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On the origins of border effects: insights from the Habsburg Empire

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Abstract

What are the origins of border effects on trade and why do borders continue to matter in periods of increasing economic integration? We explore the hypothesis that border effects emerged as a result of asymmetric economic integration in the unique historical setting of the multi-national Habsburg Empire prior to the First World War. While markets tended to integrate mainly due to improved infrastructure, ethno-linguistic networks had persistent trade diverting effects. We find that the political borders which separated the empire's successor states after the First World War became visible in the economy from the mid-1880s onwards, already 25–30 years before the First World War. This effect of a 'border before a border' cannot be explained by factors such as administrative barriers, physical geography, changes in infrastructure or patterns of integration with neighbouring regions outside of the Habsburg customs and monetary union. However, controlling for the changing ethno-linguistic composition of the population across the regional capital cities of the empire does explain most of the estimated border effects.

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1. Introduction

The relevance of political borders for trade has become a stylized fact in international economics. Border effects are visible both in large deviations from the law of one price (LOP) (Engel and Rogers, 1996) as well as in gravity estimates of border-related trade costs (McCallum, 1995). Their extent and dynamics still present a puzzle to economists. Why do borders continue to matter so much in periods of *increasing* economic integration? It is notable that even in the careful specification of Anderson and van Wincoop (2003) the US–Canadian border is estimated to have reduced trade by roughly 40% in 1993, 4 years after the introduction of a free-trade agreement.

In this article, we explore the hypothesis that border effects may emerge and persist as a result of asymmetric economic integration: while markets tend to integrate mainly due to improved technology and infrastructure, other trade costs, such as those associated with trade across ethno-linguistic networks, may have stayed high or even increased over time. We analyse the development of grain markets in the Habsburg economy at the level

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of 20 large cities from the late 1870s up to the First World War. We find that the political borders which separated the empire's successor states after the First World War were not detectable before the mid-1880s; however, they became visible in the economy from the mid-1880s onwards, already 25–30 years before the First World War. Further, this emergence of a 'border before a border' cannot be explained by factors such as administrative barriers, physical geography, asymmetric changes in infrastructure or patterns of integration with neighbouring regions outside of the Habsburg Empire. However, what does account for most of the estimated border effects is the ethno-linguistic composition of the population across the regional capital cities of the empire.

Apart from specification issues (Head and Mayer, 2002; Anderson and van Wincoop, 2003; Hillberry and Hummels, 2005), the literature suggests several explanations for the observation of border effects, where either firm heterogeneity (Evans, 2006; Chaney, 2008) or intermediate goods and endogenous firm location (Rossi-Hansberg, 2005) help to magnify initially small trade frictions along political borders. In an important empirical contribution, Combes et al. (2005) examine the effects of business and social networks on trade and the extent to which they can explain border effects, drawing on an older literature that emphasizes the trade-creating effects of networks (especially Greif, 1993; Rauch, 2001; Rauch and Trindade, 2002). According to Rauch and Trindade (2002), ethnic networks may promote trade by providing market information or by providing community enforcement of sanctions, for example by blacklisting traders that violate specific rules of the community. In the first case, their effects on trade should be larger for trade in differentiated products than for trade in homogeneous products, especially those with 'reference prices' such as grain (Rauch, 2001). In contrast, in the latter case, which has been highlighted by Greif (1993) and Rauch and Trindade (2002), community sanctions should affect trade independently of the characteristics of the traded goods.

Combes et al. (2005) find for a cross-section of French districts in 1993 that administrative borders are strongly trade diverting and, further, that business and social networks explain about one-third of this border effect. However, *after controlling* for such networks, they still find that trade within an administrative district exceeds trade across administrative borders by 170%. A problem with this approach is that—while networks seem to be a promising idea to understand border effects—the effects of networks on trade are notoriously hard to identify. Estimates of their economic impact relative to that of borders may well be biased due to issues of simultaneity and/or omitted variables: political and administrative borders were often drawn along the lines of pre-existing social networks, and the effects of these networks on the economy should have changed over time.

Here, we analyse the development of grain markets in pre-First World War Austria–Hungary. The comparatively simple nature of these product markets allows us to concentrate on the effects of networks while putting aside explanations of border effects based on firm heterogeneity or input–output linkages that may give rise to magnification effects. The specific features of the pre-1914 Habsburg economy of interest here are, first, the unusually large degree of ethnic and linguistic diversity not only across the multi-national empire as a whole, or between its major constituent parts Austria and Hungary, but also *within* its major sub-state regions and cities. Yet, second, all these cities were part of an empire-wide political, customs and monetary union. Thus, we can examine the possible emergence of 'border effects' in the absence of distorting inter-national barriers such as tariffs and exchange rate variations but also in the absence of any major intra-state administrative obstacles. Third, the Habsburg case offers an almost unique opportunity to explore the possible formation of an economic 'border before the border'. After the First World War, the Habsburg domains were split up among a number of successor states and along political borders that, obviously, were not in place before—the analysis is thus largely unaffected by the potential effects of unaccounted for political or administrative trade barriers that probably blur the results of other studies.[1]

Our approach is similar to Engel and Rogers (1996), Shiue (2005), Trenkler and Wolf (2005) and many other studies in taking non-random deviations from the LOP as indicators for trade costs. There are two identifying assumptions of our work. First, we assume that systematic deviations from the LOP reflect trade costs. Second, we assume that trade costs can be split up into three components: trade costs that depend on distance, trade costs that depend on networks (or related trade creating factors) along the lines of existing or prospective borders and location-specific trade costs (similar to the idea of multilateral resistance in Anderson and van Wincoop, 2003). If both distance-related and location-specific trade costs decrease over time, while the strength of networks stays high or even raises, the relative impact of the latter on trade will increase, which might result in the estimation of 'border effects'.

The rest of the article is organized as follows. Section 2 sets out the empirical strategy to assess the effects of borders on price dynamics, and outlines our data set including the extent of ethno-linguistic heterogeneity across the Habsburg Empire. Section 3 describes the emergence of a 'border before a border' within the Habsburg Empire prior to 1914; while, section 4 explores the impact of changing infrastructure and other candidate economic explanations on this finding. In Section 5, we test whether the effect of post-First World War borders prior to 1914 can be explained in terms of ethno-linguistic heterogeneity. Section 6 presents several robustness checks on this result. Finally, Section 7 summarizes the findings and points to some broader implications.

2. Empirical strategy and data

We test the hypothesis that within an overall integrating economy, the existence and intensification of ethno-linguistic networks gave rise to the emergence of internal borders. To this end, we look at the dynamics of grain prices: ceteris paribus two cities with little or no ethno-linguistic differences will tend to trade more with each other than cities with larger differences, given that trade networks tend to evolve along social and ethnic contacts (Greif, 1993; Rauch and Trindade, 2002). We approach this question in two steps. First, we ask whether border effects are visible at all in the dynamics of grain prices prior to the First World War and whether they changed over time. Second, we will explore to what extent these border effects and their dynamics can be explained by the impact of ethno-linguistic networks.

