

Chilosi, David; Nikolić, Stefan

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Vanishing borders: ethnicity and trade costs at the
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David Chilosì,
King's College London

Stefan Nikolić,
Bocconi University

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David Chilosì,
King's College London

Stefan Nikolić¹,
Bocconi University

Abstract

This article exploits the creation of a paradigmatic multi-ethnic state, Yugoslavia, to examine if, to what extent, and why the effect of ethnic ties on trade costs changes over time. We compile and examine a panel of over 550,000 inter-urban price gaps spanning the area of Yugoslavia in the decades before and after the Yugoslav unification of 1918. Controlling for observable trade costs, we find that crossing the border between Serbia and Austria-Hungary significantly increased price gaps before the First World War. Ethno-religious differences explained a large share of this border effect in pre-unification Yugoslavia, but their influence vanished over time. This decline began about twelve years before the unification, and is visible both in city-pairs that were separated by the pre-war border and in those that were not. These patterns suggest that nation-building, rather than a weakening incentive to rely on private order institutions, was the main unifying factor.

JEL Codes: F14, F15, F52, N7, N9, Z12, Z13

Keywords: border effect, ethnicity, market integration, nation-building, trade costs

¹Corresponding Author: Stefan Nikolić (stefan.nikolic@unibocconi.it)

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1 Introduction

Does ethnic diversity explain the “border effect”? Do the effects of ethnicity on trade costs persist also after borders change? Why? The border effect is an old puzzle in trade economics: crossing a border increases price gaps and reduces trade by much more than would be expected on the basis of observable determinants of trade costs (Engel and Rogers, 1996; McCallum, 1995). The recent “cultural turn” in economics offers a potential solution: ethnic diversity undermines trust and increases transaction costs (Aker et al., 2014; Felbermayr and Toubal, 2010; Fenske and Kala, 2021; Gokmen, 2017; Guiso et al., 2009; Robinson, 2016; Schulze and Wolf, 2009, 2012).¹ While there is growing evidence that ethnic diversity can explain a large part of the border effect, we know much less on whether ethnic differences persistently increase trade costs after borders change and on possible underlying mechanisms. In this article, we aim to fill this knowledge gap. We exploit the creation of a paradigmatic multi-ethnic state, Yugoslavia, and the associated border changes, to examine the inter-temporal effect of ethno-religious differences on trade costs and their determinants.

We compile a panel of over 550,000 inter-urban price gaps spanning the area of Yugoslavia in the decades before and after the Yugoslav unification of 1918. As Figure 1 illustrates, the sample includes city-pairs that, before the First World War, were separated by the Austro-Hungarian border (treatment group) and those that were not (control group). The inclusion of pre- and post-unification data makes for a clean identification of the border effect and distinguishes this study from most of the literature on borders, which typically relies on cross-sectional estimation. A panel set-up also allows us to track how the effect of ethno-religious similarity on price gaps changed over time. We compare the evolving role of ethno-religious similarity in treatment and control groups of city-pairs. Our dataset thus allows gaining unique insights into the interaction between political changes and the evolving role of ethno-religious ties in trade.

¹In a related body of work, scholars have looked at how the presence of ethnic minorities enable international trade with their home country by addressing problems of asymmetric information and contract enforcement in an environment with a weak protection of property rights (Gokmen et al., 2018; Parsons and Vézina, 2018; Rauch and Trindade, 2002).

Controlling for observable trade costs (transport costs and import tariffs), we find that crossing the border between Serbia and Austria-Hungary significantly increased price gaps before the First World War. The size of this border effect was close to those found by other studies that use price gaps to estimate trade costs in the present day (Brenton et al., 2014; Broda and Weinstein, 2008; Versailles, 2012). These type of estimates based on inter-city price data do not suffer from the negative aggregation bias present in estimates based on gravity models and international trade data (Coughlin and Novy, forthcoming). Yet our results are very much in line with those of Anderson and Van Wincoop (2003), who use a structural gravity model to revise downwards the size of the border effects estimated with naïve gravity models. We also find that ethno-religious differences explained a large share – 30% or more – of the border effect in pre-unification Yugoslavia, but their influence vanished over time. These results are robust across grains, the inclusion of additional controls, and the use of a battery of measures of ethnic differences.

