



ELSEVIER

Contents lists available at [ScienceDirect](https://www.sciencedirect.com)

Journal of Comparative Economics

journal homepage: www.elsevier.com/locate/jce

State formation and market integration: Germany, 1780–1830

Hakon Albers^{a,*}, Ulrich Pfister^b^a Martin Luther University Halle-Wittenberg, Department of Economics, Universitätsring 3, D-06108, Halle, Germany^b University of Münster, Institute of Economic and Social History, Domplatz 20-22, D-48143, Münster, Germany

ARTICLE INFO

JEL:

F14

N13

N43

N53

N73

O43

Keywords:

State formation

Market integration

Tariffs

Border

War

ABSTRACT

The rise of modern states is an important factor for economic development. We test the effects of territorial consolidation and the increase in legal capacity on market integration. The political transformation of Germany in the wake of the Napoleonic Wars, which reduced territorial fragmentation and transformed former semi-autonomous estates to sovereign polities, serves as a natural experiment. We apply a difference-in-differences framework to a new dataset of grain prices and show that territorial consolidation reduced trade costs conditional on trade reforms that replaced heterogeneous internal duties by a unified system of external tariffs. The effect was equivalent to a reduction of price gaps by 31 percent. Cities that were part of Prussia both before and after the Wars and experienced trade reform saw a reduction in price gaps of similar magnitude. By contrast, there was no market integration in late-reformer states such as Hanover and Saxony. Trade reforms were the main channel through which territorial consolidation fostered market integration.

1. Introduction

The rise of modern states is widely considered an important factor for economic development. A potentially relevant channel is market regulation, whereby states impact on trade costs. In order to implement and enforce market regulations, a state requires legal capacity, that is, the capacity to enforce its rule over its territory. We use the sweeping political transformation that Germany experienced in the wake of the Revolutionary and Napoleonic Wars (1792–1815) to explore and quantify the effect of increasing legal capacity in combination with territorial expansion on market integration.

Our empirical strategy exploits the variation across time and space produced by the massive transformation of Germany's political organization during and the immediate aftermath of the Wars. On the one hand, the German lands experienced massive territorial consolidation, which eliminated many borders and thus created a potential for the development of domestic markets. On the other hand, with the abolition of the Holy Roman Empire in 1806, political entities changed their status from semi-autonomous estates to sovereign polities with full legal capacity. Some immediately embarked on ambitious trade reforms that replaced a plethora of internal duties and tolls with a system of external tariffs and thereby created a unified internal market. Other states implemented similar reforms much later. Additionally, the Kingdom of Prussia featured regions without territorial changes, which only experienced trade reforms (Prussian Customs Law). This variation between different territories (territorial consolidation with trade reform, territorial consolidation without trade reform, no territorial consolidation but trade reform) serves to assess the roles of tariff reforms and territorial consolidation in the aggregate effect of the political transformation on market integration.

* Corresponding author

E-mail address: hakon.albers@wiwi.uni-halle.de (H. Albers).<https://doi.org/10.1016/j.jce.2023.01.007>

Received 29 October 2021; Received in revised form 19 January 2023; Accepted 31 January 2023

Available online 3 March 2023

0147-5967/© 2023 The Authors. Published by Elsevier Inc. on behalf of Association for Comparative Economic Studies. This is an open access article under the CC BY license (<http://creativecommons.org/licenses/by/4.0/>).

To quantify the effects of territorial consolidation and trade reform based on full legal capacity, we follow the market integration literature and relate price gaps, that is, a measure of trade costs, to factors that impacted on the latter, most importantly the removal of borders and the implementation of trade reforms. To explore the effect of the war shock and the concomitant transformation of Germany's state system on trade costs we employ a difference-in-differences (DD) specification.

The results show that territorial consolidation reduced trade costs conditional on trade reforms that replaced heterogeneous internal duties by a unified system of external tariffs. Specifically, city pairs that were newly included into larger territories *and* which experienced trade reforms experienced a reduction in their price gaps of about 6 percentage points, which corresponds to 31 percent of the price gap prevailing before the Wars. Similarly, the Prussian Customs Law of 1818, a major element of institutional unification, reduced trade costs within those parts of the Prussian territory where no territorial changes appeared by about 43 percent relative to the pre-war level. These results establish that the effect of territorial consolidation in reform-oriented states resulted primarily from the replacement of internal tolls by a system of external tariffs.

The remainder of our study is structured as follows. [Section 2](#) reviews the related literature. [Section 3](#) describes those aspects of state formation whose effects we will explore in our empirical analysis on market integration. [Section 4](#) introduces the theoretical background, and [Section 5](#) describes the data and the empirical strategy. [Section 6](#) presents the results and [Section 7](#) provides a summary of the robustness checks; [Section 8](#) concludes.

2. Related literature

While markets partly operate on the basis of private institutions, the rise of effective modern states is widely considered to have contributed to the functioning of markets (Greif, 2000; Johnson and Koyama, 2017). Specifically, legal capacity, that is, the ability to enforce a state's rule across its territory, was a precondition for the development of its administrative infrastructure: the provision of property rights, market regulations, and dispute resolution mechanisms such as courts (Dincecco and Katz, 2016, p. 189). However, quantitative evidence concerning the link between the growth of legal capacity and market integration is scanty.

Our analysis of territorial consolidation, trade reform and bilateral price gaps builds on two established insights. First, static effects of borders are well documented, although effects of border changes appear weaker; borders also correlate with other factors than trade policy that impact on trade costs, which we discuss in [Section 4](#) below (Federico, 2018, pp. 12, 16; Schulze and Wolf, 2009; Wolf et al., 2011). Second, the capacity of emerging states to overcome institutional fragmentation and to impose a unified institutional framework has been considered crucial for market integration and economic development already in pre-industrial times (Epstein, 2000, chs. 1 and 2; Malinowski, 2019). Dincecco (2010) has identified the prevalence of domestic tariffs as a major aspect of fragmented sovereignty and has documented the abolition of internal tolls and the unification of trade legislation within various European states during the eighteenth and nineteenth centuries. Nevertheless, the effect of this process on market integration has not been studied so far.

While the literature that examines the evolution of market integration in the long run using grain prices has sometimes invoked processes related to state formation as drivers of market integration, the evidence is mostly impressionistic and far from conclusive. Chilosi et al. (2013, pp. 58–60) refer to early political centralization and the rise of bureaucratic absolutism as factors lowering trade costs, but Federico et al. (2021, pp. 298–302), who follow a similar approach, do not relate the trends they observe with state formation. A further related study on the behavior of grain prices in Italy does not suggest that territorial consolidation during the Napoleonic era improved market integration (Federico and Dincecco, 2022, pp. 50–51, 54–55).

Studies dealing with market integration in nineteenth century Germany¹ have focused on the emergence of inclusive markets following French rule during the Napoleonic Wars (Keller and Shiu, 2020)² and on inter-state cooperation. The latter materialized in the formation of the German customs union—the *Zollverein*—in 1834 (Huning and Wolf, 2019; Keller and Shiu, 2014; Ploeckl, 2021; Tilly and Kopsidis, 2020, ch. 5). Inter-state cooperation also helped to reduce trade costs but appeared later and constituted another aspect of state capacity ('between states') than the growth of the *internal* strength of states and the market territory they form within their borders ('within states'), the dimension that is the focus of this study. Furthermore, the datasets of previous studies on market integration in Germany did not cover the period before 1815, which limited their capacity to analyze the effects of the political transformations taking place around 1800 in detail.

3. The war shock: territorial consolidation and the transformation of tariff regimes

In this section, we describe the exogenous variation in factors that impacted on grain markets and that we exploit in our empirical analysis below, namely, changes in territorial boundaries and the transformation of tariff regimes made possible by the increased legal capacity of states. [Section 3.1](#) describes the group of city pairs that were integrated into the same territory as a result of boundary changes, whereas [Sections 3.2](#) to [3.5](#) show how the dissolution of the Holy Roman Empire increased the legal capacity of German states and to what extent the latter exploited the resulting potential for reforming domestic trade.

¹ Apart from trade, a large literature studies various questions in growth and development by analyzing 'Germany' in different historical contexts (e.g., Acemoglu et al., 2011; Becker et al., 2011; Boerner et al., 2021; Burda and Severgnini, 2018; Huning and Wahl, 2021; Pfister und Fertig, 2020, among many others).

² Apart from Acemoglu et al. (2011) (cf. Kopsidis and Bromley, 2016 for a critique), studies on the economic impact of the Napoleonic wars through institutional change include Buggle (2016) on social capital and Donges et al. (2022) on innovation. For an overview, see Pfister (2022a).

3.1. Territorial consolidation and the political geography of grain markets

We study grain prices in a sample of 56 German towns and eight foreign markets situated in neighbouring regions. ‘Germany’ is defined by the overlap between the boundaries of the Holy Roman Empire in 1791 (bold line in Fig. 1) and the territory of the nation state formed in 1871 (cf. Pfister and Fertig, 2020, p. 1151). The location of the sample cities is shown in Figs. 1 - 3. The poor capacity of premodern states to provide for institutions supporting domestic trade is indicated by the fact that in 1791 only 9.4 percent of the German city pairs were part of the same territory. This reflects the political fragmentation of the German lands prior to the nineteenth century (Fig. 1). Most states did not have a territory of economically meaningful size in the sense that it offered a potential for the development of a domestic market with sufficient opportunities for arbitrage.

The Revolutionary and Napoleonic Wars transformed Germany’s political landscape from around 150 semi-autonomous estates within the Holy Roman Empire and a plethora of small dominions to a federation of about 35 sovereign states in 1815 (von Lancizolle, 1830, pp. 1–6, 134–35; for an overview of this process and its economic effects, see Pfister, 2022a). 60 percent of the German population were now citizens of one of the two largest states, Prussia and Bavaria (Fertig et al., 2018). Fig. 2 shows those states for which we observe data for at least two markets.

The ‘treatment’ of the Wars manifests itself in the elimination of a border between two markets forming a city-pair; we designate these city-pairs as ‘newly included’ in a territory. As a consequence of territorial consolidation, 23 of the German towns in our sample (41 percent) were newly included into either of the larger states, that is, Bavaria, Hanover, Mecklenburg, and Prussia. 139 out of 457 German city pairs, that is, 30 percent were now part of the same territory. These 30 percent include 96 newly included pairs and 43 pairs in which both cities belonged to the same territory both before and after the Wars, designated as ‘old territory’ pairs. In Fig. 3, we mark the newly included city pairs with black dotted lines. The tripling of the number of city pairs in which both cities belonged to the same territory between 1792 and 1815 illustrates the substantial potential for market integration that came about as a result of the territorial consolidation during the war period.

As an illustration, consider the case of Prussia. 11 cities were part of Prussia both in 1791 and after 1815 (21 pairs). Another 11 cities were incorporated into the kingdom, so that by 1816 a total of 22 towns in our sample were in Prussian territory. All pairs with old and new Prussian cities and between all new Prussian cities are ‘newly included’ pairs, specifically, 65 pairs. Note that mathematically more pairs are possible: with n cities, there are $n(n-1)/2$ city pairs: 11 old cities have 55 possible pairs, 22 cities on the new Prussian territory have 231 possible pairs; possible newly included pairs are: $231-55=176$. However, to avoid the inclusion of pairs that were practically irrelevant for trade given the absence of railways before the 1830s, we restrict the analysis to pairs where the distance between two cities is ≤ 200 km (Albers and Pfister, 2021, p. 481).

