadt or the South German Customs Union. Countries such as Britain and the Netherlands were eager to have low-tariff access to Frankfurt and Germany's South, and they supported the Middle German Commercial Union.

Prussia, however, had become by that time far too important an economy to ignore and the other German states found they could not boycott the customs union for long. As the Middle German Commercial Union began to crumble, Hesse-Cassel became the first to join the Prussian Customs Union in 1831.¹² In the year 1833, both the Thuringian states and the Kingdom of Saxony formed respective customs unions. All of these areas, together with the augmented Prussian Customs Union, became the German Zollverein on January 1st, 1834. Further, the terms under which states joined the union were substantively the same.¹³ Figure 3 shows the area of German Zollverein in 1834, with an area of about 163,000 square miles and a population of about 23.5 million people.

Three other German states joined the German Zollverein between mid-1835 and early 1836: Baden, Nassau, and the city of Frankfurt (see Figure 4). This connected Bavarian Palatinate, the part of Bavaria to the west of the Rhine, to Württemberg and the Bavarian core areas without customs border. The accession of Frankfurt was significant, first of all, because of

¹¹ The states were Hanover, Saxony, Hesse-Cassel, Nassau, Brunswick, Oldenburg, Frankfurt, Bremen, the Saxon duchies, and a couple of smaller ones. See Henderson (1939, 67).

¹² This 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 not reach Frankfurt and Germany's south anymore without crossing the Prussian external tariff border; see Figure 1.

¹³ Saxony entered on the same terms as Hesse-Cassel, Hesse-Darmstadt, Bavaria, and Württemberg. In some instances, however, the terms of negotiated by different states were not precisely the same, even if the general outlines of membership were the similar. For instance, Hesse-Cassel obtained privileges for the Cassel fair, whereas Hesse-Darmstadt did not receive similar rights for its own fair. Further, states sometimes tried to participate in discussions together with Prussia concerning the entry of new member states. For details on this on other agreements, see Henderson (1939, 81-82, 86-87).

the relative importance of Frankfurt as a larger city.¹⁴ It also allowed trade in manufacturing goods from Frankfurt up the Main River to Northern Bavaria in exchange for grain without paying customs duties. Figure 4 shows the German Zollverein in the year 1836.¹⁵

The Zollverein has been credited for promoting Germany's industrialization by achieving a high degree of integration among the Zollverein states, but the effect of the Zollverein on long-run growth has been much debated. Henderson (1939, 339) cautions against seeing a causal connection between the Zollverein and improved economic conditions in Germany in the twenty years after the Zollverein was established, yet he also acknowledges the positive contribution in the customs union to subsequent industrial success in Germany: "Between 1815 and 1850, the first steps towards industrial expansion were taken by the founding and extending of the Zollverein and by the improvement of communications."¹⁶ The Zollverein may have had important indirect effects as well, in particular in two areas. First, Zollverein membership may have reduced the time until a particular city (or area) became part of the railway network that was built in Germany after 1835. Second, the Zollverein may also have contributed to monetary integration among member states, as exchange rates were fixed among member states by the year 1838, soon after the creation of the Zollverein (see Bissing 1959).

Under the centralized management of Prussia, the Zollverein may have been more efficient at organizing tariff regulation and collection, at least compared to many small states

¹⁴ Prussia, for example, traded about twice as much with Frankfurt as it did with England or Austria, and about four times as much as with France in the early 1820s (Ohnishi 1973, 143).

¹⁵ Other notable accessions to the Zollverein before the political unification of Germany, in the year 1871, include Braunschweig (1841), Hanover (1851), Oldenburg (1852), Schleswig-Holstein (1866), and Mecklenburg-Schwerin, Mecklenburg-Strelitz, as well as Lübeck in 1868.

¹⁶ See also Pollard (1981, 159), who describes how the Zollverein was important for German development and subsequent industrialization because it helped achieve a large unified Empire, economically and politically. Bazillion (1990, 192) suggests that modernization quickened its pace in Saxony due to access to newly-opened markets after the Zollverein came into existence.

each monitoring its own borders. Lee (1988) and Dumke (1991, 1977) tend to view the revenue sharing function as the primary economic legacy of the Zollverein, concluding that economic union played a limited role in promoting regional long-run development.¹⁷ Still unresolved, however, is whether the external tariffs changed the level of protection that German industries had during pre-Zollverein times. Notably, Prussia's revenues also decreased between 1834 and 1838 when the Zollverein was first introduced (Henderson 1939, 140).¹⁸ Similarities can be drawn between the arguments in this debate in the context of the Zollverein and those in the literature on the effect of tariffs on economic growth in other countries of the past and present.¹⁹

For the specific case of agricultural products, the key restrictions on trade before the customs union often took the form of export tariffs, including partial border closures.²⁰ When bad harvests led to rising local prices, the government could take measures to discourage grain exports.²¹ These measures were at the government's discretion, typically staggered, and depended upon the degree of price increase. Seiffert (1893) reports that in one instance in late 18th century Bavaria, the government increased the export duties and introduced a new export permit.²² As prices rose further, the government completely prohibited grain exports, and also

¹⁷ Lee (1988, p. 351) states, "The customs union did not fundamentally affect the existing dynamic of early German industrialization; not only were inner German trade links well established before 1834, despite the persistence of internal trade barriers, but the beginning of the long-term growth of German trade can be assigned with some certainty to the mid-1820s, a full decade before the establishment of the Zollverein."

¹⁸ In 1834, Prussia's receipts were 22.5% less than what it was in 1833 (20 silver groschens per head in 1834 and 15.5 silver groschens per head in 1833), and reached former levels only in 1838 (Henderson 1939, 141-2). In other German states, Zollverein membership appears to have increased tariff revenues considerably; see, e.g., Hahn (1982) who shows that tariff revenue rose in Hesse-Darmstadt, Hesse-Cassel, and Nassau when these states joined the Zollverein, both in absolute terms as well as a share of total revenues (Hahn 1982, Tables 2-5; pp. 317-327).

¹⁹ See for example, Harley (1992) on the contribution of tariffs for the evolution of the textile industry in 18th century America, O'Rourke (2000), and Irwin (2002, 2000) on the impact of tariffs in the late 19th century.

²⁰ In the case of Bavaria, grain export in the early 19th century also entailed additional fees (Seiffert 1893, 886). See also Gailus (1994, 188).

²¹ Berger and Spoerer (2001) have recently argued that the degree of price shocks in the bad harvest years of 1845/47 can in part explain in which countries the revolutionary movements of 1848 were particularly pronounced.

²² Increases in export duties varied: an additional 48 Kreuzer by ship, 32 Kreuzer over land.

eliminated the usually required consumption tax on imported grain. During extreme food crises, the smuggling of grain out of the country was punishable by death (Seiffert 1893, 887).

The governments' interventions after the Zollverein was created may still have curtailed the extent of intra-German trade, but Prussia in the 1840s was also a relatively anti-interventionist government compared to its stance before the 1820s. Some of the measures were merely gestures intended to quell food riots.²³ For instance, the prohibition against the export of potatoes outside the Zollverein had little significance since potatoes were not generally suitable for export anyway (Gailus 1994, 188). In addition, as noted in Seuffert (1857, Introduction), these measures were often ineffective in preventing the prices from rising, in part because in response merchants would withhold grain from the markets, leading to a rise in grain prices.

It is reasonable to expect that the impact of the customs union was to increase integration in the German regions. The price studies of Kopsidis (2002, 1998) and Bass (1991), which use simple correlation as well as co-integration techniques to analyze the integration of agricultural markets in German regions in the 19th century, provide evidence that this was the case. Kopsidis (1998), for instance, finds that Westphalian rye markets became increasingly integrated between 1780 and 1880. Moreover, a major part of the overall increase in integration in Westphalia appears to have occurred only after 1850, and may be traced to the extension of the railway network after 1844. If this is the case, then railway construction was likely to have had the same influence on market integration in other German regions over the primary period of railway construction, 1835 to 1885. Other authors, however, place less emphasis on railways. Fremdling and Hohorst (1979), for example, find that a substantial part of the advancement in market integration in the German rye market occurred already by 1820, leaving a less prominent role for changes in the degree of market integration and the railways that came later in the 19th

²³ Gailus, (1990, 328, 335-6, 341).

century.

I now turn to the data that will be used in the analysis.

3. Data sources and characteristics

This paper uses price data to study the development of markets. As barriers to trade fall, arbitrage between markets will bring price differences down, until in the limit the law of one price (LOP) holds. In this framework, relatively small deviations from LOP are consistent with a relatively high level of market integration, and vice versa. At least two forms of trade barriers can be distinguished: first, transport costs as they relate to the value-to-weight ratio of goods as they are moved over some geographic distance. Second, tariffs, taxes, and fees of various kinds that relate to border crossings from one to state to another state. While my analysis encompasses both types of barriers, the focus is on how the elimination of customs borders affected trade as evidenced by changes in the price gaps between markets. I will also touch on trade barriers more broadly defined. Language differences, for instance, are frequently correlated with geographic distance.

The main source of information on prices of German states used in this study is Seuffert's (1857) analysis of monthly market prices for four types of grains—wheat, rye, barley, and oats—in Bavarian cities for the years 1815 to 1855. Specifically, I use data on wheat prices in Bavaria's core region east of the Rhine as well as for Zweibrücken, located non-contiguously in the Bavarian Palatinate area (see Figure 1).²⁴ The original source for this data are 'Schrannenzettel' ('Schranne' is the Bavarian word for markets), which are records of the proceedings on a giving market day by public officials. These data are therefore similar to the

²⁴ The fourteen cities in Bavaria's core area are: Augsburg, Bamberg, Bayreuth, Erding, Kempten, Landshut, Lindau, Memmingen, Munich, Nördlingen, Nürnberg, Regensburg, Straubing, and Würzburg; their locations are shown in Figure 5.

so-called *mercuriales* in the French-speaking part of Europe.