The relationship between price dynamics and trade costs is examined within a simple analytical framework. Consider two cities i and j, letting $P_{i,t}$ and $P_{j,t}$ denote the respective prices of the good in cities i and j and define $p_{i,t} = \ln(P_{i,t})$. Let $(p_{it} - p_{jt}) = gap_{ijt}$ denote the approximate percentage gap for the two prices at

1 For historical background to this study see Good (1984), Komlos (1983, 1989), Schulze (2000, 2007), Wolf (2005) and our CEPR discussion paper: M. S. Schulze and N. Wolf. (2007) *On the origins of border effects: insights from the Habsburg Customs Union*. CEPR Discussion Paper no 6327, CEPR, London, May 2007.

time t (Shiue, 2005). Assume further that the trade costs are proportional to the prices in the importing market place. In line with the recent economic geography literature, let $(1-e^{-\tau})P_{i,t}$ be the trade costs, where $\tau > 0$ is a cost parameter. Then, $e^{-\tau}P_{i,t}$ is the per-unit revenue when the good is sold in city i. Intuitively, τ depends positively on the geographical distance between the cities i and j. Moreover, when network effects are present, τ also differs depending on whether or not the city populations are part of the same network. Finally, trade from j to i is only profitable if $P_{i,t}\,e^{-\tau} > P_{j,t}$.This results in the condition: $\log(P_{i,t}/P_{j,t}) = \mathrm{gap}_{ij,t} > \tau$. Hence, arbitrage from j to i takes place when the percentage price gap is larger than the cost parameter τ. Equivalently, one trades from city i to j only if $\mathrm{gap}_{ij,t} < -\tau$. Thus, we obtain $[-\tau; \tau]$ as a band of no-arbitrage. Within this band, no trade occurs that could reduce price differences between the two markets because trade costs exceed possible arbitrage profits. Obviously, the size of this band increases with τ, which in turn will depend on several factors such as transport costs. In the literature, the trade cost view of the LOP is often referred to as a weak form of the LOP. It is equivalent to the so-called spatial arbitrage condition if it is only required that prices of the same good at two cities differ at most by the trade costs (see, for example, Fackler and Goodwin, 2001).

The analysis builds on three sets of data: grain prices, various measures of distance and infrastructure and language statistics. Let us briefly describe this data.

2.1. Prices

We use annual current wholesale price data for five types of grain (wheat, rye, barley, oats and corn) in 20 major cities of the empire to examine the integration of the Habsburg economy over the period 1878–1910. The main source for the price data are *SJB* and *ÖSH*, augmented by Pribram (1938, on Vienna), Hoszowski (1934, on Lemberg), Gorkiewicz (1950, on Cracow), *Preisstatistik (1913)* and *MSE* on Budapest and the other cities in Transleithania. Grain prices are given in the original sources in the same currency for all cities but sometimes for different volume or weight measures. We converted all prices into Austrian Heller per 100 kg to make them fully comparable both in the cross-section and over time. The sample of cities includes:

(a) Vienna, Linz, Graz and Innsbruck which became part of the post-World War I state of Austria;
(b) Prague in Bohemia, later becoming the capital of Czechoslovakia;
(c) Cracow and Lemberg in Galicia, both cities became part of the post-war Polish state;
(d) Czernowitz in the Bukowina which was ceded to Romania after the First World War;
(e) Trieste, in the Littoral, became part of Italy;
(f) Budapest, the centre of Hungary in both its pre- and post-war borders;
(g) Bratislava (Pozsony) in the Danube Left Bank district, later becoming part of Czechoslovakia;
(h) Pecs and Sopron in the Danube Right Bank district, part of both pre- and post-war Hungary, as was
(i) Szeged in the central Danube–Theiss Basin;
(j) Kassa on the right bank of the Theiss river, the area was ceded to Czechoslovakia after the First World War;

Map 1. The Habsburg Empire in 1914 Borders, main cities and post-1918 political borders

(k) Debreczen and Nagy Varad in the expansive Theiss Left Bank district; the predominantly Romanian parts of the region, where Nagy Varad was located, were later ceded to Romania along with
(l) Arad and Temesvar in the Theiss-Maros Basin, and
(m) Kolozsvar in Transylvania.

As shown on Map 1, the regional spread of the sample cities entails broad geographical coverage of the empire as a whole. The breakdown of the population by main language groups (Table 1) shows the extent of ethno-linguistic heterogeneity both within and across the sample cities.[2]

2.2. Distance and infrastructure measures

The analysis in Section 3 uses simple great circle distances between cities based on longitude and latitude data. The analysis in Section 4 in turn builds on a new data set that reconstructs the shortest railway connections between all 190 city pairs, starting

2 For 1880–1910, the censuses report the languages spoken rather than nationality or ethnicity. We use 'main language spoken' (Austria) and 'mother tongue' (Hungary) as proxies for the urban populations' composition by ethnicity. A comparison of the Austrian 1880 language data with the 1857 census data on nationality indicates a very close match even if allowance is made for inter-temporal shifts in the composition. See Horch (1992) on the relationship between language and national identity in the Habsburg context.

Table 1. Main languages spoken (shares in total population)