The control group is formed by city-pairs that were divided by a border both before and after the Wars (362 pairs). The control group also includes 44 pairs of German or foreign cities with other foreign cities, which remained in different territories. These pairs are included in our main analysis (Section 6.1). We discard the 43 German pairs in which both cities were part of the same territory both in 1791 and 1816 from the control group, because these ‘old territory’ pairs could not have been subject to treatment. Nevertheless, we analyze these pairs in a separate specification that focuses on the case ‘no territorial consolidation but trade reform’ further below (Section 6.2).³

3.2. Accession to full sovereignty, the increase of legal capacity, and trade reform: overview

In the Holy Roman Empire, member states constituted semi-autonomous entities with the status of estates at the Imperial Diet. The Habsburg monarch and two central courts (the Imperial Chamber Court in Wetzlar and the Aulic Council in Vienna) were overarching institutions that effectively limited the sovereignty of estates and, hence, the legal capacity of individual states (Amend-Traut, 2009). As the discussion of trade regulation in eighteenth-century Bavaria below shows, this state of fragmented sovereignty undermined attempts to introduce a uniform regulation of markets by the emerging states.

With the dissolution of the Empire in 1806, estates that had survived the first wave of territorial consolidation in connection with the so-called *Reichsdeputationshauptschluss* in 1803, acceded to the status of sovereign states (Wehler, 1987, part 2; Fehrenbach, 2001). Several former duchies became kingdoms, and supreme courts were now located at the state level (Oestmann, 2018).

The increase in legal capacity resulting from the accession to full sovereignty created room for institutional reforms providing for uniform market regulations within the new borders. The large potential for trade reform is testified by the fact that until 1806 trade faced a plethora of internal tolls and levies in Germany. Tariff regulation was heterogeneous, and in some territories, customs were blended with the excise, that is, indirect taxes on consumer goods levied at town gates. Germany had about 1800 tariff schemes in 1790, and along major rivers the average distance between two toll houses was roughly 10 to 20 km (Berding, 1980, pp. 524–26; Henderson, 1968, pp. 21–22; Stolz, 1954, pp. 26, 33–35). Immediately after acceding to sovereign status, most middle-sized German territories that were members of the French-sponsored Confederation of the Rhine enacted laws instituting unified systems of external tariffs: the Grand-Duchy of Berg in 1806/08, Bavaria in 1807, Württemberg in 1808, the Kingdom of *Westphalen* in 1811 and Baden in 1812. Compared with earlier schemes of internal customs, the new regimes were uniform in the sense that they were rule-based, introduced a centralised customs administration, and abolished individual and corporate privileges and exemptions

³ Among the foreign cities two polities with multiple cities in our dataset have ‘newly included’ cases: the Habsburg lands (Salzburg) and the Netherlands (Liège); the few corresponding ‘newly included’ pairs are dropped given the focus on the German lands. The one foreign ‘old territory’ pair, Groningen-Nijmegen, is only included in Section 6.2.

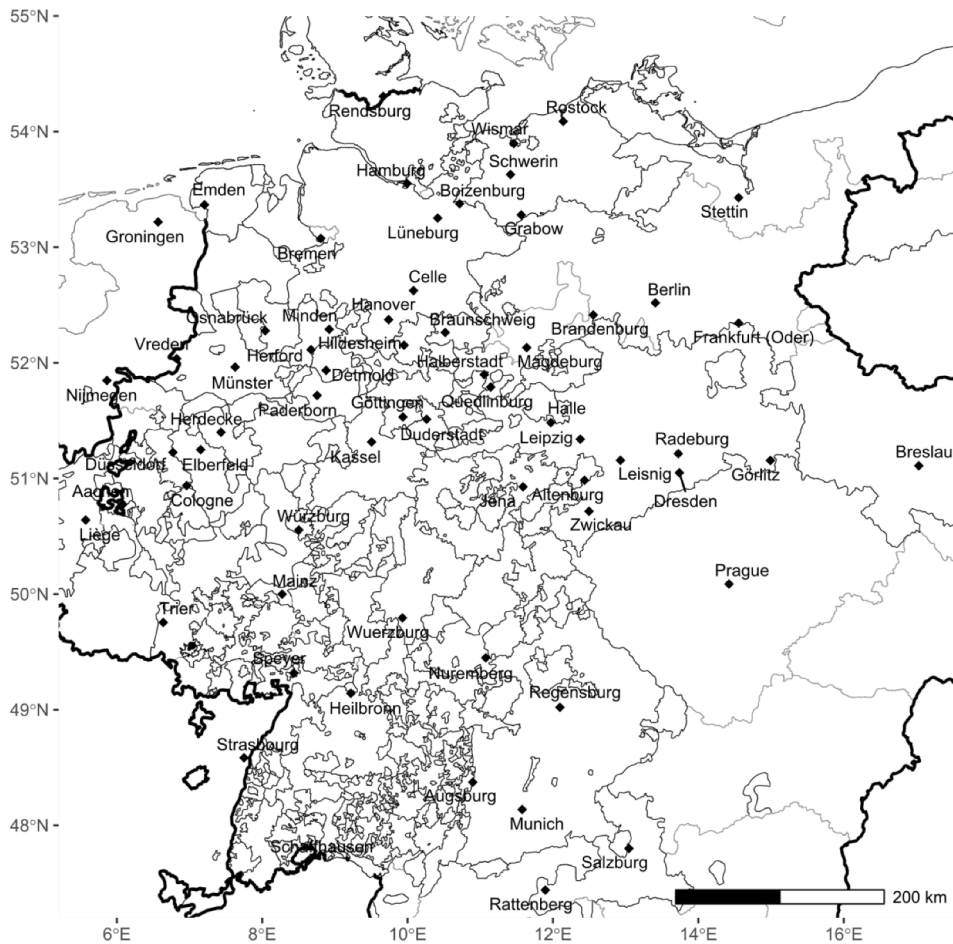


Fig. 1. Grain markets in Germany (borders of 1792)

Note: The map shows the grain markets in our dataset within the borders of 1792. Bold black line refers to border of the Holy Roman Empire.

Source: Authors' own representation based on the maps by [Kunz \(2014\)](#) and the map of Europe from www.natureearthdata.com. Further details documented in SA1.

([Berding, 1980](#), pp. 530–33). Prussia joined this group of early reformers with a comprehensive tax and tariff reform in 1818/19. [Section 3.3](#) provides more details about the evolution of trade policy in the two states among this group for which we can study grain price gaps, Bavaria and Prussia.

Other states hesitated to exploit the potential for the creation of unified market institutions created by the accession of sovereign status in 1806. The most important members of this group are Hanover, Mecklenburg and Saxony. [Section 3.4](#) provides more details on the trade policies of this group.

3.3. The early reformers: Bavaria and Prussia

Already during the second half of the eighteenth century, Bavaria attempted to reform excise and tariff regimes with a view to unifying its territory economically and bringing political frontiers in line with tariff borders ([Stolz, 1954](#), pp. 29–30, 35–36). In 1765, it introduced a unified system of external tariffs and concentrated the collection of the excise in major towns. The impact of these reforms, however, was limited: the initial reform related only to the old part of the kingdom in the south and did not apply to the recently acquired parts in the north, where the power of the ruler was limited. There also existed numerous privileges for estates located outside Bavaria: Some estates were entitled to levy tolls, others were exempted from customs. Attempts at introducing a unified tariff regime thus resulted in numerous conflicts that culminated in lawsuits in Imperial courts. Limited sovereignty restricted the scope for policies designed to create a unified institutional framework for internal markets. While some of the holes in the new system could be patched towards the end of the eighteenth century, it was only the dissolution of the Empire in 1806 and the accession to full sovereignty that made it possible to complete the transformation of the tariff regime ([Häberle, 1974](#); [Berding, 1980](#), p. 526). In 1807, Bavaria carried

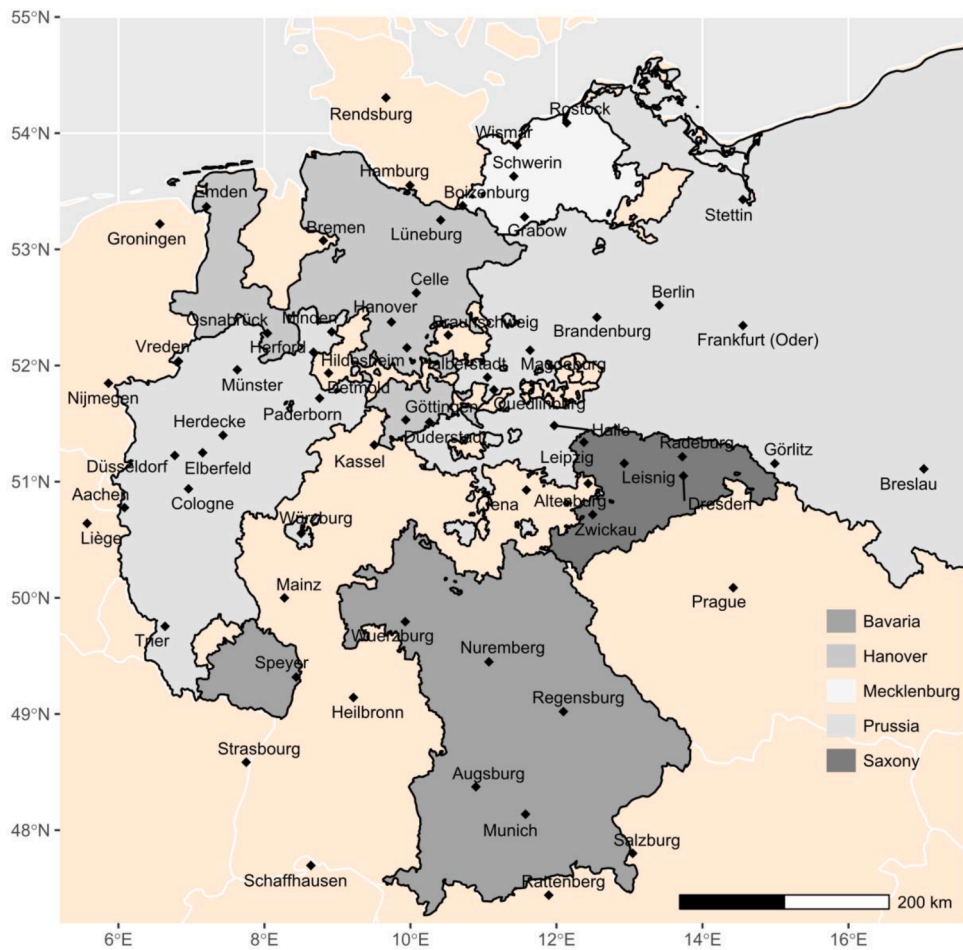


Fig. 2. Grain markets in Germany (borders of 1815)

Note: The map shows the grain markets in our dataset within the borders of 1815. Shaded areas refer to German states for which there are grain price data for at least two cities.

Source: Authors' own representation based on the map for the German Confederation from [MPIDR \(2019\)](#) and the map of Europe from [www.naturalearthdata.com](#). Further details documented in SA1.

out a trade reform that abolished all internal tolls and introduced a system of unified external tariffs ([Berding, 1980](#), p. 530). Together with massive territorial expansion, this created a relatively large internal market.