The Seuffert source lists the average price ('Mittelpreis') of all market days in a month (typically the Saturdays), as well as the quantities offered and sold. The quantity units are in Bavarian Schöffel (1 Schöffel wheat is about 223 liters in volume), and the monetary units are in Bavarian Gulden and Kreuzer (1 Gulden = 60 Kreuzer). The source does not give specific information on the quality of the grain that was offered at a particular market in a particular month, but it is fair to assume that the average price would be paid for mid-quality wheat.²⁵ The price series are highly complete and seem to be very reliable,²⁶ a fact that has been noted by other researchers.²⁷

Seuffert also provides annual prices for markets in Baden, Württemberg, Frankfurt, Prussia, and Switzerland. In the early 19th century, these states would typically use both different currencies and different quantity units. Such differences will often make absolute price comparisons difficult, but here I can use all relevant conversion rates (reported in Seuffert 1857, 351). This data is complemented with market prices for grain for certain markets in Austria and in France (from Pribram 1938 and Drame et al. 1991, respectively).²⁸

Summary statistics for the prices are given in Table 1. The 26 cities in the sample provide a total of 944 annual observations. Average bilateral distance between two cities is

²⁵ It is likely that when grain is relatively scarce, the average quality of grain on the market tended to decline, and vice versa. This would mean that the price range holding constant quality is somewhat larger than what is listed in the source. Any such effect such be of second- (or lower) order, however, and in any case, there is no evidence that this -if present-would affect the prices in different cities of my sample differentially.

²⁶ In other work, I have compared the figures for Munich in Seuffert (1857) with those for Munich in Elsas (1936), and once converted into the same monetary units the series are virtually identical; see Shiue and Keller (2004), Data Appendix.

²⁷ See, e.g., Drame et al. (1991), who find the Seuffert (1857) data to be 'tres completes et tres precises' (p.117).

²⁸ The prices for markets in Austria and France are available monthly, whereas the prices for markets in Baden, Württemberg, Frankfurt, Prussia, and Switzerland from Seuffert (1857) are available at an annual frequency.

about 337 kilometers. Markets locations with city names are shown in Figure 5.²⁹

Geographically, wheat prices in Europe tend to increase as one moves further to the North and to the West.

4. Empirical analysis

4.1 Spatial correlation in the Bavarian market for wheat

As noted earlier, I examine the evolution of market integration in Germany by explicitly incorporating spatial features. First and foremost, these are the geographic distances between the grain markets. In order to get an idea on which markets in Bavaria were relatively well connected and which were not, it is useful to consider measures of spatial correlation across markets. Geary's (1954) statistic of global spatial correlation has recently been extended by Anselin (1995) to encapsulate a local measure of autocorrelation. Omitting the time subscript for readability, for city i and a given spatial lag k , the local Geary measure is defined as

$$c_i^{(k)} = \frac{(N-1) \sum_{j \neq i} \delta_{ij}^{(k)} (p_i - p_j)^2}{N \sum_q (p_q - \bar{p})^2}, \quad i, j, q = 1, \dots, N \quad (1)$$

where for any city i , p_i is the log of its price for wheat and N is the size of the sample ($N=14$ in core Bavaria). The $\delta_{ij}^{(k)}$ is an indicator variable that equals one if the distance between cities i and j falls in the distance class k , and zero otherwise.³⁰ The statistic is seen to be a quadratic in

²⁹ The figure, as well as my analysis, associates each state with a city. In five cases I have only information on the state-wide average price (for Baden, Württemberg, and the Prussian provinces of Saxony, Westphalia, and Rhine province). If there is a significant amount of within-state price dispersion (e.g. prices in the hilly areas of Württemberg's Black Forest are higher than in the capital, Stuttgart), this may lead to higher measurement error. I have experimented with different samples and found that this has no major effect on my results. Note that Figure 5 does not show Bar-le-Duc and Chalons-sur-Marne, two French cities to the west of Luneville.

³⁰ For instance, I will consider below the distance class $(0, 200)$, so that $\delta_{ij}^{(k)}$ is equal to 1 if the distance between i and j is between 0 and 200 km, and 0 otherwise.

the cities' price differences in distance class k relative to the overall variance. All else equal, the smaller the price gaps for a given distance class, the lower the Geary statistic and the higher is the degree of spatial correlation for these distances.

Figure 6 gives these measures, averaged over all months (January to December) and years (1815-55) for distances between 0 and 200 km for the 14 Bavarian cities shown in the figure. Relatively high spatial correlation—indicated by relatively low Geary values—is primarily obtained for cities close to the center of Bavaria (Nürnberg, Augsburg, and Landshut), while lower spatial correlation is computed for cities on the sample's periphery (Nördlingen: West, Straubing: East, and Bayreuth: Northeast). This suggests that high spatial correlation is an indicator of market trade and integration opportunities.

Other factors determine the extent of spatial correlation as well. Würzburg, for instance, exhibits a relatively low Geary value even though it is located in the Northwest corner of Bavaria. This can be in part explained by its location on the Main River (see Figure 7, which shows the rivers), which lowers the unit transport costs. Würzburg is the Bavarian 'exit portal' to the large downstream city of Frankfurt, and it is also connected by water transport route to other Bavarian markets. Indeed, spatial correlation on average is higher for river cities than for non-river cities.³¹ This parallels the results for 18th century China obtained in Keller and Shiu (2004, forthcoming).

The interpretation of a high spatial correlation as evidence for relatively good market integration depends on which geographic distance is considered. Take for example Lindau, located in the Southwest of Bavaria. For distances of up to 100 km, Lindau has the highest spatial correlation of all 14 cities, while for all distances it has the lowest degree of spatial correlation. Lindau receives much of its grain from two nearby cities (Kempten and

³¹ The Geary statistics are 0.71 and 0.79 for river and non-river cities, respectively.

Memmingen), and exports a substantial amount of it to neighboring Switzerland, where prices tend to be higher than in Bavaria. This explains Lindau's high spatial correlation for short distances. At the same time, Lindau is relatively far away from other Bavarian cities (and with prices being relatively high, Lindau rarely exports grain to other Bavarian cities), which explains its low overall market integration as indicated by the high Geary statistic for all distances.

4.2 Changes in spatial correlation over time

Spatial correlation increased in Bavaria over these roughly forty years, with the local Geary statistic averaging 0.85 during the years 1815-33, but only 0.61 for the years 1837-55. This is consistent with overall increasing market integration. The relative ranking of cities did not change much, however. Nevertheless, one might expect that in the short run such effects do exist—especially if trading opportunities are opened to foreign states, such as occurred during the Zollverein trade liberalization rounds. To simplify somewhat, in 1834 Bavaria's customs borders to the North fell away (no border anymore to Saxony and to Prussia), and in 1836 Bavaria's customs borders to the West/Northwest fell away (accession of Baden and Frankfurt). At the same time, the customs borders to the South (Switzerland and Austria), to the East (Austria), and to the West of Bavaria's Palatinate province (France) remained in place.

When the trade routes towards the North were opened in 1834, trade among the Northern Bavarian markets was likely affected. Figure 8 contrasts the cases of Bayreuth, in the North, with Lindau, in the South, and with the remaining 12 core Bavarian cities. The increase in the Geary statistic for Bayreuth in the aftermath of the Zollverein picks up the *decline* in spatial correlation with other Bavarian cities at the time. It is likely due to Bayreuth's partial re-

direction of its trade to the newly opened trade routes to Saxony and Prussia.³² This interpretation is strengthened by observing that nothing comparable occurred at the time in Bavaria's South (Lindau) or, for that matter, in the average Bavarian city (labeled "Other cities" in Figure 8).

For the second Zollverein liberalization round in 1836, I focus on Würzburg because historical accounts point to the importance of trade from Würzburg on the Main to Frankfurt (Seuffert 1857). Figure 9 shows the dynamics of spatial correlation in Würzburg, compared with Munich and the other 12 cities. For the former, spatial correlation decreases slightly around 1836, while for Bavaria as a whole there is a substantial increase in spatial correlation. Thus, the results are also here consistent with trade creation to foreign states leading to a reduction of trade integration with other Bavarian cities.

While these results appear to be temporary, they suggest that customs borders might have important effects on the evolution of market integration. In the following, the importance of customs borders as a deterrent for trade is examined by using data on grain prices not only in Bavaria, but also in other German states as well as Austria, Switzerland, and France.

4.3 Border effects

I now ask whether the borders between German states in pre-Zollverein times appear to have significantly restricted trade, and if so, to quantify this effect. As noted above, the creation of the Zollverein was a gradual process; however, two years stand out as being particularly important: the year of 1834, when the (augmented) Prussian Customs Union joined with the South German Customs Union, the Kingdom of Saxony, and the Thuringian Customs Union to

³² To use the terminology of Viner (1950), the Zollverein created trade between Bavaria and its neighbors to the North, while it diverted trade that before went from Bayreuth to other Bavarian cities (even though the Zollverein did not erect new borders in this sample).

form the German Zollverein; and the year 1836, in which Baden and Frankfurt became part of the Zollverein.

The extent of arbitrage and trade is measured by the price gap between two markets. For any state i , let p_{it} be the log of its price for wheat in year t , and let $|p_{it} - p_{jt}| = gap_{ijt}$ denote the absolute percentage gap for the two prices i and j . If the Zollverein reduces the costs of trade between regions i and j , we would expect this gap to fall. However, even if the gap falls among Zollverein members after joining, without further assumptions this cannot be taken as evidence for a causal effect.