	German		Czech/Slovak		Polish		Ukrainian		Slovene		Serbo-Croat		Italian		Romanian		Hungarian	
	1880	1910	1880	1910	1880	1910	1880	1910	1880	1910	1880	1910	1880	1910	1880	1910	1880	1910
Vienna	0.958	0.941	0.036	0.054	0.003	0.003	0.001	0.001	0.001	0.001	0.000	0.000	0.001	0.001	0.000	0.000	0.000	0.000
Linz	0.984	0.995	0.015	0.004	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Graz	0.982	0.991	0.009	0.001	0.000	0.000	0.000	0.000	0.008	0.006	0.000	0.000	0.001	0.001	0.000	0.000	0.000	0.000
Innsbruck	0.985	0.976	0.002	0.002	0.000	0.001	0.000	0.002	0.000	0.002	0.000	0.000	0.013	0.016	0.000	0.000	0.000	0.000
Prag	0.165	0.068	0.834	0.930	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Lemberg	0.084	0.024	0.002	0.002	0.650	0.750	0.262	0.223	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.000
Cracow	0.039	0.024	0.007	0.012	0.953	0.959	0.001	0.004	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Czernowitz	0.266	0.288	0.004	0.003	0.071	0.098	0.479	0.383	0.000	0.000	0.000	0.000	0.000	0.000	0.180	0.228	0.000	0.000
Trieste	0.043	0.062	0.001	0.003	0.000	0.001	0.000	0.000	0.218	0.298	0.001	0.013	0.738	0.623	0.000	0.000	0.000	0.000
Budapest	0.212	0.084	0.065	0.029	0.006	0.006	0.000	0.000	0.000	0.000	0.010	0.005	0.001	0.001	0.001	0.002	0.705	0.872
Pozsony	0.183	0.137	0.456	0.434	0.002	0.002	0.000	0.000	0.000	0.000	0.007	0.005	0.001	0.001	0.000	0.000	0.351	0.421
Pecs	0.347	0.332	0.008	0.007	0.000	0.001	0.000	0.000	0.002	0.002	0.115	0.069	0.001	0.001	0.002	0.000	0.525	0.588
Sopron	0.413	0.387	0.004	0.004	0.000	0.000	0.000	0.000	0.001	0.001	0.117	0.110	0.000	0.001	0.000	0.000	0.464	0.497
Szeged	0.012	0.009	0.004	0.002	0.001	0.000	0.000	0.000	0.000	0.000	0.002	0.004	0.001	0.000	0.001	0.001	0.980	0.983
Kassa	0.069	0.031	0.293	0.181	0.004	0.004	0.003	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.631	0.781
Debreczen	0.017	0.004	0.003	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.001	0.978	0.994
Nagyvarad	0.010	0.005	0.011	0.013	0.001	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.435	0.413	0.542	0.568
Arad	0.107	0.094	0.012	0.014	0.000	0.000	0.000	0.002	0.000	0.000	0.007	0.005	0.001	0.000	0.639	0.584	0.233	0.301
Temesvar	0.350	0.328	0.016	0.012	0.001	0.000	0.000	0.000	0.000	0.000	0.141	0.133	0.000	0.000	0.421	0.363	0.070	0.164
Kolozsvar	0.042	0.029	0.002	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.612	0.574	0.343	0.395

Sources: Austria—Census (1880–1910); Hungary—Census (1880–1910).

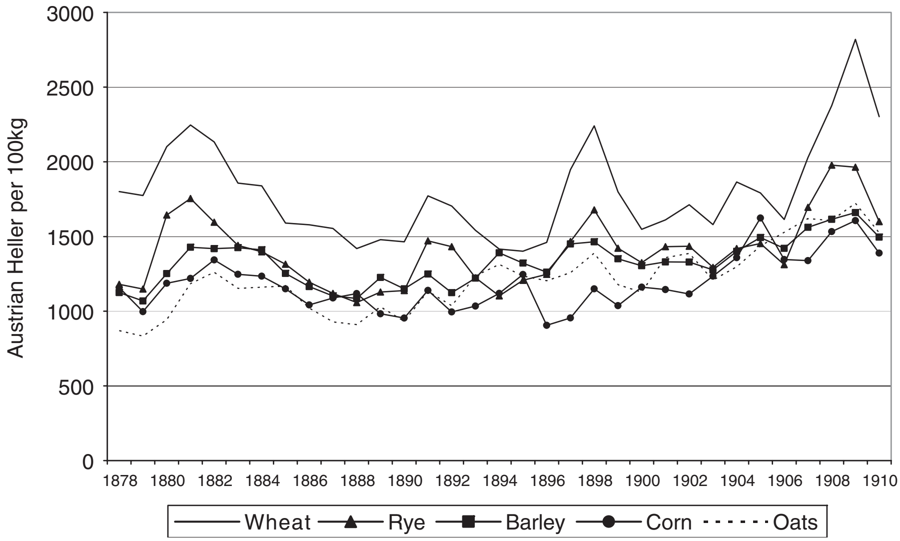

Figure 1. Cross-section average prices of five grains, 1878–1910.

in 1878. By that time, all cities in the sample were connected to the railway network. However, over time and up to the First World War, the network became denser and many bilateral distances shorter. This is fully accounted for in the time-varying railway distance measures derived here (see references under Railway Distances, 1878, 1882–1891, 1883, 1888, 1891, 1893, 1900, 1904, 1912, 1913a and 1913b).

2.3. Languages

Table 1 reports the composition of the population by language for all cities in the sample. The data, extracted from the official censuses (Austria—Census, 1880–1910; Hungary—Census, 1880–1910), refer to the population within the city boundaries and that of the immediately adjacent or surrounding administrative district. Given the widening over time of the geographical and administrative boundaries of some of the cities (especially, Budapest, Prague and Vienna), this makes for more stable and meaningful 'catchment areas'. The most striking feature here is the pronounced linguistic heterogeneity across the cities and, in some cases, the shifts in population shares held by the different national groups.

Let us consider some summary statistics of the data. A plot of the average prices of the five grains over time (Figure 1) shows that prices tended to decline until the mid-1880s, then fluctuated without a visible trend for about 20 years, before they started an upward tendency in the decade prior to the First World War. As usual, wheat prices are visibly above other grain prices, typically followed by rye and barley.

Figure 2 plots the ('global') coefficient of variation over the whole cross-section of the 20 cities averaged over wheat, rye, barley, oats and corn, 1878–1910 (solid line). The variation of grain prices across cities declined substantially over time, in line with previous findings of Good (1984). Yet, it is possible that integration proceeded asymmetrically, that is some city pairs integrated relatively more than others. Besides the 'global' coefficient of variation, the figure also plots the 'internal' coefficient

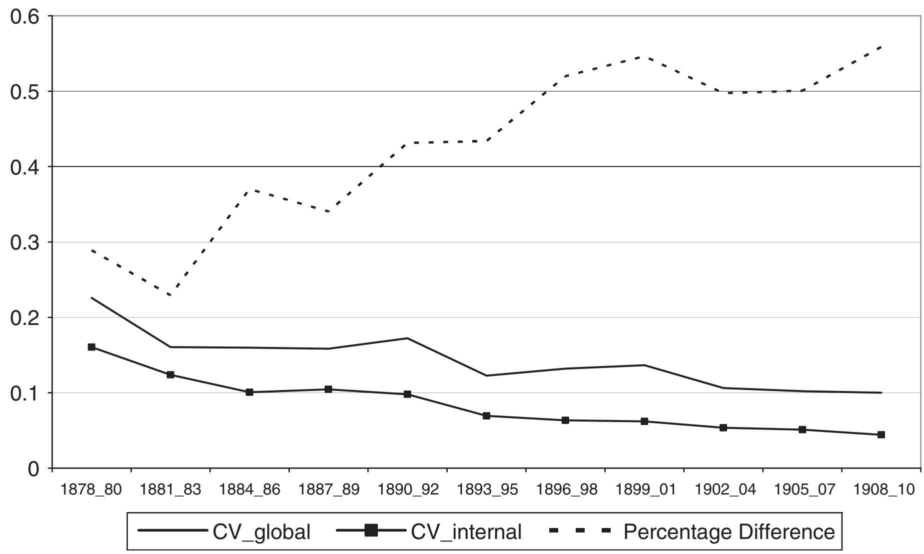

Figure 2. Global and internal coefficients of variations (CVs), pooled over five grains, 1878–1910.

of variation, calculated again as an average over the five grains but only for city pairs that after 1918 belonged to the same state (line with squares). Finally, we add the percentage difference between the 'global' and 'internal' price dispersion over time (dotted line).