The Kingdom of Prussia pursued a similar policy in some of its territories from the late eighteenth century. In the Duchy of Mark in Westphalia, for example, public authorities strongly reduced the excise in 1791 and abolished internal tariffs in 1796. Still, the different parts of the kingdom remained under separate tariff regimes; east of the Elbe River there existed 57 different tariff schemes at the beginning of the nineteenth century ([Gorissen, 2002](#), p. 106; [Jacobs and Richter, 1935](#), p. 50; [Ohnishi, 1973](#), pp. 11–16).

Prussia enacted a fully-fledged trade reform only after the Wars in the framework of a comprehensive program aiming at fiscal rationalization and consolidation. The tariff law enacted in 1818 stipulated separate tariff rates for the western and eastern provinces, but nevertheless abolished all internal tolls, thus creating a large internal market ([Ohnishi, 1973](#), pp. 30–31, 44–45; [Siebert, 2001](#), pp. 106–15; [Spoerer, 2004](#), pp. 47–51; see also [Shiue, 2005](#), p. 135 and [Tilly and Kopsidis, 2020](#), pp. 89–90). However, in the Rhine Province, which consisted mostly of newly-acquired territories, the institutional unification of markets remained incomplete because transport on the Rhine river continued to be organized by corporations; see [Section 3.5](#) for details.

3.4. The late-reformers: Hanover, Mecklenburg, and Saxony

During the war period, the territory of the Kingdom of Hanover had been part of France and the Kingdom of *Westphalen* for several years. Trade reforms conducted during this period were not sustained after 1815. Internal tolls were reintroduced, which also applied to newly acquired territories, such as Hildesheim in 1820. Only in 1825 did Hanover abolish internal customs duties ([Oberschelp, 1982](#), vol. 2, pp. 154–55).

Mecklenburg was little affected by the Napoleonic Wars and was definitely a late reformer: A fiscal reform that abolished the excise

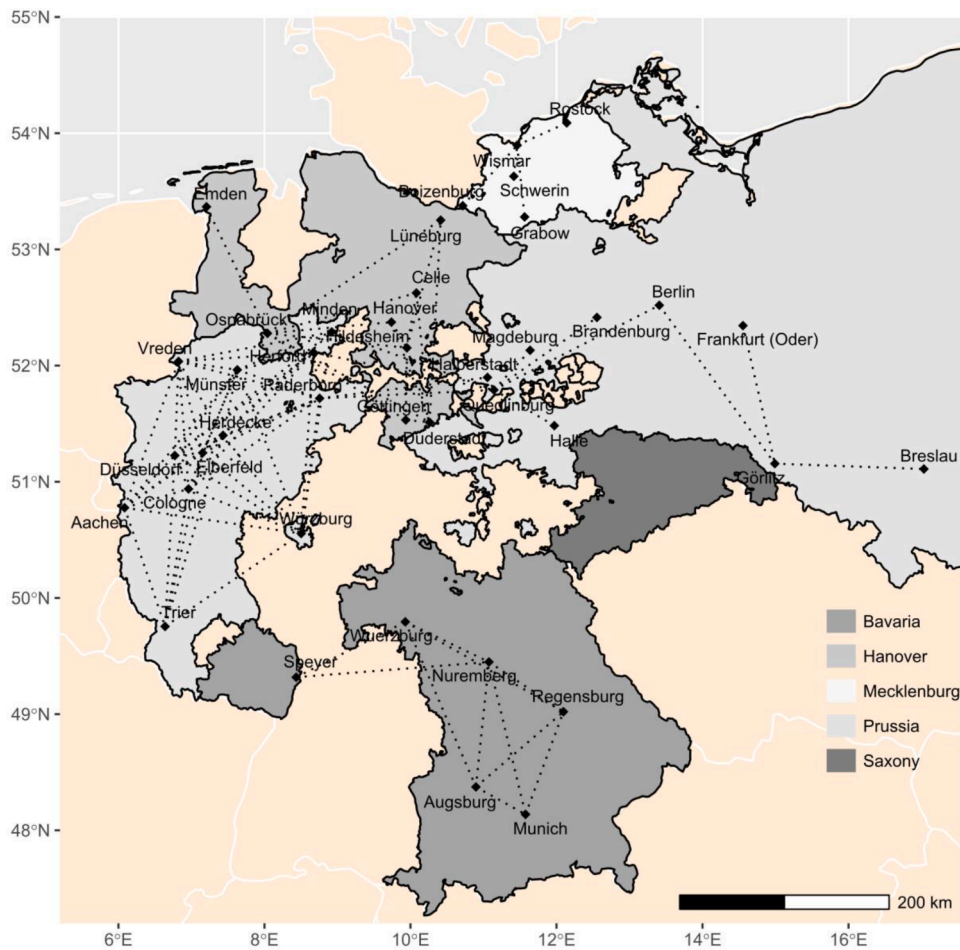


Fig. 3. City pairs experiencing a removal of borders

Note: The map shows the grain markets in our dataset that experienced a removal of borders between them in 1815 compared to 1792. Shaded areas refer to German states for which there are grain price data for at least two cities.

Source: Authors' own representation based on the map for the German Confederation from [MPIDR \(2019\)](#) and the map of Europe from [www.naturalearthdata.com](#). Further details documented in SA1.

and internal tariffs in favor of a corporate tax and external tariffs was enacted only in 1863 ([Vitense, 1920](#), pp. 479–80).

In Saxony, tolls and levies on inland trade remained in force until after the end of the Napoleonic Wars. In 1824 the government enacted a reform of the excise, which remained far short of an abolition of tolls and levies on internal trade. Only the accession to the *Zollverein* (1834) brought a full liberalization of domestic trade ([Kiesewetter, 2007](#), pp. 119–40).

3.5. The Rhine

The Rhine was and still is an important waterway for the transportation of bulky goods ([Wolf 2009](#), pp. 852, 856), and location on the Rhine was a predictor of price convergence in pre-industrial Germany until 1790 ([Albers and Pfister, 2021](#), pp. 18–19). Presumably, this was a result of the technological advantage of moving bulky goods on waterways. However, prior to the Napoleonic Wars, the high number of customs stations, legislation forcing trade into privileged markets by way of staple rights and the compulsory transfer of freight from one corporation of boatmen to the next in Cologne and Mainz contributed to high regulatory trade costs on the Rhine river. A sizeable portion of north-south trade was thus displaced eastward to the Elbe river and the overland route from Magdeburg to Nuremberg and farther south to Augsburg ([Newman, 1985](#), pp. 74–75; [Spaulding, 2011](#), pp. 209–13). As a result of pressure by France, the *Reichsdeputationshauptschluss* of 1803 abolished tolls on river trade, but the corporate organization of river transport remained in place and was incorporated into an international regime governing trade on the Rhine river with the so-called *Octroy* concluded between France and the Holy Roman Empire in 1804 ([Thiemeyer and Tölle, 2011](#)). The Central Commission for Navigation on Rhine, founded in 1815, continued the international organization of traffic on the Rhine, but did not implement any new reforms in the first years of its existence. The corporate organization of river transport and several river tolls remained in place until the

signing of the Convention of Mainz in 1831, which effectively internationalized and liberalized transport on the Rhine river (Borgius, 1899, pp. 102–03; Reinhardt, 1969, pp. 322–33, 388–92). Thus, border changes and reforms undertaken by individual states during the Revolutionary and Napoleonic Wars and its immediate aftermath could exert little effect on trade on the Rhine river.

4. Theoretical background

The following analysis aims at measuring the impact of territorial consolidation and trade reform on trade costs. To this end, we adopt the well-established methodology of quantifying market integration by studying bilateral price gaps.

Price gaps indicate trade costs between two markets and capture the extent to which the law of one price holds (Federico, 2012, p. 474; 2018, pp. 3–4, 12). This methodological approach is well established in a variety of empirical settings: development economics (Aker, 2010), economic geography (Schulze and Wolf, 2009), and economic history (Keller and Shiue, 2014).

To calculate price gaps, we follow other studies by relying on grain price data, because grain can be considered as a homogenous good. Furthermore, in the period under study, statistical offices did not exist so that data on trade flows are both fragmentary and unreliable, caveats which do not apply to grain prices.

One of the most important determinants of price gaps is the bilateral distance, which we include in all models. Effects of distance mirror transport costs, but trade costs can also arise from policy barriers such as tariffs (Federico, 2018, pp. 12, 16; Schulze and Wolf, 2009, pp. 124–25). Tariffs are the most important element of trade costs that we analyze in this study. The existing literature on international trade usually considers tariffs as an element of a border effect on trade costs. Hence, the removal of borders, as it took place in Germany at the beginning of the nineteenth century, should lower trade costs. Such a reasoning equates state boundaries with tariff zones and implicitly assumes there are no internal tariffs.

However, our discussion of trade reforms in Section 3 has established that until about 1800, taxing trade worked differently and was more akin to toll collection along roads, rivers or at town gates rather than at the border of a political territory. We refer to these forms of taxing trade as internal tariffs. As shown above, a key innovation of trade policy in Germany during and after the Wars was the regime shift from internal to external tariffs, the ‘modern’ way of taxing trade. Given this historical background, we expect no reduction of trade costs through territorial consolidation (removal of borders) alone. Rather, for the removal of borders to have an effect on trade costs, the newly sovereign states needed to promote trade actively through trade reforms that abolished internal tariffs. ‘Hypothesis 1’ that we test below in Section 6.1 thus states that territorial consolidation reduced trade costs between newly-included cities conditional on whether a state undertook trade reforms that removed internal tariffs in favor of a system of unified external tariffs.

This argument implicitly assumes that trade reform was the main force driving the effect of the removal of a border on trade costs. To explore whether this holds true, we test a second hypothesis in an alternative setup in Section 6.2 that exploits variation between pairs of cities that were in the same territory both before and after the Wars. ‘Hypothesis 2’ states that trade costs between cities that were part of the same territory both before and after the Wars declined if the political authorities of the respective territory undertook trade reforms that removed internal tariffs in favor of a system of unified external tariffs. The decline should be of similar magnitude as the effect of territorial consolidation in reformer states posited by the first hypothesis above.

Tariffs are not the only policy barrier that raises the costs of trade across state boundaries. Relevant aspects of the administrative capacity of states include market regulation in the form of transaction standards such as standards concerning weights, volumes and the currency system, as well as a legal apparatus for resolving disputes over property rights (Section 2 above). Territory-specific transaction standards and legal systems lower the transaction costs of domestic trade relative to cross-border trade. Several German states made considerable headway in unifying transaction standards and rationalizing their legal systems in the late eighteenth and early nineteenth centuries. Nevertheless, hypotheses 1 and 2 imply the argument that tariff regulation largely dominated over transaction costs in the border effect.

As controls, we consider several other forces that potentially impacted on trade costs. It is well-known that common cultural factors such as language or religion may facilitate trade. While language is an important predictor of trade costs in multi-language states such as the Habsburg Empire (Schulze and Wolf, 2009) or India (Fenske and Kala, 2021), this was not an issue in Germany; however, religious denomination might have mattered through networks among members of the same religious group and is thus considered in our analysis.

We also conjecture that transport costs were lower within river networks than for overland routes (e.g., Wolf et al., 2011). Specifically, we test for the effects of four major river systems: Elbe, Oder, Rhine, and Weser. In addition to rivers, following Wahl (2016, p. 54), we include the routes of the two most important Imperial roads as an indicator of pre-existing corridors of overland trade. With regard to institutional factors, we consider the variable ‘French presence’ which is associated with various French-inspired reforms, the channel investigated by Keller and Shiue (2020).