First, there are many reasons of why this price gap may fall between any two markets, and some of these reasons would apply regardless of whether the markets happened to be located in the Zollverein area (an omitted variable problem). For instance, we expect that improvements in transport technology will mean that transport costs for wheat fell over time. Simply comparing the price gap in markets before and after their entry into the Zollverein would not adequately account for these influences. However, if the price gap among the members of the customs union declines by *more* than that among non-members, then we can be more certain that there was a significant effect due to the elimination of borders. Thus, in the analysis below I compare what happens to the price gap between states that became Zollverein members to how the price gap changes among states that did not become members of the same customs union (a so-called “difference-in-difference” approach).

The second issue is endogeneity. Even if Prussia and Bavaria join the same customs union and the price gap between these two falls by more than between Prussia and, say, Austria, it still does not provide evidence that, in general, eliminating a customs border causes improved arbitrage and more trade. It could be that there was a good reason in the first place why Prussia

formed the customs union with Bavaria, and not with Austria. If membership in a customs union is not a random event, but rather a choice on the part of the states, one cannot be sure that one is estimating only the effect of eliminating a customs border.

This is a difficult issue to address even in the contemporaneous policy evaluation literature.³³ The problem is exacerbated for this historical setting, because there is not much high-quality auxiliary data. In fact, there is evidence for much deliberation on the part of the future Zollverein members about when and under what conditions to join a customs union with Prussia, suggesting the decision of whether or not to join the union was not a purely exogenous decision.³⁴ Nevertheless, there are two elements in my approach that can help clarify the extent of endogeneity.

First, it is plausible that one major reason of why German states became allied under the Prussian-led Zollverein while other states did not has to do with language: although dialects vary across regions, the German language was spoken in all of the future Zollverein member states. Regardless of the presence of a border, common language is often associated with significantly more bilateral trade (e.g. Rose 2002). In my sample, German is the language spoken in two of the three control-group countries, Austria and Switzerland.³⁵ Hence, if a common language significantly affects the propensity of two states to eliminate customs borders between each other, then the border effect estimate of Germany/Germany versus Germany/France ought to be quite different from that of Germany/Germany versus Germany/Austria.

Second, I can estimate the Zollverein border effect from the first round in 1834 and the

³³ See Blundell and Costa Dias (2002) for an overview.

³⁴ The German states were economically diverse (see Tipton 1976 on regional development in Germany) and states' characteristics played a critical role in determining when and whether to join the union. See for example, Henderson (1939, 103-127) on the context in which individual states decided to join the Zollverein.

³⁵ Several languages were spoken in either of these countries, but in the cities that I focus on, Rorschach and Vienna, it was predominantly German.

second round in 1836. As discussed by Henderson (1939), there was not much difference in terms of the conditions at which the German states joined the Prussia-dominated Zollverein. Given that, it is plausible to assume that if endogeneity played a role in the timing of when the German states joined the Zollverein, the states that expected a higher relative gain were the ones that joined first. Thus, if endogeneity along these lines is important, one should estimate a stronger border effect from the 1834 round than from the 1836 round of joining the Zollverein. I will look for this effect below.

The regression specification that is employed is the following:

$$gap_{ijt} = \alpha_{cc'} + \beta \ln dist_{ij} + \gamma border_{ijt} + \varepsilon_{ijt}, \quad (2)$$

where $dist_{ij}$ is the geographic distance between market i and market j (measured in 100s of kilometers), the variable $border_{ijt}$ is equal to 1 if for year t there was no border between i and j , and zero otherwise, $\alpha_{cc'}$ is a bilateral country pair specific effect, and ε_{ijt} is a mean-zero but possibly heteroskedastic error term.³⁶ Bilateral distance is included as an important control variable because transport costs for wheat are expected to rise with distance. The coefficient γ on the $border$ variable is of key interest. For example, between Würzburg (Bavaria) and Frankfurt, $border_{ijt}$ changes from 0 to 1 in 1836, the year when the free city of Frankfurt joined Würzburg to become part of the Zollverein. This implies that if—on average—the price gap between Würzburg and Frankfurt shrinks after this border is eliminated, γ should be less than zero. The variable $border_{ijt}$ has been manually coded for each year and each bilateral pair (i,j) by examining whether a direct trade route between i and j in year t had to pass through one or

³⁶ The inclusion of $\alpha_{cc'}$ means that the price gap is allowed to vary for a German-German and German-Swiss pair, for example, even before the Zollverein eliminated the customs border for the German-German pair.

more customs borders.

Table 2 shows the results. In the upper part, the sample consists of those bilateral relations for which the customs border fell away in 1834 (*border* before 1834 = 0, *border* after 1834 = 1) versus those for which the customs border did not fall away during the years 1815-55 (*border* before 1834 = 0, *border* after 1834 = 0). The coefficient γ on *border* measures thus the average change in the price gap as two states become Zollverein members in 1834, relative to the change among two states that did not become members of the same customs union in 1834. Recall that in 1834, the ‘action’ was relative to Saxony and Prussia. The difference-in-difference (or “double difference”) estimator is defined as the difference in average outcome of pairs where there was a change in border (for example, Würzburg (Bavaria) and Frankfurt) *minus* the difference in average outcome in those pairs where there was no change in the border in 1834.³⁷

As a baseline, specification (1) introduces the distance variable by itself. The coefficient suggests that doubling the distance is on average associated with a 0.093 higher price gap. Specification (2) includes the *border* variable; its coefficient γ is -0.046, significantly less than zero. This means that price gaps have fallen with the elimination of Zollverein borders, which is consistent with a border effect.

Another way of looking at the size of the border effect is to ask what the implied border effect is in terms of distance, or, how much of an increase in distance is equivalent to the customs border being eliminated? This is the distance commonly known as the border-width, and is here equal to 156 kilometers (km): the presence of the border is thus effectively equivalent

³⁷ In particular, Bavaria-Bavaria bilateral pairs are never in the sample, and neither are France-France or Prussia-Prussia bilateral pairs. All are international pairings, with the exception of those between Zweibrücken and any of the fourteen Bavarian cities in the contiguous Eastern part of Bavaria.

to adding 156 km to the distance that would have to be traveled without border.³⁸

There is the possibility that German states were intrinsically more likely to join the Zollverein than non-German states/entities, for instance due to language reasons. If this were the case, the estimate of -0.046 would likely be an overestimate for non-German speaking states. In order to examine this possibility, in the following I estimate the border effect separately from German- and non-German speaking control groups.

In specification (3), the border effect is identified from cities in Germany-Germany pairs versus cities in Germany-France pairs, whereas in specification (4), the comparison is between Germany-Germany and Germany-Switzerland/Austria pairs. The border coefficient γ varies little between the two specifications. This in itself suggests that the border effect from specification (2) might not be biased in a particular direction. At the same time, the implied border width for the German-speaking pairs is estimated to be 162 km, while the width is 144 km for the different language pairs in Germany and France.

The fact that the point estimate of the border effect for Germany-France (in specification (3)) is about 11% smaller than that for Germany-Austria/Switzerland (in specification (4)) could mean that heterogeneity might play a role in determining the size of these border effects. If, in addition, common language is correlated with other important differences between city pairs, the estimated border effect might reflect how large those effects are as well. The results suggest that eliminating a border reduces trade costs equivalent to about 160 km if the economies are quite similar and equivalent to about 140 km if they are somewhat less similar to each other. If there

³⁸ This is computed as follows: in specification (2) the elimination of borders means on average a 0.046 lower price gap, while the point estimate for distance is 0.094. Engel and Rogers (1996) suggest that given the concavity of the natural log function, it might be better to use the upper 95% confidence interval estimate for distance, which is 0.104. I adopt this approach here as well. Then, the implied border width in terms of distance is the $dist^*$ that solves $0.046 = 0.104 \times \ln(dist^*)$. Here, $\ln(dist^*)$ is 0.442, or $dist^* = 1.56$. Distance is measured in hundreds of kilometers, so that the implied border width is 156 kilometers; this figure is reported in Table 2, specification (2).

were no important other differences between the French and the Swiss or Austrian cities in the sample, the difference between these estimates may provide an estimate for the influence of language on trade in early 19th century Europe: if the same language is spoken, trade is around 11% higher than if languages are different in the two cities, according to these estimates.

The lower part of Table 2 displays the results of the 1836 Zollverein round. This captures mainly the accession of Baden and Frankfurt to the German Zollverein. The interpretation of the *border* coefficient γ is identified from bilateral price gaps for which the customs border fell away in 1836 (*border* before 1836 = 0, *border* after 1836 = 1) compared to those where the customs border did not vanish over the sample period of 1815-55 (*border* before 1836 = 0, *border* after 1836 = 0).

I find that the results in 1836 are similar to those in the first Zollverein round of 1834. The coefficient on *border* in (6) is significantly negative at -0.041—and is similar to the estimate of -0.046 in specification (2) for the 1834 round. The *border* coefficients identified from the two smaller samples are -0.047 in specification (7) and -0.049 in (8) for the Germany-France and the Germany-Switzerland/Austria samples, respectively. The implied border width is again about 150 km (see (6)). Also here the estimates suggest that in terms of distance-equivalents, the border elimination was more effective for German-speaking cities than for mixed-language pairs, with the difference being a bit larger than before (152 kilometers versus 177 kilometers, respectively, or 15%). Overall, however, the results for these two Zollverein rounds are very similar.