First, city pairs with a common post-war border were apparently already in 1878 somewhat better integrated than other pairs, which can be partly explained in terms of their different average distances. Second, the percentage gap between 'global' and 'internal' price dispersion was increasing during 1878–1910, from about 30% to >50%: integration became progressively asymmetric since the mid-1880s.

To explore such asymmetric integration and its origins systematically, we examine the complete panel of price ratios between all 190 possible city pairs in our sample, drawing on more than 20,000 observations. Clearly, our analysis of the price dynamics in this panel has to take into account that the existence of trade costs imply a no-arbitrage band in relative prices and hence non-linearities. Moreover, to analyse the dynamics of border effects in such a panel, we need to focus on the cross-section; whilst, allowing for product- and city-specific factors and, crucially, allowing for structural change over time. A simple and straightforward approach that does all this was proposed by Engel and Rogers (1996). The basic idea is that on average, higher trade costs should limit the scope for arbitrage and hence increase the absolute gap between any pair of cities. Trade costs in turn can then be decomposed into border-related trade costs and other trade costs.[3] Here, we distinguish three categories of trade costs: transportation costs

3 One alternative empirical strategy would be to estimate the dynamics of prices between each city pair in a threshold cointegration or threshold auto-regression (TAR) framework (for example Lo and Zivot, 2001; Trenkler and Wolf, 2005). The key advantage of such a framework is that it allows distinguishing between the magnitude of transaction costs and differences in the speed of bilateral price adjustments that can both affect the price gap between cities. However, this approach is not well suited to the analysis of border

and other cost components that increase in geographical distance, cost components related to 'borders' and trade costs that are city-specific. We estimate the following simple specification:

$$\left|\text{gap}_{ij,t}^{h}\right| = c_0 + c_1 * \log(distance_{ij}) + c_2 * border_{ij} + \sum_{g=1}^{20} c_g\, city_g + \varepsilon_{ij,t}^{h}, \qquad (1)$$

where *distance* is the geographical distance between the two cities, *city* is a full set of dummies over all cities g to capture unobservable city-specific factors, while $\varepsilon_{ij,t}^{h}$ is an i.i.d. error component.[4] The index h stands for the five kinds of grain in our sample. The variable *border* is a dummy defined along the post-war borders that separated the empire's successor states after 1918:

$$border_{ij} = \begin{cases} 1 & \text{if citites } i, j \text{ after WWI separated by a border} \\ 0 & \text{else.} \end{cases} \qquad (2)$$

3. Main results: the emerging border

We estimate (1) by OLS, where we start by pooling over all city pairs, all grains and over the complete period 1878–1910. That is, we initially assume that distance and border effects are equal for all types of grain and relax this assumption later. The variation to identify border effects in this and all following estimations comes essentially from the cross-section of city pairs, which allows us to control for city-specific effects. Moreover, we will use variation over time to make indirect inferences about the origins of the estimated border effects. Reported standard errors are based on White's heteroskedasticity consistent covariance matrix estimator throughout. Table 2, column 1 gives the results.

As expected, we find that price dispersion increases significantly in distance between the cities, controlling for unobservable city-specific effects. The border dummy is positive and highly significant, which is evidence of trade costs related to the post-war borders, visible for the period 1878–1910. This result is robust to other specifications that add controls for unobservable effects in the time dimension and in the cross-section of grain types and city pairs and to specifications that relax the OLS assumptions of homoskeadstic and uncorrelated errors, allowing for heteroskedasticity and correlation along these dimensions. Especially, we tested whether missing data introduced a systematic bias by restricting the estimation to a balanced panel estimation

effects. First, one needs to fit a TAR model to each of the 190 city pairs separately. Second, inference on border effects between city pairs can only be made indirectly by comparing the pair-wise results, which in turn requires structural stability over time *and* comparable time series dynamics in the cross-section. The argument of this article is that there are many factors that might affect the price gap between cities, which might be specific for specific products and specific for cities. Crucially, we argue that these factors will change over time. For a comparison of the two approaches see Trenkler and Wolf (2005).

4 We tested whether our results are affected by the definition of the dependent variable as the price gap between city pairs—defined as $\text{gap}_{ijt} = p_{it} - p_{jt} = \text{Ln}(P_{it}/P_{jt})$. According to Engel and Rogers (1996), higher trade costs should limit the scope for arbitrage and hence increase the band of no-arbitrage and the scope for fluctuations of the price ratio between locations. An alternative way to capture this idea in a panel setting is to use the standard deviation of price ratios as the dependent variable (Parsley and Wei, 1996; Chen, 2004). Using the standard deviation of price ratios over 3- or 5-year intervals instead of the annual price gap leaves all our results qualitatively unchanged.

Table 2. Border effects—basic results, pooled over five grains, 1878–1910

	Pooled OLS	Pooled OLS, balanced sample
Variable	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)
Constant	−0.144 (−10.943)	−0.193 (−7.608)
log(dist)	0.046 (21.088)	0.052 (12.399)
Border	0.028 (11.089)	0.030 (6.250)
Fixed City Effects	Yes	Yes
No. of Obs.	25,297	10,070
Adj. R^2	0.094	0.095

Dep. Var. abs($gap^{k}_{ij,t}$), White robust standard errors and covariance.

(Table 2, column 2) and specifications with two-way (period and cross-section) random effects by grain and city pairs and with period fixed and random cross-section effects by grain and city pairs (data not shown). It is equally robust to the introduction of cross-section or period weights and feasible GLS estimators that allow for heteroskedastictiy and various forms of correlation (data not shown). Note that the border effect is estimated to be highly significant after controlling for distance. The variation in our sample is sufficiently high to distinguish between bilateral price differences related to distance and bilateral price differences related to border effects. While very distant cities also tended to be separated by a border after the First World War, our sample has a lot of variation in this respect. Ten per cent of the city pairs were less than 200 km apart and still divided by a border after the First World War, and roughly 40% of all city pairs were less than the median distance (369 km) apart, but nevertheless separated by a border after the First World War.