Trade costs can also depend on market size and we consider changes in market size as proxied by population growth, which proved statistically significant in the analysis by Albers and Pfister (2021). Finally, variation in annual grain prices might be influenced by weather shocks to local output, and hence to local prices, which form the bilateral price gap. Hence, we include bilateral differences with respect to seasonal temperature and precipitation as control variables.

5. Empirical strategy

5.1. Data sources

Data sources are documented in detail in the supplementary appendix (SA, including tables S1 and S2 showing the coding of variables at the city level in SA1.2 and SA1.3). The SA also contains a table for summary statistics of all variables during the pre-treatment period (Table S3 in SA1.4). In what follows, we briefly describe the sampling for the outcome variable, the two explanatory variables and the main controls.

We significantly extend an existing dataset on the outcome variable, bilateral price gaps. Data on rye prices for 30 cities are from [Albers and Pfister \(2021\)](#); the present study adds 22 new cities and extends four series so that in total, rye price data are available for 56 German cities for most of the years 1780–1830; inclusion of eight foreign cities mainly builds on previous research (details in SA1.1). Sampling is constrained by data availability; in particular, data for southwestern Germany are very scarce. Converting information on grain prices given by contemporaries to a consistent dataset is faced with several challenges such as the standardization of data given a multitude of local currencies and units of volume. We converted prices to grams of silver per liter as is standard in the literature on pre-industrial market integration (e.g., [Federico et al., 2021](#)).

The territorial changes that occurred as a result of the Wars are coded using a relevant historical lexicon, additional documents, and historical maps (details in SA1.2 and SA1.3). The resulting variable ‘newly included’ is set to 1 if a town pair became part of the same territory after 1815 but was not in the same territory in 1791. The value of ‘newly included’ is set to 0 if the two cities of the respective pair belong to two different polities both before and after the Wars. These city-pairs form the control group in our test of the effect of territorial consolidation and trade reform (hypothesis 1). To test the effect of trade reform independent of territorial consolidation (hypothesis 2), we additionally include pairs where both cities belonged to the same territory in 1791 and after 1815 (‘old territories’) as a third group, which is coded as a separate dummy variable.

The reform indicator is constructed based on the historical evidence presented in [Sections 3.2–3.5](#). We consider city pairs to have been affected by trade reform, if they were located in a state that replaced internal tolls by a unified system of external tariffs before 1820—Bavaria or Prussia—and were not situated on the lower Rhine where the corporate organization of river transport was still in effect. Thus, for these city pairs the trade reform indicator variable is set to 1.

What follows describes the control variables. The main specifications of the river variables are dummy variables that are coded 1 for a city pair if either city is part of one of the major river systems Elbe, Oder, Rhine, and Weser ([Fig. 4](#)). Given the complexity of the Rhine river system, we also consider an alternative coding, in which we split the river system into its parts: city pairs on the Rhine river, those on its tributaries, and those pairs in which one city is located on the Rhine, and one city on one of the tributaries, which captures the between variance.⁴

The dummy variable for Imperial roads takes the value of 1, if the distance of either city in the pair to an Imperial road is ≤ 50 km. We consider two Imperial roads, the *Via Regis* and the *Via Imperii* (see also red lines in [Fig. 4](#)).

We constructed the variable ‘French presence’ (or ‘French rule’) at the city level to control for the effect of institutional reforms implemented under French influence on trade costs. We develop two versions of measuring French presence: The first requires that only one city in a pair was French-ruled, the second is limited to city-pairs where both cities were French-ruled. Both versions are considered as duration in years and as dummy variables (details in SA1.3).

We expect that networks among adherents of the same religious denomination facilitate trade and therefore include a group of control variables capturing whether both cities adhered to Catholicism, Protestantism or whether both had an arrangement that guaranteed the coexistence of the two denominations (mixed confession). Thus, the baseline category are city pairs with different denominations and cases where one city is of mixed confession and the other one is Catholic or Protestant.

Distance is calculated as great circle distance based on the coordinates for each city. Population data for German cities is from a study that constructs annual series for pre-industrial Germany (SA1.3).

Data on seasonal temperature and precipitation for the weather shock proxy variables are gridded data developed by climatologists.

5.2. Regression models

Our empirical strategy is based on the idea that the Wars provide exogenous variation in the assignment of a city to a particular polity ([Acemoglu et al., 2011](#); [Huning and Wolf, 2019](#)). The rationale underlying this assumption is that changes in territorial boundaries resulting from the peace settlements of the Napoleonic era primarily reflected strategic concerns of external powers such as France, Austria and Britain, and the relationships of German states with these powers, rather than effects of pre-existing economic geography on military campaigns. The price gap between two cities in a given year is evaluated with respect to whether this city-pair became part of a new territory as a result of the Wars (the ‘treatment’), the implementation of a trade reform in this territory, and a set

⁴ While the literature has used dummy variables to proxy lower trade costs on rivers (e.g., [Keller and Shiue, 2014](#), p. 1184), ideally, we would have data to calculate least-cost-paths to approximate trade costs between two locations. However, the required data (road and river networks, costs for transport technologies) are not available, specifically with regard to historical road connections. The main result is also robust to a more flexible specification with city-pair fixed effects (and time effects), which allows for varying transport costs across pairs and common annual shocks to trade costs across pairs (SA2, Table S8).

The dummy variable b_{ij} is the treatment group indicator ‘newly included’. Alternatively, one could think of variable b as a vanishing border. The dummy variable p_t denotes the post-treatment period, that is, the time after the end of the Wars 1816–30. Based on standard economic theory outlined above, one would expect the parameter β_1 of the interaction effect $p_t \cdot b_{ij}$ to be negative, signaling decreasing price gaps and increasing market integration in the cases in which a border had been removed between the two cities of a pair during the war period. But we also argued that during the pre-reform period, trade was not taxed at the border of political territories and that transaction costs differed little between domestic and cross-border trade; hence, we expect only a small or zero effect for β_1 .

We implement the conditionality of the territorial consolidation effect on the presence of trade reforms by including the dummy variables ref_{ij} (the group indicator for trade reform) and $pref_t$ (the post-reform period indicator). 40 out of 96 newly included pairs are ‘reformer’ pairs; these reformer pairs are constructed from 22 cities. The additional period indicator $pref_t$ is necessary because dates for the implementation of trade reforms differ by territory. Variable $pref_t$ denotes the period 1816–30 for Bavarian cities, and 1819–30 for Prussian cities as the Prussian customs law was enacted only in 1818. The conditionality of the trade cost reducing effect of border removal on trade reform should be reflected in a statistically significant regression coefficient β_2 of the interaction term $b_{ij} \cdot ref_{ij} \cdot pref_t$.

In addition, specification (1) includes distance d_{ij} , a set of city dummy variables c_i , because trade might be path-dependent (see, e.g., Wahl, 2016), and time dummy variables s_t to account for shocks common to all cities. Further variations of specification (1) also include several control variables x_{ijt} that we introduced above: access to major rivers, French presence, being close to Imperial roads, religious denomination, population growth, and the bilateral temperature gap. Control variables which are constant such as access to rivers become time variant through an additional interaction with the post-war dummy: $x_{ijt} = x_{ij} \cdot p_t$. To shed further light on the heterogeneity of the consolidation effect, we address the question which territories exhibit decreasing price gaps by adding territory-specific dummy-variables to the \mathbf{x} matrix.

To test hypothesis 2 concerning the effect of trade reform on trade costs between city pairs in which both cities belonged to the same territory before and after the Wars, we augment specification (1) with a dummy variable o_{ij} for the ‘old territories’ of Hanover, Prussia, Mecklenburg and Saxony (Eq. (2)).⁶ For this purpose, the sample must be expanded to include pairs of cities that belonged to the same territory both in 1791 and from 1815 onwards. The location of these city pairs is shown in Fig. 5. They are excluded from the estimation of Eq. (1), because they could not have been subject to the ‘treatment’ of a border change (cf. Section 3.1).

$$y_{ijt} = \alpha_0 + \beta_0 b_{ij} + \beta_1 b_{ij} \cdot p_t + \beta_2 b_{ij} \cdot ref_{ij} \cdot pref_t + \gamma_0 ref_{ij} + \gamma_1 \log(d_{ij}) + \mathbf{x}_{ijt} \boldsymbol{\delta} + \sum_{i=1}^{I=64} \alpha_i c_i + \sum_{t=1}^{T=27} \eta_t s_t + \varphi_0 o_{ij} + \varphi_1 o_{ij} \cdot D_t + \varphi_2 o_{ij} \cdot pref_t + \varphi_3 o_{ij} \cdot ref_{ij} \cdot pref_t + u_{ijt}. \quad (2)$$

The additional dummy variable for the old territories o_{ij} in Eq. (2) takes the value of 1 when both cities in a pair had been part of a territory already in 1791. The coefficient φ_0 captures a potential border effect independent of tariff regulation. For instance, if transaction costs of domestic trade were lower compared to cross-border trade, φ_0 should be negative. Like in Eq. (1) we have early reformers (dummy variables ref_{ij} and $pref_t$) and late reformers. Old states that did not implement trade reforms during the war period in the immediate aftermath of the Wars include Hanover, Mecklenburg and Saxony. The old Prussian territories, however, witnessed reforms, specifically the Prussian Customs Law of 1818. Variable D_t is a dummy for the years 1816–18 and thus, φ_1 captures the impact of the short period where the Prussian Customs Law was not in place and which coincides with the Tambora crisis of 1816–18. Coefficient φ_2 measures the effect of the post-reform period on all old territories ($pref_t$ equals 1 for 1819–30). Parameter φ_3 quantifies the impact of the Prussian Customs Law of 1818 on the ‘old’ Prussian territory (21 city pairs) through the additional interaction with the ‘Reform’ indicator ref_{ij} . We expect φ_3 to be negative and of the same magnitude as β_2 in Eq. (1). In an additional flexible specification, we include state-specific dummy variables for the old territories including time interactions rather than pooling all old states in one group.

Finally, all these specifications exclude the war period, first, because we are interested in the net effect of the boundary changes that occurred during the Wars and second, because data quality during the war period may be weak, the main reasons being monetary disturbances due to war finance, and war-related dislocation/destruction. In addition, some cities have missing data during the war period.

6. The effects of territorial consolidation and tariff reforms on market integration

We start with a presentation of our results concerning the effect of territorial consolidation conditional on trade reform (hypothesis 1, Section 6.1) and continue with an analysis of the effects of the transition from a regime resting on internal levies and tolls to a unified system of external tariffs in cases where borders remained stable (hypothesis 2, Section 6.2).

6.1. Price gaps of newly included pairs in reformer states decreased

The regression estimates in Table 1, which implement Eq. (1), show two important results. First, column 1 shows that the price gap between two cities did not fall when the border between them was removed: the effect ‘Newly included x 1816–30’ is practically zero

⁶ Note that Bavaria does not feature in these models with an ‘old territory’, because five of the six cities available in our sample were Bavarian in 1816 but were not part of Bavaria in 1791, implying that our sample does not include city-pairs on the old Bavarian territory.