So far I have quantified the border effect by expressing it in terms of kilometers of distance (the “Implied border width” rows in Table 2). An alternative way to estimate the

importance of borders is to examine the fraction of the price gap was accounted for by borders. In fact, this may be the preferred measure for doing so, for at least two reasons: first, it is the price gap (i.e., the extent of the deviation of the LOP) that is our primary indicator of arbitrage and market integration, and thus one may want to express the ‘size’ of the border in terms of that gap.

Second, calculating the border effect in terms of the price gap is particularly useful when making comparisons over long periods of time, as I will do in section 4.5 below. This is because the cost of transporting grain over a distance of say 200 km has become much lower since the 19th century, and therefore a ‘200 km border effect’ could mean something very different in different periods. However, transport technology improvements will also tend to lead to a smaller price gap over time. That is, in the 19th century, transport technology was relatively poor (or, distance was a relatively big obstacle) compared to the late 20th century, and price gaps were consequently relatively high compared to the late 20th century. Thus, computing border effects in terms of price gaps may be preferable to computing them in terms of distance. The results for border effects in terms of price gaps are presented in Table 3.

According to specification (2), the price gap falls by 0.046 when the border vanishes. The mean absolute price gap is 0.155, so the border effect accounts 29.7% of that. The mean log distance is 1.334, which with a distance coefficient of 0.094 means that on average distance adds 0.125, or 80.9% to the price gap. In the 1836 Zollverein round, the elimination of the border accounts for 22.4% of the mean price gap between cities (0.041 over 0.183, the mean of the dependent variable), whereas on average distance accounts for 53.9% of the price gap.

To compare the relative sizes of border and distance effects, the last row of Table 3 shows the “Relative Border Effects”, defined as the ratio of the border effect on price and the

distance effect on price. The relative border effect is 36.7% for the 1834 Zollverein round, and it is equal to 41.6% for the 1836 Zollverein round. These results suggest that on average, distance appears to have been a more important obstacle to the equalization of prices than borders were at the time.

What do these results say about endogeneity? Is there evidence that the regions that became Zollverein members were a self-selected group in the sense that they benefited more in terms of trade and arbitrage than a randomly chosen city would have? If that would be the case, one would expect that the benefits from joining the Zollverein are greater for the early-joiners than for the later-joiners, for the very reason that the former group has joined earlier than the latter. One might be concerned that the two rounds of Zollverein accession are only two years apart, which could mean that the sequence of accession does in fact not mean anything. However, it is clear from the history of the Zollverein creation that the sequence of accession is in fact indicative of which state expected a relatively high benefits-to-costs ratio, and which state did not.³⁹

Consistent with the endogeneity argument, I estimate a slightly higher border effect equivalent distance for the early joiners (1834 group) than for the later-joiners (1836 group), with 156 km versus 149 km (see Table 2, specifications (2) and (6)). At the same time, I estimate that on average, the elimination of the border did relatively more for reducing the price gaps in the 1836 round than in the 1834 round: the relative border effect is estimated at 41.6% in the 1836 round, but only 36.7% in the 1834 round (Table 3). This is the opposite of what we would expect if the 1834 group endogenously entered the Zollverein earlier based on the belief

³⁹ It is for instance clear that Baden and Frankfurt (1836 joiners) were more interested in generally lower tariffs, and thus not eager to adopt the relatively high Prussian-determined external tariff, in contrast to, for example, Bavaria and Württemberg (1834 joiners).

that the customs union would give them a larger relative gain compared to what the non-joiners could expect. There does not appear to be a clear pattern that would be consistent with an endogeneity problem. Overall, this suggests that it is unlikely that these border effect estimates are strongly affected by the endogenous choice of states to join the Zollverein.

In the following section, I examine the dynamics associated with border effects in the aftermath of customs union formation.

4.4 The Dynamics of Border Effects

I have just discussed the economic interpretation of the border effect, based on the estimate of γ (approximately -0.04 in all specifications) shown in Table 2. In addition, I ask whether the size of the border effect has exhibited any dynamics after the Zollverein rounds. In particular, did it become stronger over time? Or was there a dramatic effect right after the liberalization, but little change thereafter? A priori, it is not clear what one should expect. These issues are not addressed in the specifications in Table 2 since these specifications identify an average border effect over a period of 20 or 22 years: for the years 1836-55 in the 1836 Zollverein round, and for the years 1834-55 in the 1834 Zollverein round, respectively.

In the following, therefore, I allow for the border effect to vary by subperiod. Three different specifications are shown in Table 4, both for the 1834 and for the 1836 Zollverein round. In the top part (specification I), the estimated border effect is allowed to vary across five subperiods of four to five years each. Below (specification II), I estimate a short-run border effect for the first six years, a medium-run effect for the next six years, and the long-run effect for the remainder years in the sample. At the bottom of Table 4 (specification III), I distinguish only the short-run from the long-run effect (roughly the first and the second decade after

elimination of the customs borders).

Overall, the dynamics for the two Zollverein rounds are similar, and they suggest that the strength of the border effect has increased over time (for the 1836 round, this effect is monotonic). For instance, the parameter γ is -0.043 during the years 1834/37 and -0.089 during the years 1852/55 for the 1834 Zollverein round, while the comparable results for the 1836 round are indistinguishable from 0 and -0.127, respectively (see specification I).

The result that the elimination of customs borders appears to generate effects that become stronger over time is consistent with at least two explanations. First, there may be adjustment costs to establishing new trade links, as well as to re-directing existing ones. When certain trade routes become available as a consequence of selective liberalization, new trade networks have to be formed, new warehouses built, etc, all of which takes time. Thus, the effects of eliminating a border on trade and market integration may get stronger over time. Second, there could also be an endogenous response that affects trade possibilities inside and outside the Zollverein. One important example of this could well be that the emerging German railway network was built with an emphasis on connecting cities within the Zollverein territory. Another example is the creation of a monetary union among Zollverein members around 1838, as mentioned above. Such effects, supported by either private agents or the state and built up over time, might have had a cumulative impact on trade and market integration. My finding of an increasing price gap differential among Zollverein members and non-members is consistent with these arguments.

In the following section, I turn to a comparison of my results with other border effect estimates.

4.5 The Size of the Border Effect Compared

How large is this effect in comparison to other estimates? Table 3 summarizes my results and compares them to those of Engel and Rogers (1996), a highly influential paper that estimates border effects between U.S. and Canadian cities for the years 1978 and 1994. Before proceeding, it is important to note how their approach differs from what I have presented above.

First, Engel and Rogers (1996) estimate border effects for the *relative* LOP by studying the volatility of the two-month change in relative prices between cities. The authors have access only to price indices data, rather than product level price data, so they estimate border effects as a deviation from the relative law of one price. Estimates of the *absolute* LOP are generally preferable if product level data is available, as they are here, so I focus on that.⁴⁰ Second, Engel and Rogers (1996) estimate their border effects for a diverse set of goods and services ranging from medical care to footwear to alcoholic beverages. Some of these are highly differentiated goods, and others are non-tradables, both of which are likely to face additional barriers that prevent arbitrage compared to wheat, the good under analysis here.

A third difference lies in the fact that Engel and Rogers (1996) identify their border effect with data from U.S. and Canadian cities only from cross-sectional variation—the U.S.-Canadian border existed throughout their sample period—whereas my analysis uses both time series and cross-sectional variation.

The preferred estimate of Engel and Rogers for the border effect in terms of distance is 2,848 kilometers, considerably larger than what I estimate here (see Table 3 for a comparison).⁴¹ When one computes the border effect not in terms of distance, but rather in terms of price behavior, my border effect estimates are also smaller than those of Engel and Rogers: according

⁴⁰ This is in line with Parsley and Wei (1996) as well as Goldberg and Verboven (2003), even though these authors do not estimate border effects.

⁴¹ This is the average of Engel and Roger's border effect estimates across 14 product categories. For the one category ('food at home') that is closest to wheat, the product I analyze, their border effect estimate is even larger (about 3,987 km).

to my estimates, the relative border effect on price is about 40%, whereas Engel and Rogers estimate it to be about 160% (see Table 3, last row). This means that the relative importance of border and distance are reversed in my and in Engel and Rogers' research.

How much of that is due to the fact that Engel and Rogers study the relative LOP using monthly data, whereas my regressions examine the absolute LOP using annual data? I have used a subset of my sample for which monthly data is available, together with their methods in order to shed some light on that.⁴² The border effect using Engel and Rogers-type methods is 149 km in the years 1825-33, and it is 121 km in the years 1837-55. The implied border width using these numbers is thus about 130 km. Border effects account for 7% and 4.4%, respectively, of the mean volatility of the price gap, while distance explains 25.5% and 35.8%. Thus, the differences in research design—especially absolute versus relative LOP—between Engel and Rogers' and my study do not appear to be of major importance in explaining why I estimate relatively small border effects for the 19th century, compared to their 20th century estimates.

In part, the differences in terms of results may indeed have to do with the homogeneous versus differentiated goods characteristics that are analyzed in the two studies. At least as important, however, appears to be the identification issue. It may well be that the size of the border effect is overestimated if there is no time series variation that can identify it. Future research may be needed to fully settle these issues. At this point, I note that the border effects estimated in this paper are not large compared to what might have been expected for early 19th century Europe based on the results of existing studies for the 20th century.

⁴² Specifically, using monthly prices on France and Bavaria, I estimate the width of the border by looking at the standard deviation of the difference in the log of the relative price between time t and $t-2$ as the measure of the relative price in location j relative to the price in location k ; this corresponds closely to the method used in Engel and Rogers (1996).