Innovatively, our research design enables us to systematically explore the mechanism behind the changing role of religion. Two hypotheses predict a declining role for religion in trade in our context. First, the formation of Yugoslavia weakened the incentive to rely on private order institutions to assist trade between cities that were previously separated by an international border. Second, Yugoslav nation-building fostered a pan-Slavic ethnic identity based on common language and transcending religious differences. While the two hypotheses are not entirely mutually exclusive, they have different implications for the diffusion and timing of the changes. The first, demand-driven, hypothesis would predict that agents change their behaviour only in the treated city-pairs, which were separated by the pre-war border and became part of the same state after the unification of Yugoslavia. The second, supply-driven, hypothesis is consistent with behavioural changes across both the treated and the control groups of city-pairs already before unification, since a phase of mass mobilisation typically predates the formation of a new nation-state (Hroch, 2000). We observe that the effect of religious similarity began declining about twelve years before the unification of Yugoslavia. The decline is visible both in city-pairs that were separated by the Austro-Hungarian border and in those that were not. This pattern supports the

nation-building hypothesis: southern Slavs of different religions began to form a common national identity already before the formation of Yugoslavia.

Our findings have implications for debates in the economics of ethnicity and culture. Fixed ethnic boundaries agree with the idea that a defining feature of culture is its historical persistence, which has been a central assumption in shaping the research agenda of economists working on culture (Guiso et al., 2006). By contrast, recent work on social identity stresses that individuals can switch group-identification swiftly, depending on rapidly shifting perceptions of cultural similarity and group status (Atkin et al., 2021; Sambanis and Shayo, 2013). While the international trade literature has so far stressed how ethnic ties tend to be persistent or only slowly changing (Beestermöller and Rauch, 2018; Guiso et al., 2009; Head et al., 2010; Schulze and Wolf, 2009, 2012), we find that nationalism swiftly affected patterns of group identification. This finding is at odds with the assumption of fixed ethnic boundaries and fits neatly with the results of the literature on social identity. In other words, our results suggest that the appeal of specific ethnic ties, such as Serbian or Croatian ties, ebbs and flows, rather than being transmitted “*fairly unchanged from generation to generation*” (Guiso et al., 2006, p. 23; emphasis in original).

Our article contributes to the literature on ethnicity and international trade by casting a new light on the causes of changes in the commercial role of ethnic ties. Several works have noticed how the role of ethnic ties in trade can change across space and time (Gokmen, 2017; Gokmen et al., 2018; Jha, 2013; Richman, 2017; Schulze and Wolf, 2009). To explain this pattern, most scholars (Carrère and Masood, 2018; Head et al., 2010; Jha, 2013; Richman, 2017) stress that the utility of ethnic ties depends on context. Shared ethnicity can be, for instance, more beneficial in international trade than in domestic trade (Richman, 2017), where agents can rely on public order institutions (Greif, 2000). Our findings are consistent with this idea. However, they suggest that the key mechanism is another one, with social and political elites playing the crucial role. This is not because, as Gokmen (2017) argues, state policy has the potential to repress the natural tendency that people have to trade with ethnically similar groups. Rather, it is because elites can influence patterns of ethnic identification.

Our results contribute also to the burgeoning economic literature on nation-building, by redressing the previous neglect of its impact on economic variables. Recently, scholars have examined how and why political elites seek to influence patterns of ethnic identity of the population with policies of nation-building (Alesina et al., 2021, 2020; Bandiera et al., 2019). Econometric investigations have also looked at the effects, showing that nation-building policies can foster the formation of inclusive national identities and inter-ethnic marriage and cooperation (Bazzi et al., 2019; Depetris-Chauvin et al., 2020; Kersting and Wolf, 2019; Kukic, 2019; Miguel, 2004). Economic variables, however, have received scant attention. We follow in the footsteps of Schulze and Wolf (2009, 2012), who stress that nationalism can directly affect economic variables like trade costs, even before nation-states are formed. We take this argument a step further by showing that integration along national lines progressed even across customs borders. Crucially, we map out the process of integration also after the formation of the new nation-state, detecting an intensification of the effects of nation-building.²