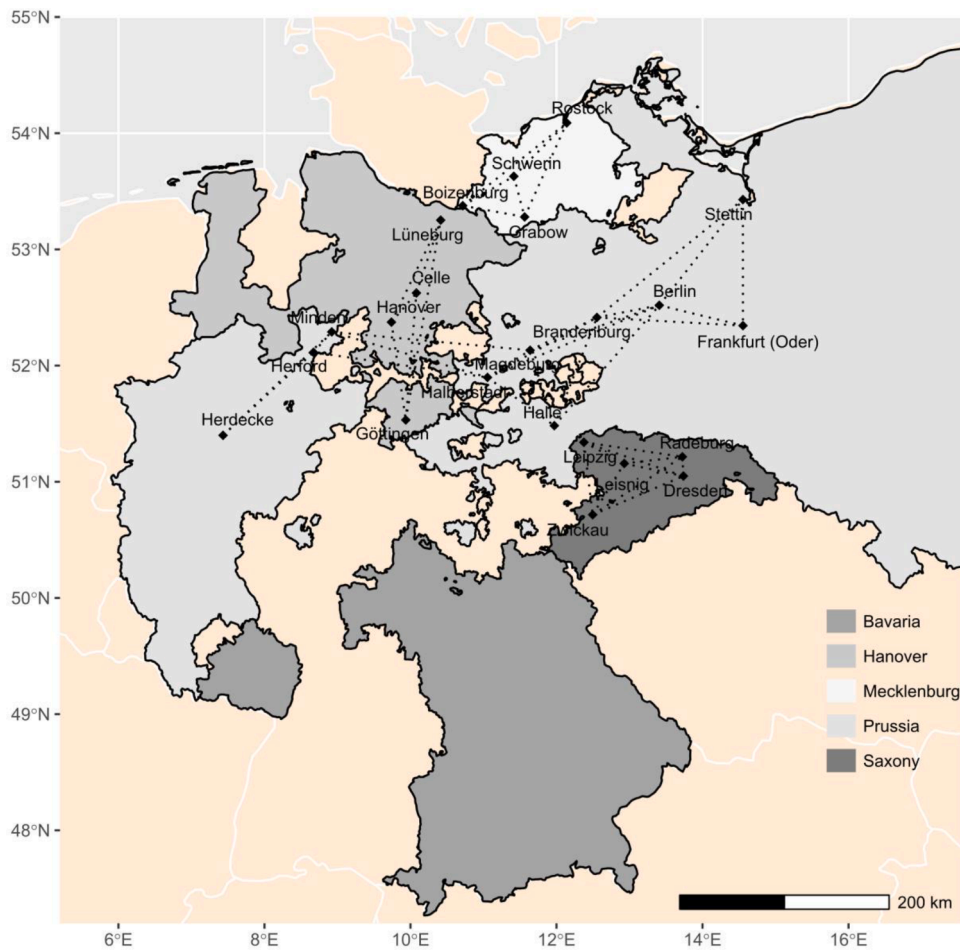


Fig. 5. Old territories: pairs of cities that were in same territory in 1791 and 1816

Note: Dotted lines show pairs where both cities belonged to the same territory both in 1791 and 1816. Shaded areas refer to German states for which there are grain price data for at least two cities.

Source: Authors' own representation based on the map for the German Confederation from MPIDR (2019) and the map of Europe from www.naturalearthdata.com. Further details documented in SA1.

(all standard errors made robust to spatial and serial correlation, specifically the SCC error).⁷ This indicates that territorial consolidation alone did not reduce trade costs, which is consistent with hypothesis 1 (see Section 4). Second, column (1) shows that the insignificant result masks considerable regional heterogeneity: price gaps of city-pairs that were newly included into the territory of the same state (that is, borders between them were eliminated) and which were located in a reformer state (but not along the Rhine) had significantly lower price gaps after the Wars compared to the control group: approximately 5 to 6 percentage points (-0.059, effect 'Reform x newly included x 1816–30' in column 1, corresponding to β_2 in Eq. (1)).⁸ This corresponds to a reduction of about 31 percent relative to the average price gaps before the Wars (0.19, refers to newly included city pairs in reformer states). This result confirms that the effect of the removal of borders on trade costs was conditional on trade reform (hypothesis 1). Territorial consolidation as such did

⁷ The SCC standard error by Dricoll and Kraay (1998) implemented in the software R by Millo (2017) can be regarded as an extension of the Newey and West standard error: in addition to serial correlation, it allows for contemporaneous cross-sectional correlation by clustering along the time dimension, and cross-serial correlation (Croissant and Millo 2018, pp. 111, 115–16). To achieve robustness against cross-serial correlation, the lagged covariances of any observational unit (here the pair) are added and weighted using the Bartlett kernel (ibid., pp. 110, 117). The number of lags included is automatically selected and is a function of the available data. The time dimension of the panel should be about 20 periods, independent of the number of cross-sectional units (Croissant and Millo 2018, p. 116), but the estimator performs still respectively with a shorter time dimension (Driscoll and Kraay 1998, pp. 555–56).

⁸ In a specification without the additional interaction with 'Reform', price gaps in newly included territories fell by about 1 percentage point—not significantly different from zero. We also considered a specification of the reform indicator which codes all city pairs located in Prussia and Bavaria with 1 and conditions on the Rhine; the main result is similar (coefficient: -0.046 , $p < 0.01$, SA2, Table S8, column 4) and we opted for the parsimonious specification.

Table 1
Negative effect of territorial consolidation in combination with internal trade reform.

	Dependent variable: price gap		
	(1)	(2)	(3)
Constant	-0.050* (0.030)	-0.053* (0.030)	0.011 (0.019)
Newly included	-0.020*** (0.008)	-0.012* (0.006)	-0.017** (0.008)
Newly included x 1816–30	0.007 (0.008)		
log(dist)	0.043*** (0.004)	0.044*** (0.004)	0.046*** (0.004)
Reform	0.026 (0.018)		
Reform x newly included x 1816/19–30	-0.059*** (0.021)		
Bavaria x 1816–30		-0.055*** (0.018)	-0.062*** (0.018)
Prussia Core x 1819–30		-0.052*** (0.012)	-0.039*** (0.011)
Between Prussia Core and West x 1819–30		-0.060** (0.026)	-0.057** (0.026)
Prussia West of Core x 1819–30		-0.008 (0.012)	-0.024** (0.012)
Hanover x 1816–30		0.006 (0.019)	0.015 (0.021)
Mecklenburg x 1816–30		0.011 (0.027)	0.019 (0.029)
Rhine			-0.056*** (0.012)
Rhine x 1816–30			0.035** (0.014)
City fixed effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Observations	11,410	11,410	11,410
R ²	0.189	0.189	0.190
Adjusted R ²	0.183	0.182	0.183

Note: * $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$. Dependent variable: city-pair price gap: $|\text{ratio of prices} - 1|$. SCC standard errors in ().

not impact on market integration; a state needed to implement reforms that removed internal tariffs to trade in order to lower trade costs in its expanded territory.

Column (2) presents the results disaggregated by territory. The negative effect operates in Bavaria and in those parts of Prussia that were added to the core region situated mostly east of the Elbe river. The Prussia Core effects operate independently of whether both cities of a pair became part of the Prussian Core territory ('Prussia Core') or only one city was situated in 'Prussia Core' while the other was in 'West of Core' ('Between Prussia Core and West').⁹ In all these cases, the effect of territorial consolidation on price gaps is about 5 to 6 percentage points. By contrast, the newly formed western provinces of Prussia, the Rhine Province and Westphalia, which we label 'Prussia West of Core', seem to have experienced no integration of grain markets; but controlling for location on the Rhine, there is a modest integration effect (column 3). The inclusion of towns into the Kingdom of Hanover or Mecklenburg, the late-reformer states in the sample, shows no significant effect on price gaps.

The regressions shown in Tables 2 and 3 test whether the main result is robust to the inclusion of various controls. Table 2 considers the effect of location on a river in general and location on the Rhine in particular.

Column (1) of Table 2 shows a parsimonious specification that includes one common dummy variable for the four major river systems, Elbe, Oder, Rhine, and Weser. The time interaction proves significantly positive. By adding an additional time interaction for the newly included city pairs, regression (2) tests whether the impact of being newly included might have operated along the rivers rather than through location in a reformer state; this is not the case. Column (3) introduces individual effects for each river system. The effect of becoming newly integrated into a territory undergoing trade reform is reduced somewhat but remains statistically significant at the 5 percent level. There is no uniform change of the effect of being located on a river over time: Markets situated on the Rhine and the Elbe disintegrated, those on the Weser integrated, whereas the effect of being located on the Oder does not exhibit any change.

Specification (4) shows that disintegration in the Rhine system stemmed from a disintegration between the main river and its

⁹ We explicitly address 'between' pairs, because the Western part of Prussia was disconnected from the core territory in 1815 (as visible in Figs. 2 and 3 in Section 3.1). This geographical situation later also formed an important motive for Prussia's strategy in the bilateral negotiations of the Zollverein (see Ploeckl, 2021, pp. 314–15). Note also that a few cities in 'West of Core' were already part of Prussia before the Wars, e.g., Herford, and formed 'new territory' pairs with cities that became part of 'Prussia Core', e.g., Quedlinburg.

Table 2
Testing control variables for rivers.

	Dependent variable: price gap				
	(1)	(2)	(3)	(4)	(5)
Newly included	0.007	0.014	-0.00002	0.0001	0.003
x 1816–30	(0.007)	(0.010)	(0.008)	(0.008)	(0.009)
Reform x newly	-0.055***	-0.057***	-0.047**	-0.048**	-0.049**
included x 1816/19–30	(0.020)	(0.021)	(0.021)	(0.021)	(0.022)
River x 1816–30	0.013*	0.015*			
	(0.007)	(0.008)			
River x newly		-0.009			
included x 1816–30		(0.007)			
Rhine x 1816–30			0.033**		
			(0.016)		
Rhine river x 1816–30				0.012	0.011
				(0.017)	(0.017)
Rhine tributary				0.038**	0.012
x 1816–30				(0.019)	(0.017)
Btw Rhine river and				0.066***	0.042**
tributary x 1816–30				(0.017)	(0.019)
Elbe x 1816–30			0.025***	0.025***	0.025***
			(0.009)	(0.009)	(0.009)
Weser x 1816–30			-0.014**	-0.012*	-0.013*
			(0.007)	(0.007)	(0.007)
Oder x 1816–30			0.011	0.011	0.011
			(0.019)	(0.019)	(0.019)
City fixed effects	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes
Group effects and distance included	Yes	Yes	Yes	Yes	Yes
Observations	11,410	11,410	11,410	11,410	10,678
R ²	0.189	0.189	0.192	0.193	0.202
Adjusted R ²	0.182	0.182	0.185	0.186	0.194

Note: * $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$. Dependent variable: city-pair price gap: |ratio of prices - 1|. SCC standard errors in (). Column (5) drops all pairs with Herdecke and Trier.

Table 3
Testing control variables for French Presence, imperial roads, population growth, religion, and weather shocks.