5. Conclusions

The border changes that occurred under the Zollverein provide a unique opportunity for understanding border effects because not just a few, but a sizeable number of states decided to join (or not join) the union. This provides a large number of bilateral observations with which to study the border effect. This paper uses prices in France, Switzerland, the Habsburg Empire of Austria, and fifteen Bavarian cities for the years 1815 to 1855 to assess the size of border effects in the context of the German Zollverein. Local indicators of spatial association show that within Bavaria, cities near the center (Nürnberg, Augsburg, and Landshut) are more highly integrated than cities in the sample's periphery. In the years immediately subsequent to the Zollverein liberalization rounds of 1834 and 1836, cities closest to the newly opened border became more integrated with these trade partners. At the same time, those cities located further away from the newly opened border may have seen a slight reduction in their trade integration, an effect that suggests the presence of trade diversion. Both effects, trade creation and trade diversion, appear to be temporary.

The main result of my analysis is that the estimated border effects for Europe in the early to mid-19th century are small compared to estimates using contemporary data. For the 1834 liberalization round, the implied border effect, calculated as the implied decrease in distance that comes about as the result of the customs border being eliminated, is between 140 and 160 kilometers, with the smaller distance for non-German speaking cities, and the larger distance for German speaking cities. Thus, common language in our sample provides for an additional benefit of lowering trade barriers by 11%, making border elimination more valuable among German-speaking cities than for mixed-language speaking cities.

In addition, the results suggest that the border effect for early joiners to the Zollverein

was not substantially different from that for late joiners. For both rounds, the Zollverein effects become stronger over time, consistent with future paths of market integration being in part shaped by Zollverein membership—and that may have been especially true because of both railway building and monetary integration.

One reason border width estimates in the historical sample are not as large as that found in contemporary samples may be because this calculation depends on the size of the distance effect. Overall trading possibilities in the 19th century were more constrained in the first instance by virtue of distance, whereas trade today is less constrained by distance. Where the width of the border depends on the distance effect, the former will be magnified by the extent of the latter. This cannot be the only explanation, however, since I still estimate smaller border effects in terms of its effect on price directly when no distance effect is involved (see Table 3). It is very likely that another reason for the relatively small border effect estimate found in this paper is that the analysis uses a difference-in-difference approach. Thus, I compare the difference in outcomes before and after the Zollverein for Zollverein members with the before and after of a comparison group of non-Zollverein members. The border effect, as it is often estimated, does not focus on the periods when borders changed, nor does it explicitly compare the border effect pairs with control group pairs. Instead, a large and unspecific number of inter-country differences appear to be captured in the estimate, which may lead to the border width being overstated.

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Table 1: Summary Statistics

City	State/Country	Price*		No. of observ.**	Distance***
		Average	Std. Dev		
Chalon-sur-Marne	France	18.94	5.38	31	596.08
Luneville	France	19.43	5.76	31	415.73
Bar-le-Duc	France	18.52	5.54	31	522.89
Stuttgart	Wurttemberg	20.28	6.27	23	258.62
Rorschach	Switzerland	21.50	8.73	14	293.40
Karlsruhe	Baden	16.15	2.10	28	287.56
Magdeburg	Prussia	15.36	5.06	39	419.55
Koblenz	Prussia	18.87	6.44	39	356.35
Munster	Prussia	17.77	6.18	39	442.62
Frankfurt	Free City	21.71	8.06	16	294.00
Bamberg	Bavaria	16.32	6.14	41	260.22
Erding	Bavaria	16.33	8.09	41	293.49
Landshut	Bavaria	15.58	7.55	41	302.03
Straubing	Bavaria	14.65	7.34	41	329.38
Regensburg	Bavaria	15.09	7.21	41	294.78
Bayreuth	Bavaria	16.82	6.41	41	284.59
Wurzburg	Bavaria	16.41	6.72	41	253.02
Augsburg	Bavaria	16.92	7.87	41	252.43
Kempten	Bavaria	18.81	7.82	41	271.66
Lindau	Bavaria	19.14	8.05	41	284.96
Memmingen	Bavaria	18.00	7.69	41	258.82
Nordlingen	Bavaria	16.14	7.73	41	239.19
Munich	Bavaria	17.59	8.11	41	283.89
Zweibrucken	Bavaria	17.60	5.79	38	345.26
Nurnberg	Bavaria	16.27	7.33	41	252.26
Vienna	Austria	17.32	9.01	41	682.13
Sample average		17.61	6.87		337.51
Sum				944	

* Average annual price of wheat; in Bavarian Gulden per Bavarian Schaffel

** Number of annual price observations between 1815 and 1855;
prices for Bavaria and France are computed from monthly prices

*** Average of bilateral Euclidean distances, in kilometers

Table 2**Zollverein Round 1834**

	(1)	(2)	(3)	(4)
			{Ger-Ger vs Ger-Fra}	{Ger-Ger vs Ger-Sw/Aus}
Distance	0.093** (0.005)	0.094** (0.005)	0.107** (0.006)	0.082** (0.006)
Border		-0.046** (0.005)	-0.043** (0.005)	-0.045** (0.006)
Rbar-sq	0.085	0.095	0.103	0.108
F-stat (p-value)	75.7 (0.000)	82.11 (0.000)	122.08 (0.000)	56.34 (0.000)
# of obs.	4988	4988	3783	3142
Implied border width***		156 km	144 km	162 km

Zollverein Round 1836

	(5)	(6)	(7)	(8)
			{Ger-Ger vs Ger-Fra}	{Ger-Ger vs Ger-Sw/Aus}
Distance	0.093** (0.005)	0.093** (0.005)	0.101** (0.007)	0.074** (0.006)
Border		-0.041** (0.007)	-0.047** (0.008)	-0.049** (0.008)
Rbar-sq	0.065	0.071	0.079	0.069
F-stat (p-value)	43.55 (0.000)	56.30 (0.000)	67.46 (0.000)	34.84 (0.000)
# of obs.	4299	4299	3091	2463
Implied border width***		149 km	152 km	177 km

All regressions include fixed effects for each country pair

Huber-White heteroskedasticity-consistent standard errors in parentheses

** (*) indicates significantly different from zero at a 1% (5%) level

*** Computed from distance coefficient at upper 95% confidence interval; c.f. Engel/Rogers (1996)

Table 3: Relative Border Effects and 19th Century and 20th Century Border Effect Estimates Compared

	1834 Zollverein Round	1836 Zollverein Round	Engel/Rogers (1996)
	Table 2, (2)	Table 2, (6)	(page 1120)
Implied Border Width** (kilometers)	156	149	2848*
Border Effect on Price*** (%)	29.7	22.4	32.4
Distance Effect on Price**** (%)	80.9	53.9	20.3
Relative Border Effect***** (%)	36.7	41.6	159.6

* 2848 kilometers = 1780 miles

** Computation: see text

*** Border coefficient divided by average of left-hand side variable for cross-border pairs

**** Distance coefficient times average log distance over average of left-hand side variable, both for cross-border pairs

***** Border Effect on Price divided by Distance Effect on Price

Table 4: Border Effects Dynamics

		Zollverein Round 1834		Zollverein Round 1836	
Specification I					
Distance		0.094** (0.005)		0.096** (0.005)	
Border Effects	Years 1834/37	-0.043** (0.008)		Years 1836/39	0.012 (0.011)
	Years 1838/41	-0.012 (0.008)		Years 1840/43	-0.021* (0.010)
	Years 1842/46	-0.029** (0.008)		Years 1844/47	-0.080** (0.010)
	Years 1847/51	-0.067** (0.007)		Years 1848/51	-0.094** (0.011)
	Years 1852/55	-0.089** (0.007)		Years 1852/55	-0.127** (0.011)
Rbar-sq		0.102		0.083	
Specification II					
Distance		0.094** (0.005)		0.095** (0.005)	
Border Effects	Years 1834/39	-0.030** (0.007)		Years 1836/41	0.005 (0.010)
	Years 1840/45	-0.025** (0.007)		Years 1842/47	-0.060** (0.008)
	Years 1846/55	-0.071** (0.005)		Years 1848/55	-0.105** (0.008)
Rbar-sq		0.100		0.082	
Specification III					
Distance		0.094** (0.005)		0.095** (0.005)	
Border Effects	Years 1834/44	-0.026** (0.006)		Years 1836/45	-0.018* (0.008)
	Years 1838/41	-0.068** (0.006)		Years 1846/55	-0.100** (0.008)
Rbar-sq		0.100		0.079	