2 Historical background

2.1 Political borders

For centuries South Slavic peoples lived in Central and South-Eastern Europe under Habsburg or Ottoman imperial rule. Serbs and Montenegrins were alone to establish independent nation states before the turn of the twentieth century. Croatia and current-day Slovenia remained part of the dual monarchy of Austria-Hungary, which first occupied (1878) then annexed Bosnia-Herzegovina (1908). At the end of the First World War, Austria-Hungary collapsed. On 29 October 1918, the Yugoslav National Council in Zagreb declared independence from the monarchy and proclaimed the State of Slovenes, Croats, and Serbs. Only a month later, on 1 December 1918, the State joined Serbia and Montenegro to form the Kingdom of Serbs, Croats, and Slovenes. The new Kingdom was

²A corollary of this result is that, even if we confirm that trade links anticipated future border changes, we have to qualify the conclusion of Wolf et al. (2011) that the dissolution of the Habsburg Empire had little effect on grain trade.

proclaimed in Belgrade. Its name accommodated the three largest ethnic groups in the country. In 1929, the kingdom was officially renamed Yugoslavia, the name by which it was colloquially known since its inception.

The creation of Yugoslavia removed political and administrative borders that had previously separated its peoples and territories. Figure 1 maps the interwar borders, highlighting former Austro-Hungarian territories in grey. On the eve of the First World War, a 600 kilometres long border separated South Slavs living under different jurisdictions. To the North, Serbia bordered Croatia-Slavonia and Hungary. To the West, both Serbia and Montenegro shared a border with Bosnia-Herzegovina. In the South, a narrow border separated Montenegro from Dalmatia, a former Austrian province.

Serbia and Montenegro gained internationally recognised independence at the Congress of Berlin in 1878. In 1900, Serbia had 2.5 million inhabitants, of which 90% were Serbs.³ Another two million or so Serbs lived across the border in Austria-Hungary. With a population of 200 thousand, Montenegro was tiny in comparison to Serbia. In the Balkan Wars of 1912 and 1913, the Ottoman Empire was driven out of Europe, and Serbia expanded its territory southwards to include Kosovo and northern Macedonia. At the same time, Serbia and Montenegro partitioned the administrative district Sanjak of Novi Pazar. The new Serbian territories, known as South Serbia, were a multi-ethnic region inhabited by both Christian and Muslim populations.

Around 1900, approximately 2.9 million Croats lived in Austria-Hungary, mostly in the Kingdom of Croatia-Slavonia, which enjoyed autonomy within imperial Hungary, and in the Austrian provinces of Dalmatia and Istria. Croats also lived in southern Hungary and Bosnia-Herzegovina, which constituted a separate part of the empire jointly administered by Austria and Hungary. In 1900, it comprised 1.3 million South Slavs, of which 43% were Orthodox-Christians, 35% Muslim, and 21% Roman-Catholic. Some 1.3 million Slovenes, the third largest ethnic group of the future Yugoslavia, were clustered in several Austrian crown lands, most of which were ceded to the Yugoslav state in 1918.

³Population figures quoted in this section are from Calic (2019, pp. 3-7).

2.2 Ethnic relations

South Slavs are linked by their language and cultural kinship. Since the early nineteenth century, intellectuals and social elites thought that it would be possible to create a united South Slav nation based on shared descent, language, and culture. It took a century, however, for the idea to gain mass following. In the meantime, the Yugoslav idea had to compete against Croatian and Serbian nationalism and was repressed by the Austro-Hungarian Empire.