	Dependent variable: price gap				
	(1)	(2)	(3)	(4)	(5)
Newly included	0.019***	0.009	-0.007	0.002	0.007
x 1816–30	(0.007)	(0.009)	(0.008)	(0.009)	(0.008)
Reform x newly included	-0.064***	-0.056**	-0.051**	-0.056***	-0.059***
x 1816/19–30	(0.022)	(0.024)	(0.022)	(0.022)	(0.021)
French Presence dummy	-0.034***				
x 1816–30	(0.011)				
Imperial road x 1816–30		0.003			
		(0.012)			
Imperial road x newly		-0.012			
included x 1816–30		(0.017)			
Lag pop. growth			0.022		
			(0.019)		
Protestant x 1816–30				-0.006	
				(0.013)	
Catholic x 1816–30				0.012	
				(0.008)	
Mixed confession				-0.008	
x 1816–30				(0.025)	
Summer temp. gap					-0.013**
					(0.006)
City fixed effects	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes
Group effects and distance included	Yes	Yes	Yes	Yes	Yes
Observations	11,410	11,410	7,553	11,410	11,410
R ²	0.193	0.190	0.185	0.190	0.190
Adjusted R ²	0.186	0.183	0.177	0.183	0.184

Note: * $p < 0.1$. * $p < 0.05$. * $p < 0.01$. Dependent variable: city-pair price gap: |ratio of prices - 1|. SCC standard errors in ().

tributaries: ‘Rhine tributary x 1816–30’ and ‘Btw Rhine river and tributary x 1816–30’ are both significantly positive. While trade on the Rhine as such was unaffected, increasing price gaps with tributaries indicate frictions in the river system as a whole, likely as a result of the persistence of the corporate organization of river transport in the lower Rhine area. A part of the result on the disintegration within the Rhine system should be interpreted with caution, however. Column (5) demonstrates that some coefficient estimates may be driven by the idiosyncratic behavior of price series of individual towns: when dropping two cities, namely Herdecke (connected to the Rhine via the Ruhr) and Trier (connected via the Moselle), the term ‘Rhine tributary x 1816–30’ becomes insignificant and the parameter for the between variance is reduced, although it remains still significant.

Among the control variables considered in Table 3, two are of special relevance, namely, French presence and location in a corridor of overland trade. Against the background of existing scholarship, we test whether the reduction of price gaps among city pairs newly included into states carrying out trade reforms was independent of the effects of institutional reforms implemented in territories under French control during the war period.

The regression in column (1) of Table 3 revisits the argument of Keller and Shiue (2020) that French rule contributed to market integration via institutional reforms. In contrast to their study, which relates exclusively to the post-1815 period, we can observe changes of market integration between the 1780s and the period 1816–30 and test whether they were related to the spatial pattern of French presence during the war period. The effect turns out being unstable. In Table 3 we only report the specification with the strongest model fit (Adj. R^2) among the different versions considered (both cities French ruled, dummy variable); the presentation of further specifications is relegated to the SA (see Table S4 in SA2).¹⁰

While French presence may have contributed to market integration, its inclusion into the analysis does not significantly alter the magnitude of the net effect of territorial consolidation in reform states. Note that the inclusion of French Presence leads to a positive effect for the term ‘Newly included x 1816–30’, however, at the same time ‘Reform x newly included x 1816/19–30’ gains in magnitude, so that the net effect is still significantly negative. Territorial consolidation in combination with trade reforms and the creation of inclusive markets under French rule were obviously separate and only partly overlapping mechanisms that contributed independently to market integration.

The majority of the German states used their newly developed administrative infrastructure to embark on programmes of infrastructure development in the form of the development of networks of paved roads, which were virtually non-existent in 1815. From the late 1810s to 1850, road construction constituted the single most important type of public expenditure that was not related to core functions of state maintenance (Borchard, 1968, pp. 200–05, 214–15, 260–78; Tilly and Kopsidis, 2020, p. 97). Thus, infrastructure development, rather than a change in market regulation, may have contributed to a decline of trade costs in reform states. In column (2) of Table 3, we test whether location close to an Imperial road is connected with a decrease of price gaps over time. This rationalizes the idea that road improvement started in pre-existing corridors of transport on trade. We find that location close to an Imperial road creates a static advantage in trade costs (about 3 percentage points, $p < 0.01$, not shown), but the effect does not increase over time. This is consistent with the regional case study by Uebele and Gallardo-Albarrán (2015), who show that the existence of a connection of two markets with a paved road reduced bilateral price gaps only from the 1830s onwards. Apparently, investment in the improvement of overland transport infrastructure took a long time to pay off.

Columns (3) to (5) in Table 3 examine further control variables. To consider the effect of changes in market size, regression (3) controls for city-pair-specific lagged population growth (the first lag of the difference of the logged product of population in cities i and j).¹¹ Column (4) tests the effect of common religious denomination on trade costs. With respect to weather shocks, we tested for an impact of temperature and precipitation gaps in all four seasons (eight variables in total), but show only the regression with the variable having a statistically significant effect, namely, the gap in summer temperature (column 5). Among the control variables considered in columns (3) to (5) of Table 3, only one shows a statistically significant regression coefficient, and the effect of being included into a territory undergoing trade reform during the war period remains stable.¹²

Overall, our results show that territorial consolidation decreased trade costs conditional on trade reforms. The removal of borders through territorial consolidation alone did not reduce price gaps. This is in line with the historical fact that trade was not taxed at the border before the reforms and with only small cultural differences between cities that could explain border effects. The now sovereign states had to use the newly acquired legal capacity to liberalize domestic trade in order to promote market integration. The result is robust to numerous controls capturing potentially relevant alternative drivers of market integration.

6.2. Price gaps between cities that belonged to the same territory both in 1791 and 1816 and experienced trade reform decreased

Up to this point, we have identified the effect of reforms that liberalized domestic trade and created a unified system of external tariffs on bilateral price gaps using the contrast between city pairs that became integrated into the same territory during the war period and pairs of cities that continued to be separated by a border. To strengthen the point that the observed reductions in trade costs were indeed due to changes in tariff regulation (hypothesis 2 in Section 4), we now explore the effect of a trade reform on trade costs among city pairs that belonged to the same territory both in 1791 and 1816.

Table 4 shows the regression estimates for Eq. (2) (most constant group effects not shown due to space limitations). According to

¹⁰ Note that we also tested the specification chosen by Keller and Shiue (at least one city French-ruled, without Hanseatic cities; cf. Keller and Shiue, 2020, p. 281). However, it proved insignificant in our test design.

¹¹ We considered to include population size as such but failed to reject unit root and therefore decided to include log-differences.

¹² The results is also robust to inclusion of multiple controls and city-pair fixed effects, see Table S8 in SA2. Coefficient size is: -0.055 , $p < 0.01$.

column (1), city pairs belonging to the same territory both before and after the Wars had significantly lower price gaps (-2.4 percentage points, corresponding to φ_0 in Eq. (2)) than the control group. This indicates that there was a small static advantage of location in the same territory independent of tariff regulation compared to city pairs that were not newly included and were not situated in the same territory (the control group). This points to the possibility that territory-specific transaction standards and legal systems lowered the transaction costs of domestic trade relative to cross-border trade.

However, the advantage of being located in the same territory was not stable over time as the time interactions show. While no changes in market integration appear in old territories in 1816–18, that is, prior to the implementation of the Prussian tariff law (see the interaction term ‘Old territory x 1816–18’, φ_1 in Eq. (2)), city pairs in old territories exhibit disintegration in 1819–30 (‘Old territory x 1819–30’, 0.036, corresponding to φ_2). Nevertheless, the sub-group that was subject to trade reforms experienced a decline of price gaps (‘Old territory x reform x 1819–30’, -0.102, φ_3). The net effect of both interactions is negative with an economically relevant magnitude of about -6.6 percentage points (3.6–10.2). The size of this reform effect is much larger than the static effect of location in the same territory (-2.4 percentage points, φ_0). Trade reform tripled the magnitude of the border effect prevailing during the pre-reform era, implying that costs of cross-border trade relating to tariff regulation largely dominated over transaction costs.

The increase of price gaps between cities that belonged to the same territory both before and after the Wars and did not experience trade reform implies that the static advantage of location in the same territory became approximately nullified after the Wars (-2.4 + 0.5 + 3.6 = 1.7 percentage points). A corresponding F test cannot reject the hypothesis that the effect is significantly different from zero ($p = 0.17$, p -values based on robust variance-covariance matrix). This disintegration in late-reformer states suggests the possibility of trade diverting effects of trade reform in combination with territorial consolidation. In particular, the development of a large internal market in Prussia may have redirected trade that originally involved only cities in neighboring late-reformer states such as Hanover towards the Prussian market.

The more flexible specification in column (2) allows for state-specific dummy variables. The static effects are negative and significant for Prussia and Hanover, negative and not significant for Saxony, and positive and mostly insignificant in Mecklenburg (not shown). We find that the interaction ‘Prussia old x reform x 1819–30’, that is, the dynamic effect, is negative and significant. The effect size of -6.8 percentage points is of the same magnitude as the aggregate net effect of trade reform in old territories found in column (1) and corresponds to a decrease of about 43 percent relative to the average price gap prevailing until 1791 in the Prussian territory that was stable between 1791 and 1816 (0.159). With regard to the late reformers, there is weak evidence that market integration in Saxony increased (columns 2 and 3) but this is not significant when location on a river is taken into account (column 4; coefficients for river controls not shown). In addition, the magnitude is smaller than in Prussia as would be expected in the absence of a trade reform. Hanover and Mecklenburg experienced an increase of price gaps, but the effect is not stable in the case of Mecklenburg. The contrasting trajectories of price gaps between city pairs located in the stable territory of Prussia, an early reformer, and the three late reformers suggest that the replacement of internal tolls and levies with a unified external tariff effectively reduced trade costs and massively increased the border effect measured by location within the same political territory.

The results of Table 4 also imply that trade reform was the main channel through which territorial consolidation contributed to market integration. First, this follows from the fact that the effects of trade reform among ‘newly included’ and ‘old territory’ city pairs are of similar magnitude. In column (3) of Table 4 we restrict the impact of reforms in old and new territories to one common parameter in using the term ‘Reform x newly included or old x 1816/19–30’. That is, a common effect is tested for Bavaria, the old and the new parts of Prussia (net of city pairs connected to the Rhine). The aggregate reduction of trade costs for this group of city pairs is about 5.8 percentage points, which lies between the values for the individual effects of trade reform in newly included city pairs (‘Reform x newly included x 1816/19–30’: -5.0 percentage points) and pairs of cities located in the same territory both before and after the Wars (aggregate of ‘Old territory x 1819–30 + Old territory x reform x 1819–30’: -6.6 percentage points) in column (1). F tests cannot reject the hypothesis that the coefficient estimates are equal.¹³

Second, column (1) of Table 4 corroborates earlier findings that components of the border effect other than those stemming from tariff regulation, such as transaction costs, were of little importance. While location in the same territory was associated with relatively lower price gaps prior to the era of trade reform, this effect was small and disappeared in late reformer states after the Wars, possibly as a result the trade diverting effects of tariff reform in neighbouring early reformer states. Overall, our findings support the hypothesis that the institutional unification of domestic markets that followed from the introduction of unified systems of external tariffs was the main channel through which territorial consolidation reduced trade costs within states.

Finally, Table 4 tests for third-market effects by considering effects of pairs that include eight foreign cities located in the neighborhood of the German lands (Figs. 1 and 2). The results on the effect of trade reform in the old territory of Prussia and the (stable) coefficient estimate for the effect ‘Reform x newly included x 1816/19–30’ (columns 1 to 2) imply that our main result is not driven by third-market effects. Furthermore, the significant positive effect for ‘Foreign 1 city x 1816–30’ in columns 1 to 3 suggests the possibility that territorial consolidation in the German lands combined with trade reform led to the diversion of trade with foreign cities towards domestic trade. However, the effect declines in magnitude and loses statistical significance when location on the Rhine is controlled for (column 4). Further exploration of the potentially trade diverting effects of territorial consolidation and trade reform is beyond the scope of this study.