All regressions include fixed effects for each country pair

Huber-White heteroskedasticity-consistent standard errors in parentheses

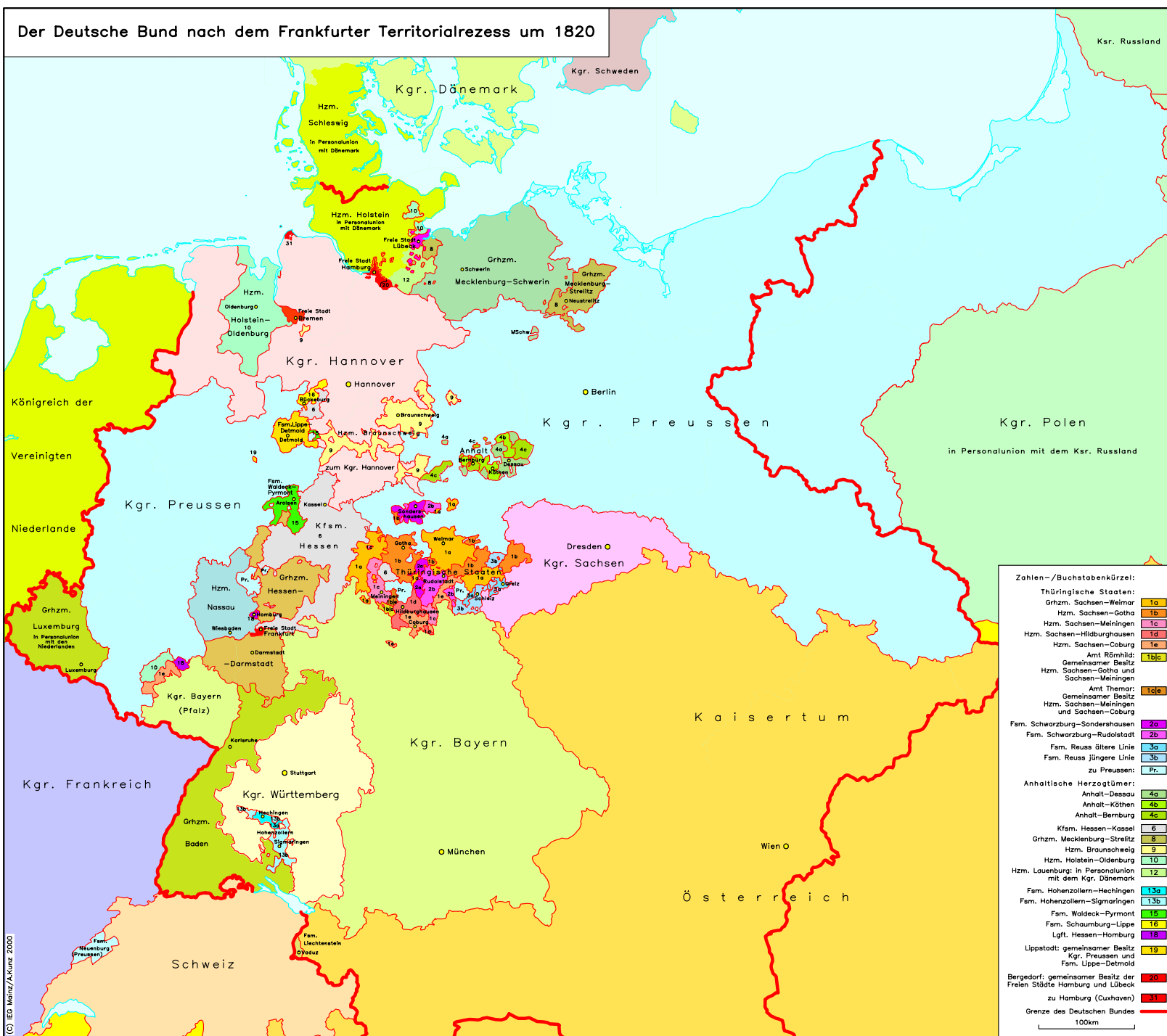
** (*) indicates significantly different from zero at a 1% (5%) level

Number of observations: 4988 for Zollverein round 1834, and 4299 for Zollverein round 1836

List of Figures

- Figure 1** The German Confederation around 1820 (“Der Deutsche Bund nach dem Frankfurter Territorialrezess um 1820”)
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Der Deutsche Bund nach dem Frankfurter Territorialrezess um 1820

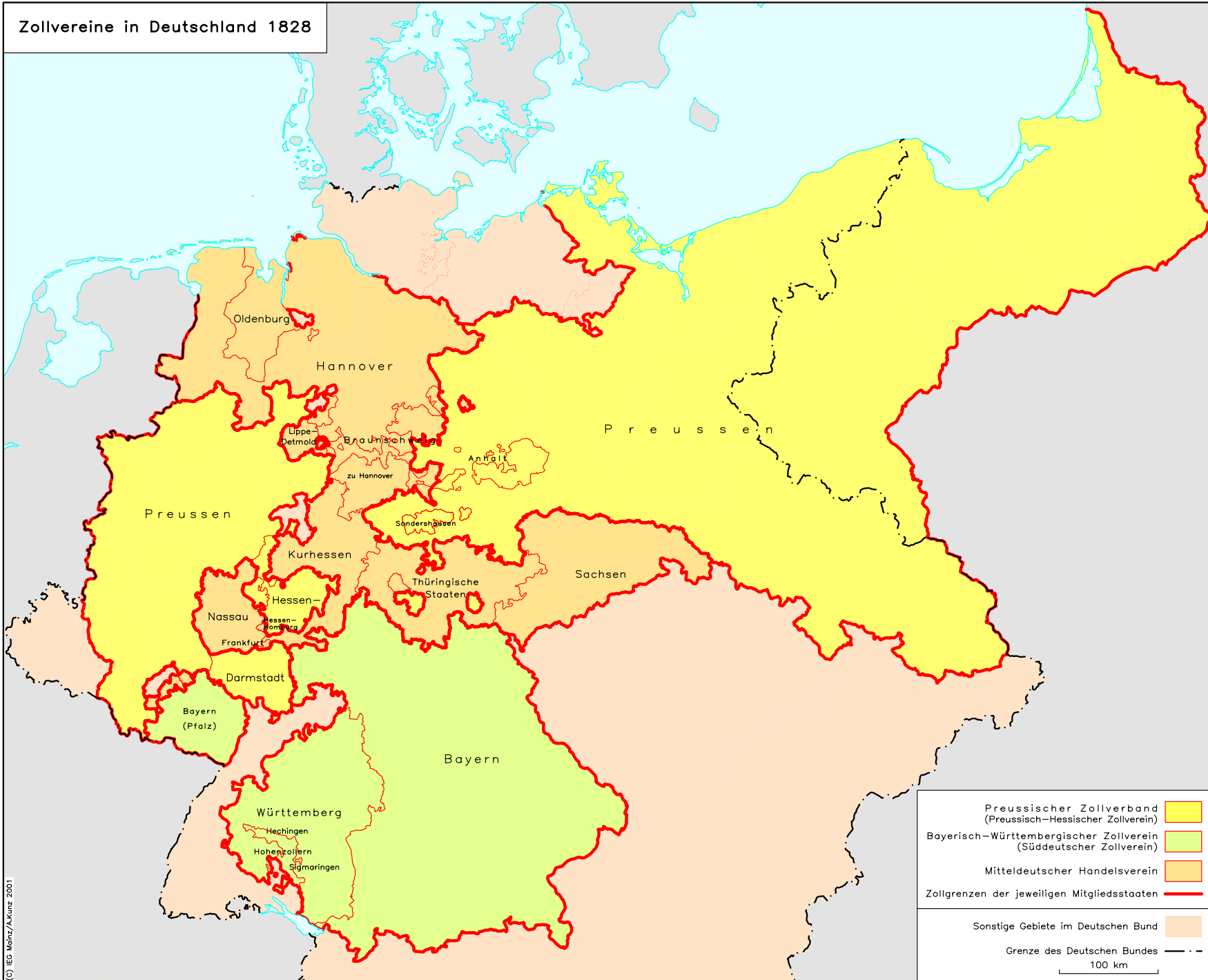


Zahlen-/Buchstabenkürzel:

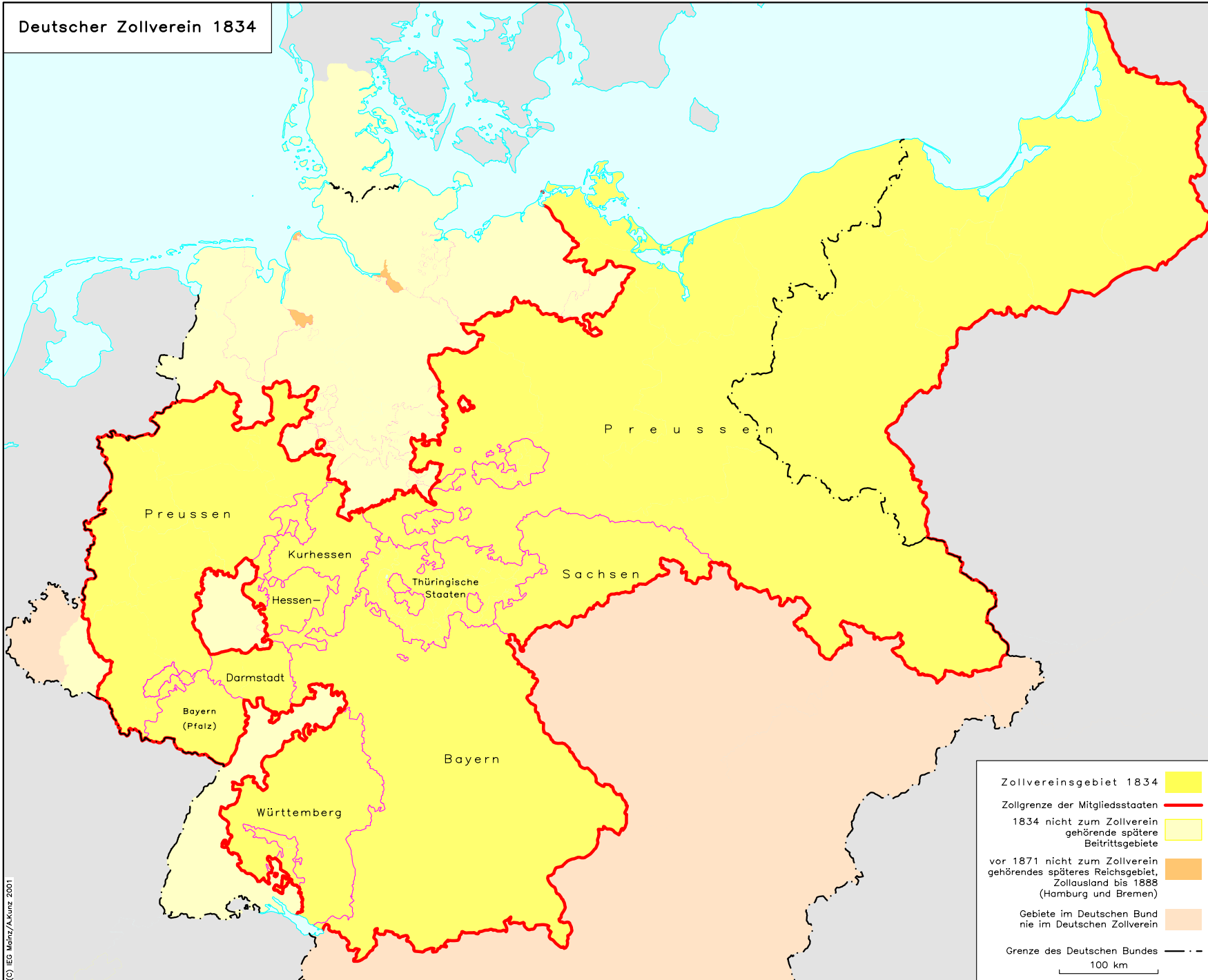
- Thüringische Staaten:
- Grhzm. Sachsen-Weimar 1a
- Hzm. Sachsen-Gotha 1b
- Hzm. Sachsen-Meiningen 1c
- Hzm. Sachsen-Hildburghausen 1d
- Hzm. Sachsen-Coburg 1e
- Amt Römhlid: 1blc
- Gemeinsamer Besitz 1blc
- Hzm. Sachsen-Gotha und Sachsen-Meiningen 1blc
- Amt Themar: 1ole
- Gemeinsamer Besitz 1ole
- Hzm. Sachsen-Meiningen und Sachsen-Coburg 1ole
- Fam. Schwarzburg-Sondershausen 2a
- Fam. Schwarzburg-Rudolstadt 2b
- Fam. Reuss ältere Linie 3a
- Fam. Reuss jüngere Linie 3b
- zu Preussen: Pr.
- Anhaltische Herzogtümer:
- Anhalt-Dessau 4a
- Anhalt-Köthen 4b
- Anhalt-Bernburg 4c
- Kfam. Hessen-Kassel 6
- Grhzm. Mecklenburg-Strelitz 8
- Hzm. Braunschweig 9
- Hzm. Holstein-Oldenburg 10
- Hzm. Lauenburg: in Personalunion mit dem Kgr. Dänemark 12
- Fam. Hohenzollern-Hechingen 13a
- Fam. Hohenzollern-Sigmaringen 13b
- Fam. Waldeck-Pyrmont 15
- Fam. Schaumburg-Lippe 16
- Fam. Lippe-Detmold 17
- Lippstadt: gemeinsamer Besitz Kgr. Preussen und Fam. Lippe-Detmold 19
- Bergedorf: gemeinsamer Besitz der Freien Städte Hamburg und Lübeck zu Hamburg (Cuxhaven) 20
- Freie Städte Hamburg und Lübeck 21
- Grenze des Deutschen Bundes 22


100km


Zollvereine in Deutschland 1828





Deutscher Zollverein 1834





Zollvereinsgebiet 1834 

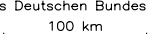
Zollgrenze der Mitgliedsstaaten 

1834 nicht zum Zollverein gehörende spätere Beitrittsgebiete 

vor 1871 nicht zum Zollverein gehörendes späteres Reichsgebiet, Zollaussland bis 1866 (Hamburg und Bremen) 