How can we interpret this result? The estimated border effect is obviously not explained by any systematic administrative barrier related to it, since these barriers were not in place before 1918.[5] It is equally not explained by the fact that typically cities that after 1918 belonged to the same state were geographically close to each other, or by city-specific effects that possibly happened to differ along the future border. A key to the nature of this border effect may lie in changing patterns over time: if the border effect was present in 1878 as well as in 1910 it probably reflects differences in time-invariant characteristics of city pairs across the Habsburg Empire, such as effects from natural geography. If, however, the border effect is changing over time, the timing of these changes might point to the forces behind. In a next step, we re-estimate model (1) where we allow both the distance and border coefficients to vary over 3-year intervals (Table 3, column 1).

The border effect begins to emerge only during the 1880s, increases up to the early 1890s, and stabilizes between the mid-1890s and 1910. A Wald-coefficient test indicates that the increase during 1878–1890 is significant at the 5% level. The story is similar for the unbalanced and the balanced panel estimation, except that for the latter (data not shown) the increase during the early 1890s is stronger. After a peak in the 1890s, there is some decline, but the effect stays above the level reached in the early-1880s.

5 We also tested for the impact of the pre-war border between Austria and Hungary and found that this does not alter our results.

Table 3. Border effects—time variation, infrastructure, city-specific shocks, and random-border effects, 3-year intervals 1878–1910

	Pooled OLS	Pooled OLS, time-varying railway distance	Pooled OLS, railway distances and time varying city shocks	Pooled OLS, railway distances, and 'random'-border, balanced sample
Variable	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)
Constant	−0.141 (−11.080)	−0.156 (−11.811)	−0.165 (−13.06)	−0.271 (−9.221)
log(dist)*78_80[a]	0.060 (20.907)	0.063 (21.306)	0.055 (12.005)	0.089 (6.956)
log(dist)*81_83	0.054 (20.883)	0.057 (21.296)	0.046 (10.003)	0.077 (9.115)
log(dist)*84_86	0.050 (20.774)	0.053 (21.171)	0.044 (11.315)	0.069 (10.219)
log(dist)*87_89	0.466 (19.905)	0.050 (20.309)	0.036 (9.636)	0.063 (9.654)
log(dist)*90_92	0.455 (18.362)	0.049 (18.846)	0.041 (9.258)	0.067 (7.349)
log(dist)*93_95	0.436 (19.430)	0.047 (19.842)	0.048 (15.181)	0.068 (8.738)
log(dist)*96_98	0.045 (20.692)	0.048 (21.096)	0.062 (21.665)	0.071 (12.444)
log(dist)*99_01	0.043 (20.090)	0.046 (20.537)	0.061 (23.219)	0.072 (13.313)
log(dist)*02_04	0.042 (19.884)	0.045 (20.327)	0.060 (22.627)	0.088 (13.610)
log(dist)*05_07	0.040 (19.884)	0.043 (19.553)	0.046 (19.656)	0.062 (12.006)
log(dist)*08_10	0.038 (18.143)	0.041 (18.597)	0.050 (19.859)	0.064 (12.116)
border*78_80[b]	−0.012 (−0.933)	−0.012 (−0.894)	−0.006 (−0.461)	−0.075 (−2.616)
border*81_83	0.015 (1.414)	0.015 (1.426)	0.004 (0.367)	−0.037 (−1.963)
border*84_86	0.044 (5.186)	0.043 (4.989)	0.026 (3.077)	−0.010 (−0.757)
border*87_89	0.045 (6.079)	0.042 (5.730)	0.038 (4.951)	−0.016 (−1.288)
border*90_92	0.086 (9.322)	0.082 (8.928)	0.076 (7.345)	−0.004 (−0.144)
border*93_95	0.029 (4.817)	0.026 (4.366)	0.025 (4.010)	−0.022 (−1.254)
border*96_98	0.022 (4.333)	0.019 (3.748)	0.033 (6.315)	0.007 (0.967)
border*99_01	0.031 (7.876)	0.027 (7.018)	0.034 (8.212)	0.008 (1.153)
border*02_04	0.022 (5.108)	0.019 (4.435)	0.031 (6.806)	−0.012 (−1.306)
border*05_07	0.012 (3.616)	0.010 (2.847)	0.019 (5.319)	0.004 (0.573)
border*08_10	0.021 (5.602)	0.018 (4.874)	0.028 (7.187)	−0.001 (−0.181)
Fixed City Effects	Yes	Yes	No	No
Time varying City Effects	No	No	Yes	Yes
No. of Obs.	25,297	25,297	25,297	10,070
Adj. R^2	0.151	0.151	0.228	0.247

Dep. Var. abs($gap^k_{ij,t}$), White robust standard errors and covariance.
[a]log(dist) is the natural logarithm of the geographical distance between two cities in column 1 only. It is the shortest possible distance on railways between two cities at any given period of time in all remaining columns.
[b]In columns 1–3, border is a dummy variable defined along the post-war borders according to (2) in the text. Instead, in column 4 border is a random border dummy as explained in section 4 in the text.

This pattern over time corresponds to a political historiography which stresses the rise in intra-empire national conflict from the late-1880s and, for instance, the political deadlock between Czechs and Germans from the early-1890s (Kornish, 1949; Sked, 2001; Berend, 2003), as much as recent research on intra-state economic nationalism which emphasizes the growing significance of ethnic mobilization and antagonism during the 1890s (Bruckmüller and Sandgruber, 2003; Jaworski, 2004; Lorenz, 2006). The effect of geographical distance decreases over time—as do the city-specific effects (not reported). This reflects the general trend towards better integration of grain markets across the Habsburg domains, as noted in Figure 2 and stressed in the work of Good (1984). We also tested for differences between grains (data not shown) and find that the border effect is significantly smaller for wheat than for other grains. This is in line with economic reasoning, namely the idea of 'shipping the good apples out'. As shown in Figure 1, the unit value of wheat is above that of other grains. According to the Alchian–Allen Hypothesis, an increase in trade costs will decrease the price of goods with a high unit value relative to the low unit value good, which results in an increase in the relative demand for the high unit value good (Bocherding and Silberberg, 1978; Hummels and Skiba, 2004). This also explains the persistently smaller distance coefficient for wheat compared to other grains.

4. Non-network explanations for the border effect: infrastructure bias, cross-border integration and some robustness checks

In principle, several factors may account for the estimated border effect. Increasing inter-regional differences in the density of communications networks may have followed the fault-lines of the post-1918 political dismemberment of the empire already *before* the First World War. If this had been the case systematically, it would not be captured in the time-varying distance coefficient but instead show up in the estimated coefficient on the border dummy. To explore this, we use the new data set on the shortest railway distances between all 190 city pairings and for each year over 1878–1910. Let us re-estimate model (1) replacing constant geographical distances by time-varying railway distances (Table 3, column 2).