¹³ F test of equality of the coefficient estimates for the ‘Reform x newly included x 1816/19–30’ and ‘Old territory x 1819–30 + Old territory x reform x 1819–30’ in column (1) with joint effect in column (3): not rejected with $p = 0.64$. Test of equality of the coefficient estimates for ‘Reform x newly included x 1816/19–30’ with the aggregate effect for ‘Old territory x 1819–30 + Old territory x reform x 1819–30’ in column (1): not rejected with $p = 0.36$.

Table 4

Results including city pairs that are located exclusively in old territories.

	Dependent variable: price gap			
	(1)	(2)	(3)	(4)
Newly included x 1816–30	0.008 (0.009)	0.008 (0.009)	0.010 (0.008)	0.007 (0.010)
Reform x newly included x 1816/19–30	-0.050** (0.022)	-0.052** (0.022)		
Foreign x 1816–30	0.052 (0.038)	0.066* (0.039)	0.066* (0.039)	0.047 (0.043)
Foreign 1 city x 1816–30	0.028** (0.014)	0.027** (0.014)	0.028** (0.014)	0.019 (0.014)
Old territory	-0.024*** (0.009)			
Old territory x 1816–18	0.005 (0.008)			
Old territory x 1819–30	0.036*** (0.012)			
Old territory x reform x 1819–30	-0.102*** (0.022)			
Prussia old x 1816–18		-0.016 (0.012)	-0.011 (0.011)	-0.016 (0.012)
Prussia old x reform x 1819–30		-0.068*** (0.019)		
Saxony x 1816–30		-0.018* (0.011)	-0.018* (0.011)	-0.015 (0.013)
Hanover old x 1816–30		0.049** (0.022)	0.050** (0.022)	0.057*** (0.022)
Mecklenburg old x 1816–30		0.042* (0.025)	0.042* (0.025)	0.030 (0.025)
Reform x newly included or old x 1816/19–30			-0.058*** (0.019)	-0.056*** (0.021)
River controls included	No	No	No	Yes
R ²	0.202	0.204	0.204	0.206
Adjusted R ²	0.195	0.197	0.197	0.199

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: city-pair price gap: $|\text{ratio of prices} - 1|$. SCC standard errors in (). All regressions include: city and time fixed effects, group effects and distance. No. of observations: 12,442.

7. Robustness checks

This section summarizes the robustness checks reported in the supplementary appendix. First, we discuss endogeneity concerns. Although existing research (Acemoglu et al., 2011; Huning and Wolf, 2021) regards the border changes that resulted from the Napoleonic Wars and the Congress of Vienna as plausibly exogenous, one might question this view. Because border changes agreed on at peace settlements should create utility for states whose territory is expanded, they may have targeted areas experiencing economic integration. Thus, it cannot be excluded that the border changes resulting from the peace settlements concluded in the Napoleonic era fail to constitute a natural experiment.

To address the concern of an association of treatment status with pre-existing integration trends, we run a falsification test by interacting the period 1786–91 (before the Wars) with treatment group status (Table S9 in SA2). If border changes and belonging to a reformer state were anticipated, we would see significant integration effects already before the Wars. This is not the case. To control for unobserved but time constant factors that could potentially be associated with treatment status, we also tested specifications with city-pair fixed effects (Table S8, models 2 and 3); results are similar.

Second, we test the common trend assumption underlying DD estimates by interacting the treatment indicator with a linear time trend (Table S8 in SA2). The estimate is not affected.

In addition, we address several potential concerns with the empirical results: the exclusion of the war period, the argument that disintegration of the control group might drive the main result, omission of relevant control variables, specific observations in the sample driving the main result and the specification of the distance restriction.

One potential problem with our empirical approach might be the considerable length of the Revolutionary and Napoleonic Wars, which effectively leads to a period of treatment. This distinguishes our baseline model from a standard DD framework, where a particular date that marks a sudden change is typically identified (e.g., Wooldridge, 2009, p. 453). The gap between the pre- and post-treatment periods could potentially affect our results if other events during the Wars impacted on price gaps. While we are unaware of such interventions, it is useful to check whether inclusion of data for the war period overturns the main result. For Bavaria, we reconstructed the timing of territorial changes during the Wars. When the beginning of the treatment is set to 1806 (Bavaria's first round of territorial gains) the effect is significant and of a similar size (see SA2, Table S5).

DD estimates can potentially be driven by disintegration in the control group (city-pairs where boundaries persist between the two

towns) rather than integration in the treatment group. However, descriptive statistics show that the average price gaps in the control group increased only marginally, from 17.7 percent (1780–91) to 17.9 percent (1816–30). Price gaps of those city pairs that were included into a new territory and became part of a reformer state decreased from 19 to 12.7 percent.

We also tested further control variables in addition to those discussed above. We motivate the inclusion of these variables at full length in the SA and report the corresponding results in Table S6. Furthermore, we performed sample variation exercises in which we drop specific cities or city-pairs (see Table S7) and vary the distance restriction (Table S10). These alternative estimates do not change the main result either.

8. Conclusion

We use the political transformation of Germany during the Napoleonic Wars as a natural experiment to study the impact of state formation on market integration. Specifically, territorial consolidation created a potential for developing domestic markets, and accession to full sovereignty following the dissolution of the Holy Roman Empire enhanced legal capacity, such as the capacity to provide basic trade regulations. Variation between states regarding the timing of trade reform, which primarily took the form of replacing heterogeneous internal tolls with a unified system of external tariffs, allows assessing the role of increasing legal capacity in the effect of territorial consolidation on market integration.

We construct a new dataset of price gaps and show that the removal of boundaries between pairs of cities located in territories experiencing trade reform reduced trade costs by about 5 to 6 percentage points (roughly 31 percent of the pre-war level). Our result is in line with a contemporary observer who noted: “A large obstacle to interior transport was the restrictions which one German state imposed upon another. As a result of the decreased number of states, trade will benefit.” (Winkopp, 1806, p. 66). However, we qualify this historical perspective by establishing that the effect of the removal of borders on trade costs was conditional on reforms that replaced heterogeneous internal tariffs by a unified system of external tariffs. City pairs in states that did not implement tariff reforms also did not experience a beneficial impact of territorial consolidation on trade costs.

We also show that city pairs that were part of Prussia already in 1791 experienced a substantial decline of price gaps following the tariff reform of 1818, which is of similar magnitude as the effect of being included into a territory implementing trade reform. Moreover, the advantage of location in the same territory prior to era of tariff reforms was small and unstable over time. Sharing territory-specific transaction standards and legal systems apparently had little effect on the relative costs of domestic vs. cross-border trade. Taken together, these findings establish that the effect of territorial consolidation on trade costs in reform-oriented states indeed operated primarily via a replacement of internal tolls and levies through external tariffs. Because such a reform became only possible following the accession of German states to full sovereignty, it provides a rough quantification of the economic effects of the rising legal capacity of states.

The missing integration effect of pure border removal without trade reform is in line with current explanations of border effects. In the considered case, political boundaries increased transaction costs during the pre-reform period only to a limited degree through differing transaction standards and legal systems. Trade was not taxed at borders but internally along rivers, roads and at town gates. Thus, the trade reforms gave state borders much of their ‘modern’ economic meaning by shifting formerly internal taxation of trade to the state frontier. In other words, to have strong border effects, borders need to be associated with significant policy barriers such as tariffs that require legal capacity of the state or alternatively, following Wolf et al. (2011), with cultural or geographical fragmentation.

The importance of territorial consolidation and rising legal capacity for economic development is underscored by a striking chronological parallel: The decline of trade costs in the two largest German states in the period immediately following the Napoleonic Wars went together with a transition of the German economy to a post-Malthusian regime of economic growth: Market integration and the resulting Smithian growth (e.g., Kelly, 1997) may have contributed to break the negative relationship between population and material welfare, and the improved functioning of grain markets reduced the demographic vulnerability with respect to short-term bottlenecks of food supply (Fertig et al., 2018; Pfister and Fertig, 2020; Pfister 2022b).

While territorial consolidation combined with trade reform created internal markets, it also possibly created new barriers that hindered inter-state trade. Our result that trade costs increased between German and foreign cities and within late-reformer states between the 1780s and the 1820s corroborates this view. Already contemporaries deplored the trade-diverting effects that resulted from the implementation of unified systems of external tariffs at the beginning of the nineteenth century (Berding, 1980, p. 536). This trade diversion likely created a motive for the institutional integration of markets at an inter-state level.

Thus, the territorial expansion and internal institutional unification of states occurring in the wake of the Revolutionary and Napoleonic Wars led to a limited round of economic integration at the regional level, which laid the basis for subsequent integration at the inter-state level via the *Zollverein* (Keller and Shiue, 2014; Huning and Wolf, 2019) and potentially via the Central Commission for Navigation on the Rhine. A further factor that became relevant after the 1820s was improved transport technology, particularly railway construction (Hornung, 2015). Nevertheless, it would take Germany another 100 years to evolve as an integrated economy in the 1930s (Wolf, 2009).

Acknowledgements

We thank the editor Timur Kuran and two anonymous reviewers for their constructive feedback. We are grateful for comments received by Lars Börner, Wolf-Heimo Grieben, Andreas Hoffmann, Michael Kopsidis, Martin Uebele, Fabian Wahl, Simone Wegge, Christoph Wunder and participants at earlier presentations at the FRESH meeting Vienna 2016, Department of Economics Halle, CGDE-workshop Jena, EHES Conference 2017 Tübingen, the SMYE 2018 Palma de Mallorca, the EHA-meeting in Montreal 2018, the GSWG conference Vienna 2021, and the WEast meeting Berlin 2021. All errors remain ours. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.jce.2023.01.007](https://doi.org/10.1016/j.jce.2023.01.007).