Gebiete im Deutschen Bund nie im Deutschen Zollverein 

Grenze des Deutschen Bundes 

100 km 

Deutscher Zollverein 1836

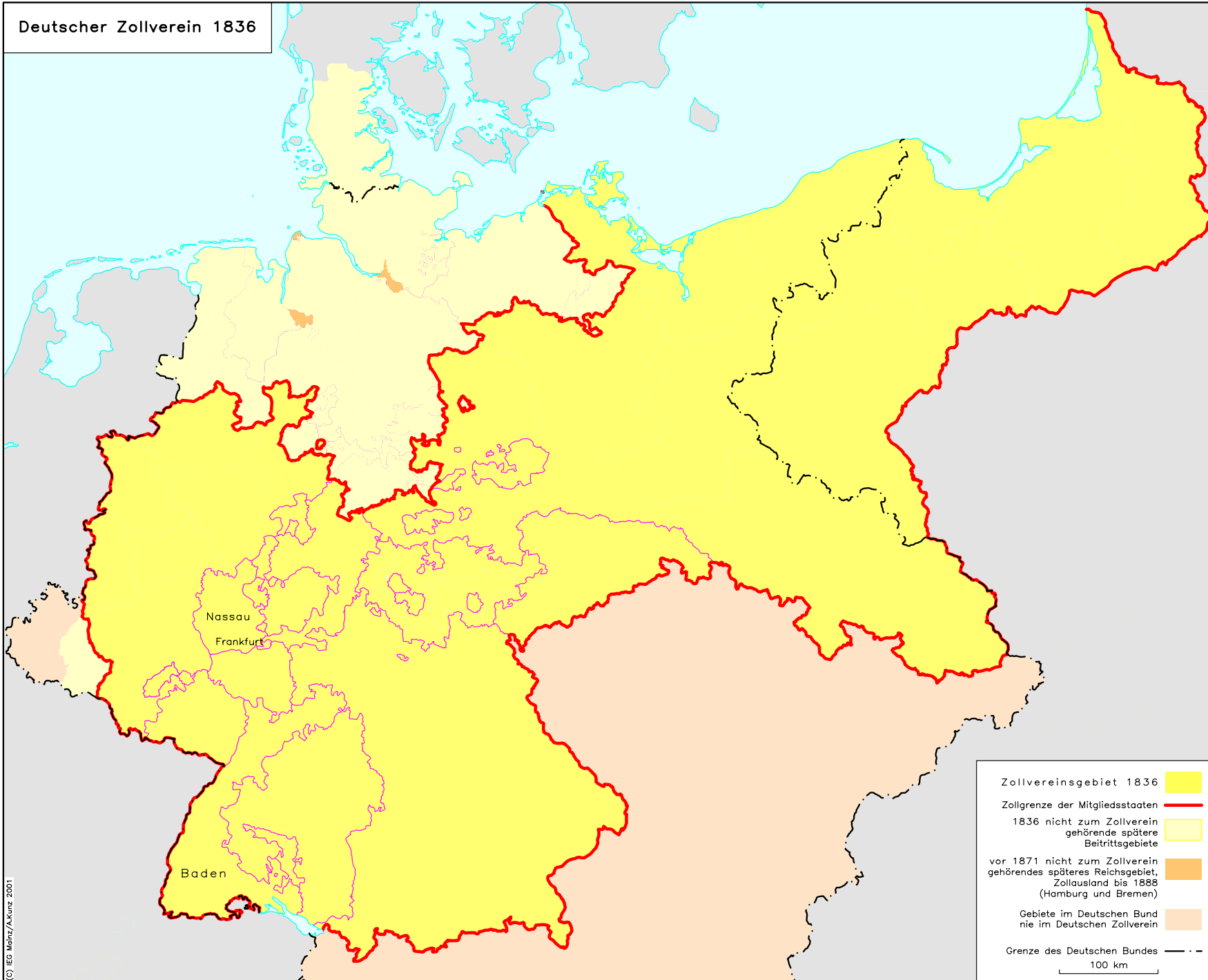


Figure 5: Bavaria and Central Europe in 1820



Figure 6. Spatial Correlation in Bavaria, 1815-1855

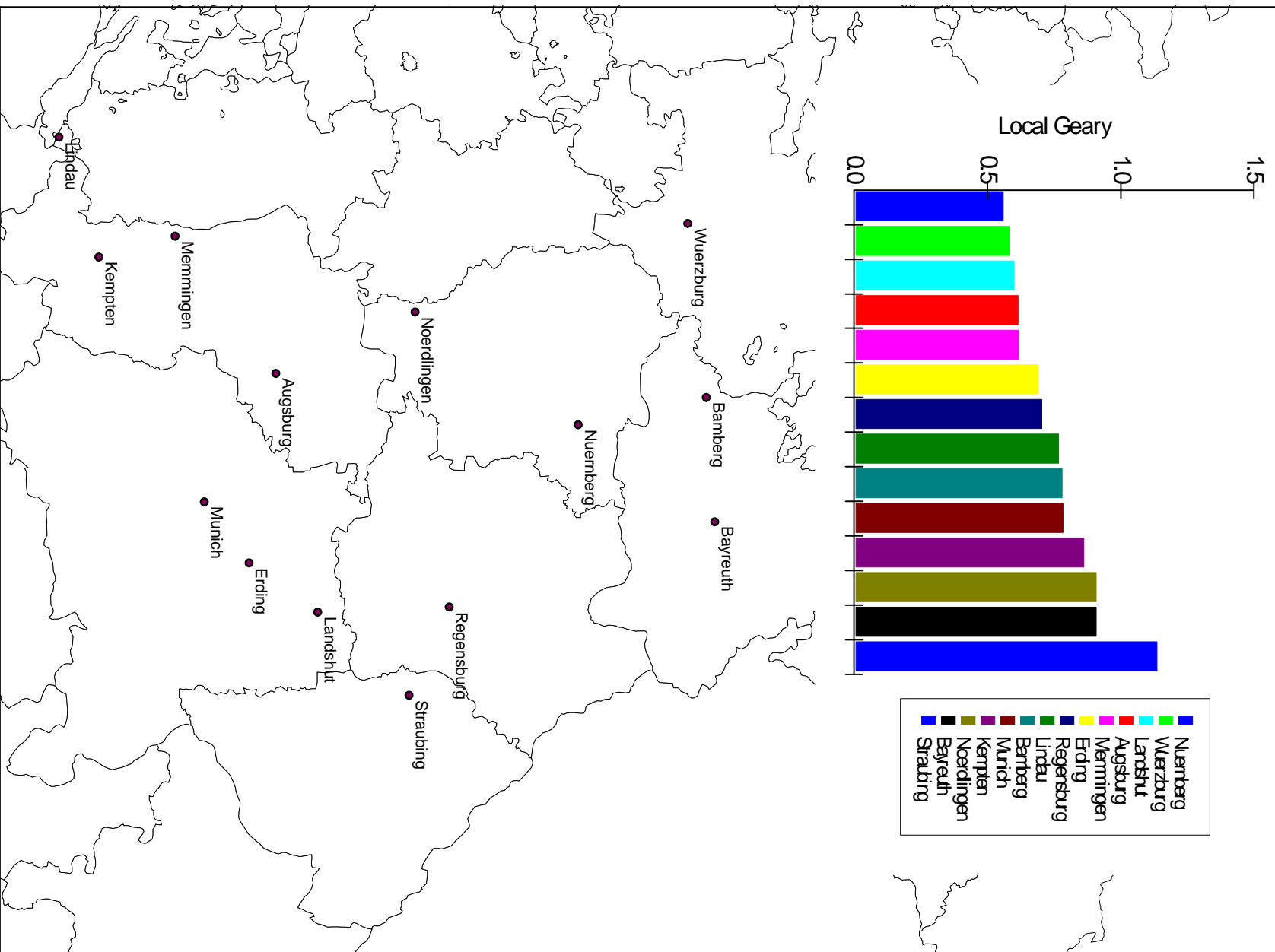


Figure 7. Spatial Correlation and Rivers

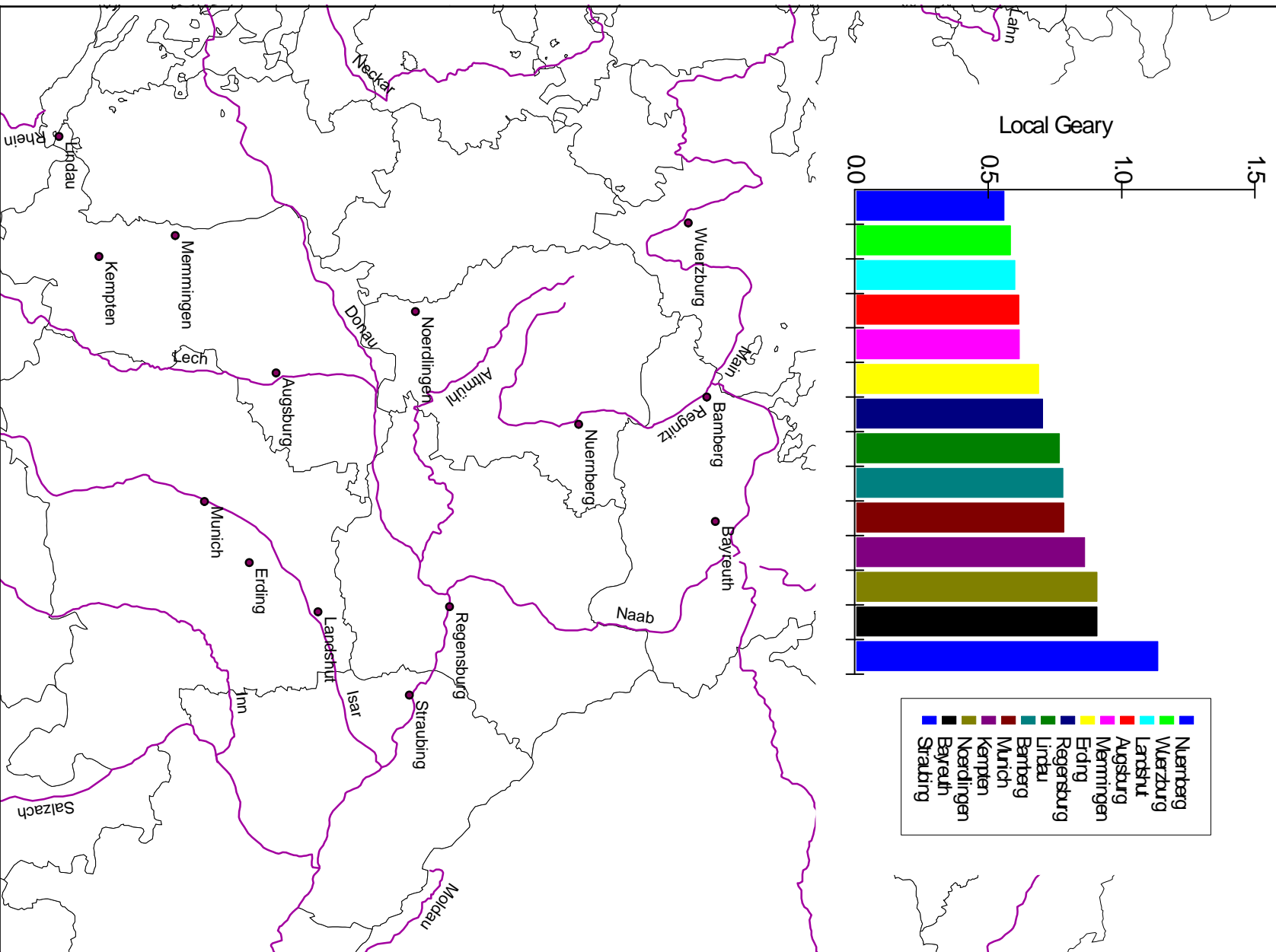


Figure 8: Changes in Spatial Correlation in the 1834 Zollverein Round

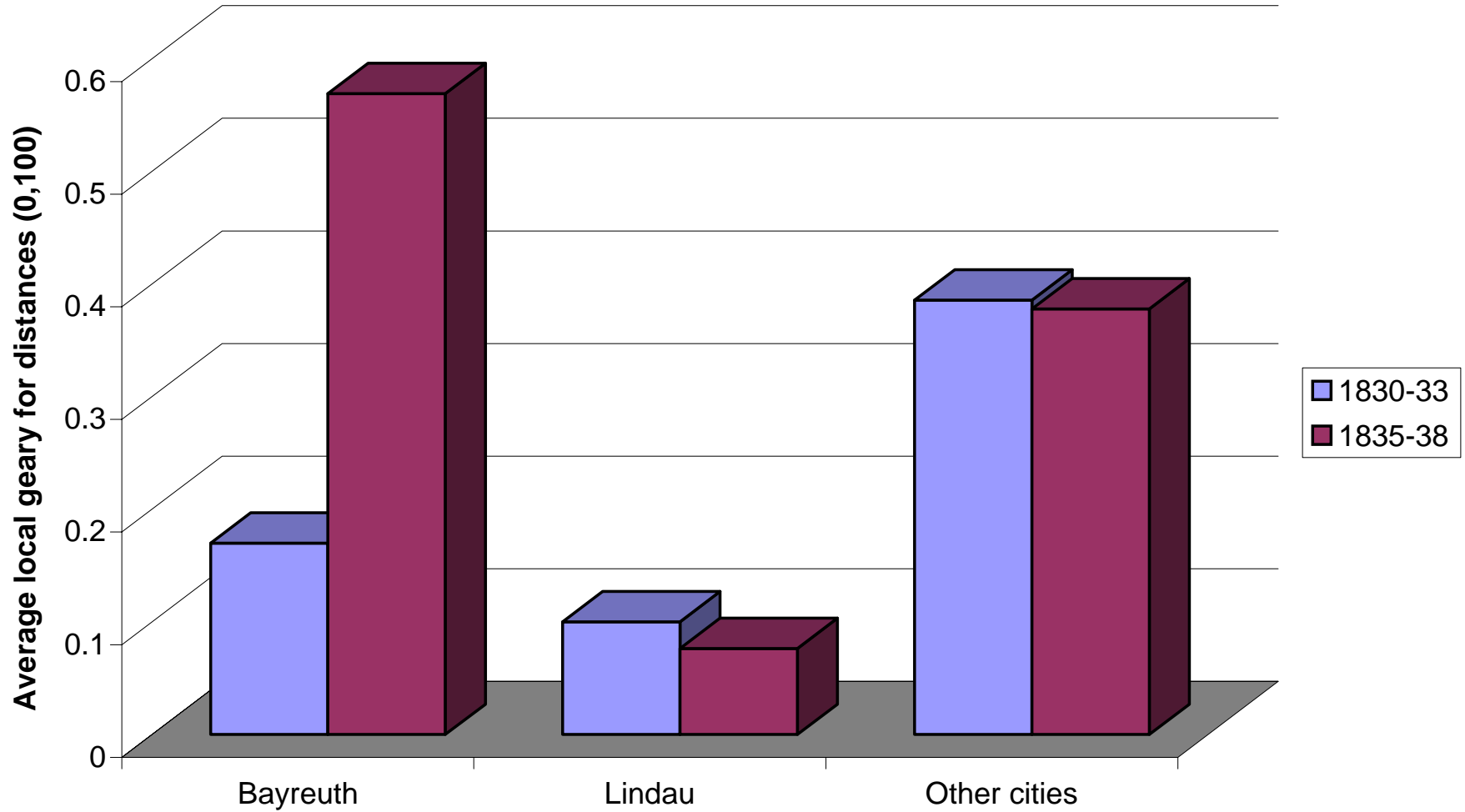


Figure 9: Changes in Spatial Correlation in 1836 Zollverein Round