The estimated coefficients on railway distances differ only slightly from the coefficients on geographic distances, and show the same pattern over time: they decline, reflecting the general trend towards better integration. Given this, it is no surprise that the estimated border effects are not changed very much. In fact, the coefficients are slightly lower after controlling for changing distances due to improved railway infrastructures, which indicates that to some very limited degree infrastructure was better and improved more along the lines of the future political borders than across them. But this effect is tiny. Moreover, the pattern of insignificant border effects for the first years, followed by the emergence of a significant effect, remains unchanged.

Next, features of natural geography might in principle account for the observed border effects, for example a mountain range or rivers. But there are few obvious candidates. We tested for the effect of location at or close to the Danube river, which did not affect our results. The Carpathian Mountains help little to explain the segmentation of our sample along the future borders but rather delineate the Habsburg Empire from Russia and the Ottoman Empire in the east and south-east. However, what might have affected the pattern of relative integration across Austria–Hungary is changing integration or disintegration with neighbouring regions. Especially the various large changes in German and Russian tariffs 1878–1910 might have affected different regions of the Habsburg economy in a different way. In our econometric model, we can take this into account by adding to model (1) a set of time-varying city effects, to capture city-specific integration or disintegration shocks:

$$\left|\text{gap}^{k}_{ij,t}\right| = c_0 + c_1 * \log\left(raildistance_{ij,t}\right) + c_2 * border_{ij} + \sum_{t=1878}^{t=1910} \sum_{1}^{J} c_{kt} location_k + \varepsilon^{k}_{ij,t}. \quad (3)$$

In Table 3, column 3 shows that the introduction of time-specific city effects does not alter the estimated patterns of border effects. However, the estimated distance effects are changed insofar as the overall trend towards better integration is now picked up partly by the time-varying city effects, which tend to decline over time (data not shown). We conclude that city- and time-specific differences in integration—such as varying degrees of integration with neighbouring regions—cannot explain the observed emergence of a border effect within the Habsburg economy. The results are not changed if we restrict attention to a balanced sample (data not shown).

Finally, it is still possible that the estimated border effects capture some effects that just happened to affect some groups of cities more than others, without being related to the course of the future political borders. We can estimate effects of a 'nonsense border' that simply splits the sample of 20 cities into four groups of five cities, located roughly in the south-east, south-west, north-east and north-west of the Habsburg Empire.[6] The last column in Table 3 shows that the effect of such a border is in most cases not significantly different from zero, and has the wrong sign otherwise. Hence, if anything some city pairs appear to be better integrated across than within this hypothetical 'border'. The future political borders apparently do capture some trade costs, which cannot be easily explained in terms of infrastructure, natural geography, external integration or coincidence. The most plausible explanation seems to be some effect of social networks on trade, which both in absolute terms and relative to other factors gains in importance over the period under review. This is what the next sections turn to.

5. Did ethno-linguistic networks matter?

If our border dummy is in fact capturing the trade creating and diverting effects from ethno-linguistic networks within the Habsburg Empire, the border effect should be explicable by some measure of ethno-linguistic heterogeneity across the Habsburg regions. To this end, we use the language statistics from Table 1 to construct a *bilateral* measure of the ethno-linguistic similarity of the two cities in any one of our 190 city pairs. We calculate this measure as

$$Language_{ij,t} = \sum_{k=1}^{n} \left(a_{i,t}^{k} * a_{j,t}^{k} \right) \tag{4}$$

where $a_{i,t}^{k}$ is the percentage share of language k in city i and at time t, and n is the total number of language groups (in our case $n=9$). This is equivalent to summing up over all possible same language encounters between individuals from the two different cities in each pair and dividing by the total number of all possible encounters for that city pair.[7]

This 'matching probability' varies between 0 (no similarity between two cities i and j) and 1 (no differences) and is, therefore, comparable to the 0/1 border dummy.

6 We grouped the cities into the following four groups. 'Southwest': Innsbruck, Trieste, Graz, Pecs and Budapest. 'Southeast': Szeged, Arad, Temesvar, Nagy Varad and Kolozsvar. 'Northwest': Prague, Linz, Vienna, Sopron, and Bratislava. 'Northeast': Cracow, Lemberg, Kassa, Debreczen and Czernowitz.

7 The underlying absolute figures are truncated to facilitate a full match of the Austrian and Hungarian language groups. The very small group of 'other languages' (i.e. those different from the nine main languages referred to in Table 1), which is not reported for Austrian cities and for Hungary available only for 1910, is excluded.

For example, for 1910 the lowest score is 0.0004 for the city pair Lemberg–Szeged, the highest is 0.9864 for Graz–Linz. If the emerging border was driven by ethno-linguistic network effects and if these networks can indeed be captured via language statistics, our matching probability might help to explain the estimated border effect. Note that our idea is to use language as a proxy for membership in a specific ethno-linguistic network, say Czech or Hungarian, similar to Rauch and Trindade (2002), rather than to use language as a control for the costs of communication. We will explore the latter issue in the next section.

To test for the effect of common network membership, we re-estimate (3) replacing the border dummy (2) by the bilateral ethno-linguistic 'matching probability' (4). Given the definition of 'matching probability', which increases in ethno-linguistic similarity between city pairs, we expect to find a negative effect on price dispersion. In a further step, we re-estimate the border-effect controlling for language heterogeneity. Table 4 reports the results, where we always limit our attention to the balanced sample, control for possible city-specific and period-specific shocks, and allow for heteroskedasticity in the sample.

As hypothesized, ethno-linguistic heterogeneity essentially captures the estimated border effect. It has a very similar impact on relative price integration from the mid-1880s onwards insofar as the effect starts to be visible (statistically significant with the expected negative sign) only after the mid-1880s. Furthermore, if we re-estimate the border effect but control for ethno-linguistic heterogeneity, this border effect vanishes, except in one sub-period in the late 1880s, where the border effect is still estimated to be positive and significant, but much smaller than without the control.[8] Overall, we conclude that ethno-linguistic networks as captured by our measure of matching probability almost fully explain the emergence of border effects along the future political borders across the Habsburg Empire. While transport costs and probably other trade barriers declined, the trade creating and trade diverting effects of ethno-linguistic networks stayed high and even gained in relevance, which significantly biased the process of economic integration. To put a number on that effect, according to the average coefficients on distance and ethno-linguistic heterogeneity from Table 4, column 1, doubling the 'matching probability' between any two cities in the sample reduces the price dispersion between them by more than a 50% reduction in the railway distance between the two cities.