References

- Acemoglu, D., Cantoni, D., Johnson, S., Robinson, J.A., 2011. The consequences of radical reform: the French Revolution. *Am. Econ. Rev.* 101 (5), 3286–3307.
- Aker, J.C., 2010. Information from markets near and far: mobile phones and agricultural markets in Niger. *Am. Econ. J. Appl. Econ.* 2 (3), 46–59.
- Albers, H., Pfister, U., 2021. Climate change, weather shocks, and price convergence in pre-industrial Germany. *Eur. Rev. Econ. Hist.* 25 (3), 467–489.
- Amend-Traut, A., 2009. Wechselverbindlichkeiten vor dem Reichskammergericht. Praktiziertes Zivilrecht in der Frühen Neuzeit, Böhlau, Cologne.
- Becker, S.O., Hornung, E., Woessmann, L., 2011. Education and catch-up in the Industrial Revolution. *Am. Econ. J. Macroecon.* 3 (3), 92–126.
- Berding, H., 1980. Die Reform des Zollwesens in Deutschland unter dem Einfluss der napoleonischen Herrschaft. *Gesch. Ges.* 6 (4), 523–537.
- Boerner, L., Rubin, J., Severgnini, B., 2021. A time to print, a time to reform. *Eur. Econ. Rev.* 138 (103826), 1–20.
- Borchard, K., 1968. Staatsverbrauch und öffentliche Investitionen in Deutschland 1780–1850. PhD Dissertation. University of Göttingen.
- Borgius, W., 1899. Mannheim und die Entwicklung des Südwestdeutschen Getreidehandels. *Geschichte des Mannheimer Getreidehandels*, Mohr, Leipzig.
- Buggle, J.C., 2016. Law and social capital: evidence from the code Napoleon in Germany. *Eur. Econ. Rev.* 87, 148–175.
- Burda, M.C., Severgnini, B., 2018. Total factor productivity convergence in German states since reunification: evidence and explanations. *J. Comp. Econ.* 46 (1), 192–211.
- Chilosi, D., Murphy, T.E., Studer, R., Tunçer, A.C., 2013. Europe's many integrations: geography and grain markets, 1620–1913. *Explor. Econ. Hist.* 50 (1), 46–68.
- Croissant, Y., Millo, G., 2018. Panel Data Econometrics With R. Wiley, Hoboken, NJ.
- Dincecco, M., 2010. Fragmented authority from Ancien Régime to modernity: a quantitative analysis. *J. Inst. Econ.* 6 (3), 305–328.
- Dincecco, M., Katz, G., 2016. State capacity and long-run economic performance. *Econ. J.* 126 (590), 189–218.
- Donges, A., Meier, J.-M., Silva, R.C., 2022. The impact of institutions on innovation. *Manag. Sci.*, Forthcoming 1–24.
- Driscoll, J.C., Kraay, A.C., 1998. Consistent covariance matrix estimation with spatially dependent panel data. *Rev. Econ. Stat.* 80 (4), 549–560.
- Epstein, S.R., 2000. *Freedom and Growth: The Rise of States and Markets in Europe*. Routledge, London, pp. 1300–1750.
- Federico, G., 2012. How much do we know about market integration in Europe? *Econ. Hist. Rev.* 65 (2), 470–497.
- Federico, G., 2018. Market integration. In: Diebolt, C., Hauptert, M. (Eds.), *Handbook of Cliometrics*, 2nd edn. Springer, Berlin, pp. 1–26.
- Federico, G., Dincecco, M., 2022. Napoleon in Italy: a legacy of institutional reform? In: O'Brien, P. (Ed.), *The Crucible of Revolutionary and Napoleonic Warfare, 1792–1815 and European Transitions to Modern Economic Growth*. Brill, Leiden, pp. 141–168.
- Federico, G., Schulze, M.-S., Volckart, O., 2021. European goods market integration in the very long run: from the Black Death to the First World War. *J. Econ. Hist.* 81 (1), 276–308.
- Fehrenbach, E., 2001. *Vom Ancien Régime zum Wiener Kongress*. Oldenbourg, Munich.
- Fenske, J., Kala, N., 2021. Linguistic distance and market integration in India. *J. Econ. Hist.* 81 (1), 1–39.
- Fertig, G., Schlöder, C., Gehrman, R., Langfeldt, C., Pfister, U., 2018. Das postmalthusianische Zeitalter: die Bevölkerungsentwicklung in Deutschland, 1815–1871. *V. S.W.G.* 105 (1), 6–33.
- Gorissen, S., 2002. *Vom Handelshaus zum Unternehmen: Sozialgeschichte der Firma Harkort im Zeitalter der Protoindustrie, 1720–1820*. Vandenhoeck and Ruprecht, Göttingen.
- Greif, A., 2000. The fundamental problem of exchange: a research agenda in historical institutional analysis. *Eur. Rev. Econ. Hist.* 4 (3), 251–284.
- Häberle, E.J., 1974. Zollpolitik und Integration im 18. Jahrhundert: Untersuchungen zur wirtschaftlichen und politischen Integration in Bayern von 1765 bis 1811. *Wölfe*, München.
- Henderson, W.O., 1968. *The Zollverein*, 2nd ed. F. Cass, London.
- Hornung, E., 2015. Railroads and growth in Prussia. *J. Eur. Econ. Assoc.* 13 (4), 699–736.
- Huning, T.R., Wahl, F., 2021. The origins of agricultural inheritance traditions. *J. Comp. Econ.* 49 (3), 660–674.
- Huning, T.R. and Wolf, N., 2019. *How Britain unified Germany: endogenous trade costs and the formation of a customs union*. CEPR Discussion Paper DP13634, Centre for Economic Policy Research.
- Jacobs, A., Richter, H., 1935. *Die Großhandelspreise in Deutschland von 1792 bis 1934*. Hanseatische Verlagsanstalt, Berlin.
- Johnson, N.D., Koyama, M., 2017. States and economic growth: capacity and constraints. *Explor. Econ. Hist.* 64, 1–20.
- Keller, W., Shiue, C.H., 2014. Endogenous formation of free trade agreements: evidence from the Zollverein's impact on market integration. *J. Econ. Hist.* 74 (4), 1168–1204.
- Keller, W., Shiue, C.H., 2020. Market integration and institutional change. *Rev. World Econ.* 156, 251–285.
- Kelly, M., 1997. The dynamics of Smithian growth. *Q. J. Econ.* 112 (3), 939–964.
- Kiesewetter, H., 2007. *Die Industrialisierung Sachsens: ein regional-vergleichendes Erklärungsmodell*. Steiner, Stuttgart.
- Kopsidis, M., Bromley, D.W., 2016. The French Revolution and German Industrialization: dubious models and doubtful causality. *J. Inst. Econ.* 12 (1), 161–190.
- Kunz, A., 2014. *Deutschland 1792. Karte 720*. Institut für Europäische Geschichte Mainz, Server for Digital Historical Maps. <https://doi.org/10.25359/ISSN.1614-6352.MAP720>. <http://www.ieg-maps.uni-mainz.de/maps/mapp792d.htm>.
- Malinowski, M., 2019. Economic consequences of state failure—Legal capacity, regulatory activity, and market integration in Poland, 1505–1772. *J. Econ. Hist.* 79 (3), 862–896.
- Millo, G., 2017. Robust standard error estimators for panel models: a unifying approach. *J. Stat. Softw.* 82 (3), 1–27.
- MPIDR, 2019. *Historical GIS datafiles: Germany, German Union*. Max Planck Institute of Demographic Research and Mosaic, <https://censusmosaic.demog.berkeley.edu/data/historical-gis-files>. Accessed on September 16, 2019.
- Newman, K., 1985. Hamburg and the European economy, 1650–1750. *J. Eur. Econ. Hist.* 14 (1), 57–94.
- Oberschelp, R., 1982. *Niedersachsen 1760–1820*. Lax, Hildesheim.

- Oestmann, P., 2018. Court records as sources for the history of commercial law: the Oberappellationsgericht Lübeck as a commercial court (1820–1879). In: Pihlajamäki, H., et al. (Eds.), *Understanding the Sources of Early Modern and Modern Commercial Law*. Brill Nijhoff, Leiden, pp. 364–386.
- Ohnishi, T., 1973. Zolltarifpolitik Preußens bis zur Gründung des Zollvereins: Ein Beitrag zur Finanz- und Außenpolitik Preußens. Schwartz, Göttingen.
- Pesaran, M.H., 2007. A simple panel unit root test in the presence of cross-section dependence. *J. Appl. Econom.* 22 (2), 265–312.
- Pfister, U., 2022a. War, state growth and Germany's transition to post-Malthusian growth. In: O'Brien, P. (Ed.), *The Crucible of Revolutionary and Napoleonic Warfare, 1792–1815 and European Transitions to Modern Economic Growth*. Brill, Leiden, pp. 141–168.
- Pfister, U., 2022b. Economic growth in Germany, 1500–1850. *J. Econ. Hist.* 82 (4), 1071–1107.
- Pfister, U., Fertig, G., 2020. From Malthusian disequilibrium to the post-Malthusian era: the evolution of the preventive and positive checks in Germany, 1730–1870. *Demography* 57 (3), 1145–1170.
- Ploeck, F., 2021. A novel institution: the Zollverein and the origins of the customs union. *J. Inst. Econ.* 17 (2), 305–319.
- Reinhardt, K.-H., 1969. *Der deutsche Binnengüterverkehr 1820–1850, insbesondere im Stromgebiet des Rheins: Eine historisch-statistische Studie über die Entwicklung des Güterverkehrs auf Rhein, Neckar, Main, Mosel, Saar, Lahn, Ruhr und Lippe unter Berücksichtigung des entsprechenden Landstraßen- und Eisenbahngüterverkehrs*. PhD Dissertation. University of Bonn.
- Schulze, M.-S., Wolf, N., 2009. On the origins of border effects: insights from the Habsburg Empire. *J. Econ. Geogr.* 9 (1), 117–136.
- Shiue, C.H., 2005. From political fragmentation towards a customs union: border effects of the German Zollverein, 1815 to 1855. *Eur. Rev. Econ. Hist.* 9 (2), 129–162.
- Siegert, R., 2001. *Steuerpolitik und Gesellschaft: Vergleichende Untersuchungen zu Preußen und Baden 1815–1848*. Duncker und Humblot, Berlin.
- Spaulding, R.M., 2011. Revolutionary France and the transformation of the Rhine. *Cent. Eur. Hist.* 44 (2), 203–226.
- Spoerer, M., 2004. *Steuerlast, Steuerinzidenz und Steuerwettbewerb: Verteilungswirkungen der Besteuerung in Preußen und Württemberg (1815–1913)*. Akademie, Berlin.
- Stolz, O., 1954. Zur Entwicklungsgeschichte des Zollwesens innerhalb des alten Deutschen Reiches. *V.S.W.G.* 41 (1), 1–41.
- Thiemeyer, G., Tölle, I., 2011. Supranationalität im 19. Jahrhundert? Die Beispiele der Zentralkommission für die Rheinschifffahrt und des Octroi-Vertrages 1804–1832. *J. Eur. Integration Hist.* 17 (2), 177–196.
- Tilly, R.H., Kopsidis, M., 2020. *From Old Regime to Industrial State: A History of German Industrialization from the Eighteenth Century to World War I*. Chic. Univ. Pr., Chicago.
- Uebele, M., Gallardo-Albarrán, D., 2015. Paving the way to modernity: Prussian roads and grain market integration in Westphalia, 1816–1855. *Scand. Econ. Hist. Rev.* 63 (1), 69–92.
- Vitense, O., 1920. *Geschichte von Mecklenburg*. Perthes, Gotha.
- von Lancizolle, C.W., 1830. *Übersicht der Deutschen Reichsstandschafts- und Territorial-Verhältnisse vor dem Französischen Revolutionskriege, der seitdem eingetretenen Veränderungen und der gegenwärtigen Bestandteile des Deutschen Bundes und der Bundesstaaten*. Dümmler, Berlin.
- Wahl, F., 2016. Does medieval trade still matter? Historical trade centers, agglomeration and contemporary economic development. *Reg. Sci. Urban Econ.* 60, 50–60.
- Wehler, H.-U., 1987. *Deutsche Gesellschaftsgeschichte, vol. 1: Vom Feudalismus des Alten Reiches bis zur defensiven Modernisierung der Reformära 1700–1815*. Beck, Munich.
- Winkopp, P.A., 1806. *Ansicht des rheinischen Bundesstaates. Der Rheinische Bund: Eine Zeitschrift historisch-politisch-statistisch-geographischen Inhalts* 1, 56–67. http://ds.ub.uni-bielefeld.de/viewer/image/2105257_001/67/. accessed on 21 May 2018.
- Wolf, N., 2009. Was Germany ever united? Evidence from intra- and international trade, 1885–1933. *J. Econ. Hist.* 69 (3), 846–881.
- Wolf, N., Schulze, M.-S., Heinemeyer, H.-C., 2011. On the economic consequences of the peace: trade and borders after Versailles. *J. Econ. Hist.* 71 (4), 915–949.
- Wooldridge, J.M., 2009. *Introductory Econometrics: A Modern Approach*, 4th ed. South-Western Cengage Learning, Mason, Ohio.