This finding ties in well with recent qualitative work on the prevalence of intra-state economic nationalism in Central and Eastern Europe. This literature suggests that ethnically based networks increasingly affected trading costs between different ethnic groups by systematically directing trade towards the own group (Jaworski, 2004; Lorenz, 2006). Jaworski's (2004) research on boycott movements between different ethnic groups within the multi-national setting of East Central Europe points to ethnic mobilization as a key element of intra-state economic nationalism at work. Nationalist élites sought to mobilize popular political support for the national cause and to advance the economic interests of their clientele. If so, arbitrage trade between ethnic groups became increasingly limited and hence affected price dynamics in a similar way as other, e.g. distance related, trading costs. In his study of rural cooperatives in Galicia,

8 We also tested for the role of changes in the ethno-linguistic composition over time (Table 1), by keeping the composition of 1880 constant over time. This had only very minor effects on our results.

Table 4. Explaining border effects—ethno-linguistic networks, pooled over time and pooled in 3-year intervals 1878–1910

	Ethno-linguistic networks	Networks and borders	Herfindahl index	'Communication Effect'
Variable	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)	Coeff. (*t*-stat.)
Constant	−0.133 (−4.858)	−0.140 (−5.150)	−0.157 (−6.055)	−0.167 (−6.377)
log(dist)*78_80	0.055 (5.266)	0.059 (5.087)	0.060 (5.322)	0.063 (5.401)
log(dist)*81_83	0.051 (6.679)	0.053 (6.669)	0.053 (6.556)	0.056 (7.161)
log(dist)*84_86	0.047 (7.842)	0.046 (7.371)	0.046 (7.298)	0.049 (8.046)
log(dist)*87_89	0.039 (6.752)	0.037 (6.011)	0.037 (6.053)	0.041 (6.788)
log(dist)*90_92	0.043 (5.329)	0.041 (4.634)	0.032 (3.661)	0.044 (4.925)
log(dist)*93_95	0.042 (6.081)	0.041 (5.427)	0.039 (5.293)	0.043 (7.575)
log(dist)*96_98	0.052 (10.988)	0.052 (11.031)	0.052 (7.982)	0.055 (11.998)
log(dist)*99_01	0.054 (11.857)	0.055 (12.205)	0.054 (11.973)	0.057 (13.252)
log(dist)*02_04	0.066 (12.692)	0.065 (12.237)	0.066 (12.251)	0.068 (13.039)
log(dist)*05_07	0.042 (9.840)	0.044 (10.265)	0.043 (10.126)	0.047 (11.403)
log(dist)*08_10	0.044 (10.096)	0.044 (10.245)	0.046 (11.169)	0.048 (11.388)
border*78_80	–	−0.026 (−0.607)	−0.029 (−0.682)	−0.024 (−0.636)
border*81_83	–	−0.008 (−0.383)	−0.011 (−0.529)	0.000 (0.007)
border*84_86	–	0.021 (1.597)	0.020 (1.606)	0.027 (2.053)
border*87_89	–	0.029 (2.422)	0.029 (2.011)	0.031 (2.578)
border*90_92	–	0.023 (0.931)	0.016 (0.624)	0.056 (2.060)
border*93_95	–	0.019 (0.955)	0.013 (0.674)	0.039 (1.947)
border*96_98	–	0.013 (1.575)	0.016 (1.738)	0.022 (3.063)
border*99_01	–	−0.001 (−0.092)	0.002 (0.356)	0.013 (1.808)
border*02_04	–	0.013 (1.294)	0.013 (1.358)	0.020 (2.099)
border*05_07	–	−0.004 (−0.670)	−0.003 (−0.486)	0.005 (0.933)
border*08_10	–	0.004 (0.685)	0.005 (0.765)	0.009 (1.511)
lang*78_80[a]	0.011 (0.201)	−0.016 (−0.248)	0.026 (0.379)	−0.010 (−0.203)
lang*81_83	−0.038 (−1.238)	−0.046 (−1.229)	0.056 (1.398)	−0.020 (−0.618)
lang*84_86	−0.068 (−2.625)	−0.046 (−1.544)	0.044 (1.369)	−0.027 (−0.975)
lang*87_89	−0.075 (−3.395)	−0.044 (−1.685)	0.045 (1.602)	−0.039 (−1.589)
lang*90_92	−0.271 (−5.311)	−0.246 (−4.533)	0.268 (4.573)	−0.167 (−3.190)
lang*93_95	−0.098 (−2.747)	−0.078 (−1.864)	0.092 (2.056)	−0.014 (−0.337)
lang*96_98	−0.060 (−4.666)	−0.046 (−3.016)	0.037 (2.359)	−0.017 (−1.275)
lang*99_01	−0.069 (−5.678)	−0.069 (−4.859)	0.061 (4.148)	−0.033 (−2.417)
lang*02_04	−0.055 (−4.255)	−0.041 (−2.753)	0.038 (2.467)	−0.019 (−1.425)
lang*05_07	−0.068 (−6.580)	−0.072 (−6.006)	0.068 (5.672)	−0.046 (−4.170)
lang*08_10	−0.043 (−3.635)	−0.040 (−3.005)	0.034 (2.714)	−0.024 (−2.332)
Time varying City Effects	Yes	Yes	Yes	Yes
No. of Obs.	10,070	10,070	10,070	10,070
Adj. R^2	0.252	0.253	0.252	0.250

Dep. Var. abs($gap^{k}_{ij,t}$), White robust standard errors and covariance, balanced sample.
[a]'Lang' in columns 1 and 2 is defined as a matching probability according to (4) in the text. In Table 4, column 3 is defined as a Herfindahl index according to (5) in the text. In column 4, it is defined as a matching probability that takes lexicostatistical similarities into account, according to (6) in the text.

Struve (2006, 229) identifies a key feature that applies not only to the specific case he investigates but more broadly to the late 19th century nation-building efforts that evolved within the multi-ethnic Habsburg setting: '(. . .) different nation-building projects competed with one another'. This 'competition', the evidence suggests, acquired an increasingly exclusionary quality—Bruckmüller and Sandgruber's (2003) 'self-integrating national communities' were indeed alive and ventured to keep 'others' out. The impact of ethno-linguistic networks was, in turn, magnified by the general process of integration due to declining transport costs. Taken together, this explains largely why political borders which divided the empire after 1918 became visible in the price dynamics of grain markets already 25–30 years before the First World War.

6. Robustness tests on the role of ethno-linguistic networks

We argue that language statistics can capture membership in ethno-linguistic networks, which in turn help to explain the emergence of a border effect within the Habsburg Empire prior to 1914. First, let us explore whether these results are driven by the specific metric of a 'matching probability' that we use. Alternatively, we can define a bilateral index of ethno-linguistic heterogeneity, similar to a Herfindahl index as

$$\text{Lang\_Heterogeneity}_{ij,t} = \frac{1}{2}\sum_{k=1}^{n}\left(a_{i,t}^{k} - a_{j,t}^{k}\right)^{2}. \tag{5}$$

If both cities have an identical ethno-linguistic composition, the index equals 0. If there is no overlap in the ethno-linguistic composition, the index can reach 1 at maximum. This index works essentially like a 'fine-tuned' border dummy as it varies between 0 and 1 and directly reflects the idea that trade costs between a pair of cities are expected to increase in the ethno-linguistic heterogeneity between them. As shown in Table 4, column 3, the results remain essentially unchanged compared to Table 4, column 2.

Finally, let us explore in some more detail what kind of network effects are actually captured by the language statistics that we use here. Following Rauch and Trindade (2002), we can distinguish between a 'communication effect' and a 'community effect' of membership in an ethno-linguistic network. The basic idea is that networks can provide market information, which should be facilitated by the ability to speak a common or at least a very similar language. In contrast, networks can also provide community enforcement of sanctions, for example by blacklisting traders that violate specific rules of the community. In the former case, one would expect to find network effects on trade in differentiated products, not necessarily on trade in homogeneous products like wheat or rye. However, in the latter case, which has been highlighted by Greif (1993) and discussed in Rauch and Trindade (2002) and others, community sanctions should affect trade independently of the characteristics of the traded goods. They should affect trade in grain as much as trade in machinery or textiles. Because our focus here is on grain, it should be the second effect that matters most in our case. To distinguish between the 'communication effect' and the 'community effect', we extend our 'matching probability' using the lexicostatistical similarities between languages, following Dyen et al. (1992).

$$\text{Communication}_{ij,t} = \sum_{l=1}^{n}\sum_{k=1}^{n}\left(a_{i,t}^{k} * a_{j,t}^{l} * SW200^{kl}\right), \tag{6}$$

where SW200 is the index of lexicostatistical similarity between languages k and l based on the Swadesh-200 list of basic words as used in Dyen et al. (1992). For example, this index is 0.660 for Rumanian and Italian, 0.223 for Rumanian and Czech, 0.249 for

Rumanian and German, 0.766 for Czech and Polish and 0.259 for Czech and German.[9] In this way, the index captures the probability that a citizen from Lemberg meets a citizen from Arad or Vienna in our sample with whom he can actually communicate. In this way, we can exploit the available language statistics via (6) to capture the 'communication effects' rather than the 'community effects' of membership in a specific ethno-linguistic group. Given that the latter effect should be driving our results on grain prices, we expect that the communication index (6) helps much less to explain the border effect. As shown in Table 4, last column, this is in fact what we find. Not communication as such was the problem, which would also be difficult to square with an increase in the border effects over time, but conflict between distinct ethno-linguistic groups; in short, economic nationalism.[10]

7. Conclusion

What explains the existence of border effects and why do borders continue to matter even in periods of increasing economic integration? This article explores the idea that border effects measured in terms of non-random deviations from the law of one price reflect persistent network effects on trade in an environment of otherwise declining trade costs. We analyse the dynamics of grain markets in the late 19th Habsburg economy at the level of 20 large cities as a natural experiment with some convenient features. The relatively simple nature of these grain markets allows us to exclude explanations of border effects that draw on firm heterogeneity or input–output linkages that may give rise to magnification effects. Instead, the focus is on network effects, more precisely, the effects of ethno-linguistic networks. Further, the evidence from the pre-war Habsburg economy entails the opportunity to explore the possible emergence of significant border effects in the absence of *inter*-national barriers such as tariffs or major *intra*-national administrative barriers: we can trace the effects of the political borders along which the empire was split after 1918. These borders were, obviously, not in place during the period of analysis and so one can exclude the potential impact of unaccounted for administrative trade barriers that are likely to affect the results of other studies.

There are three key results. First, we confirm the findings of Good's (1984) research pointing towards increasing overall integration in Habsburg grain markets, but show that integration was systematically biased: while, there is no evidence of the post-war borders in Austria–Hungary's grain markets for the years 1878–1884, they become visible from the mid-1880s onwards. Second, the emergence and persistence of this 'border before a border' cannot be explained by changes in infrastructure, simple

9 Hungarian is not part of the Indoeuropean language family. We therefore set the index to zero for all pair-wise combinations with Hungarian, which tends to understate the ability to communicate between city populations. Similarly, we neglect the fact that many people will have been able to speak more than one language. This again leads us to underestimate the ability to communicate between city populations.

10 This helps also to explain the difference between our article and the finding of Wolf (2000) on large effects of state borders within the United States. While the degree of ethno-linguistic heterogeneity across US states is apparently too small to explain his finding, social and business networks, reaching back to the historical origins of the various US states might explain much more. Also, specification issues might affect the result in Wolf (2000), such as the neglect of multilateral resistance terms (Anderson and van Wincoop, 2003), estimation bias introduced by the occurrence of zero-trade flows (Santos et al., 2006), or the bias that is introduced by aggregating trade flows along state borders (Hillberry and Hummels, 2005). In contrast, none of these issues should affect our results.

geographical features, asymmetric integration with neighbouring regions, or just random effects. However, third, what does account for the emerging border effect is the extent of ethno-linguistic heterogeneity across regions and cities. While such heterogeneity does not appear to have mattered in the early years of the period studied, it became a force making for asymmetric intra-empire market integration in the later decades. This, the analysis suggests, was the outcome of two factors, in particular. First, as markets became more closely integrated as a result of declining transport costs, the *relative* importance of other non-distance related barriers to inter-regional exchange, such as ethnic or linguistic differences, increased. Second, the *absolute* importance of these differences rose with increasingly ethnically based forms of social and economic organization such as the trade co-operatives, especially from the mid-1880s. While the formation of ethno-linguistic networks entailed a lowering of information costs among members and helped diffuse common preferences, it probably also reduced the extent of exchange with non-network members. We can show that our results are not driven by difficulties in communication between ethno-lingyistic groups, but rather by increasing conflict between them. Thus asymmetric market integration was likely driven by both trade-creating and trade-diverting effects. This article shows empirically that the presence and strength of ethno-linguistic networks between cities can indeed explain the emerging 'border effect'.

These findings raise several issues. More generally, we suggest that the persistence of network effects within an environment of overall declining transport costs can give rise to economic 'borders' and may well play a major role in shaping trade patterns by their differential impact on trade costs. Next, explaining such trade patterns will, therefore, require a clearer understanding of the economics of network formation at both the national and international levels. Finally, from a historical perspective, the largely politically motivated re-drawing of the map of Central-Eastern Europe in the aftermath of the First World War may have had less of an adverse impact as thought so far due to the extent that regional integration patterns along the lines of the post-war borders began to emerge already before the war.

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