The Yugoslav idea began to evolve among Croat intellectuals and aristocrats.⁴ Its origins can be traced back to the 1830s and the Illyrian movement, which adopted an early-modern view that Croats, Serbs, and Slovenes originated from an antique people, the Illyrians, and propagated the vision of all South Slavs as a single nation, since they spoke the same language. They supported the Vienna Literary Agreement (1850) that standardised the Serbo-Croatian language based on the *Štokavian* dialect spoken by most Croats and all Serbs, Montenegrins and Bosnians. In the 1860s, Illyrianism gave way to Yugoslavism. Josip Juraj Strossmayer, a Croatian Catholic bishop, and Franjo Rački, a Croat-German liberal politician and historian, led the new movement. The Yugoslavists' manifesto, *Jugoslovjenstvo*, claimed that Croats, Serbs and possibly Slovenes, despite their historical conditions and different religions, formed part of a single primordial nation, united by common descent and shared history. Yugoslavists founded the Yugoslav Academy of Sciences and Arts in Zagreb aiming to make it the central scientific and artistic institution of all South Slavs.

Yet there were other unifying ideas, Croatian and Serbian.⁵ While the Yugoslavists called for the formation and autonomy of the “Triune Kingdom of Croatia, Slavonia, and Dalmatia”, Croatian nationalists demanded the creation of an exclusively Croatian nation state, outside of Austria-Hungary. They opposed the argument that South Slavic peoples had a national identity of their own and claimed that Serbs and Slovenes were essentially also Croats. Serbian nationalists envisioned an enlarged Serbia that would

⁴See, for example, Calic (2019, pp. 28-30), Lampe (2000, pp. 41-46), and Wachtel (1998, pp. 19-24) on the origins of the Yugoslav idea.

⁵See Calic (2019, pp. 30, 33-37), Lampe (2000, pp. 52-56, 58-65), and Wachtel (1998, pp. 24-31) on the history of South-Slavic, Croatian, and Serbian nationalism.

liberate territories under foreign rule, in which Serbs had lived. Many of them perceived independent Serbia as their “Piedmont” that had a potential to unify all Serbs in one country.

In the decade before the First World War, South Slavic ethnic relations entered a “new course”.⁶ The overthrow of the Obrenović dynasty in Serbia in 1903 and the creation of a popular government within a constitutional monarchy opened room for increased South-Slav cooperation. Politicians, academics, lawyers, bankers, entrepreneurs, and students supported the new course. In November 1905, Croat and Serb political parties agreed to close cooperation and a shared political programme. They won the majority of votes in the 1906 and 1908 elections in the Croatian-Slavonian diet. In 1909, the Yugoslav Academy in Zagreb, together with Serb and Slovene partners, commenced work on a “Yugoslav Encyclopedia”. In the same year, the first Pan-Yugoslav conference of South-Slav socialists took place. The press spread the winds of change and newspapers names like “New Century”, “New Age”, “New World”, and “Change” captured the spirit of the time. Social mobilisation was evident in the founding of civil society organisations, charities, reading groups, choirs, athletic clubs, professional, youth, and women associations. The athletic Sokol (meaning falcon) movement promoted the idea of South Slavic unity and became the backbone of Yugoslavism.

As South-Slav cooperation increased, Vienna and Budapest went on the offensive to solve the “South-Slav question” in their favour. In 1906, Austria-Hungary made an unsuccessful attempt to economically subdue Serbia through an unfavourable trade agreement (Djordjević, 1962). In 1908, the Austro-Hungarian annexation of Bosnia-Herzegovina made the thirty year old occupation permanent, which angered many South-Slavs within and beyond Austria-Hungary. In 1909, Habsburg prosecutors falsely accused Serbo-Croat political coalition members of treason presenting forged evidence (Calic, 2019, pp. 40-46). These Austro-Hungarian actions further fuelled the cooperation amongst South-Slavs who saw the dual monarchy increasingly as a common threat.

Defeat in the First World War sealed the fate of the Austro-Hungarian Empire. The

⁶See Calic (2019, pp. 38-45) and Wachtel (1998, pp. 53-63) for detailed discussions.

nationalist movements that had sprang in its midst, including the South Slavic one, took the opportunity to form new nation-states, giving to their success the appearance of inevitability. In reality, Yugoslav identity was work in progress (Wachtel, 1998, p. 68).

In interwar Yugoslavia, both the state and independent intellectuals played a role in nation-building (Wachtel, 1998, p. 94). The works of intellectuals highlighted the unity of Yugoslav literature and trickled-down to state approved school textbooks, which attempted to build, or even prove, the existence of Yugoslav national identity (Wachtel, 1998, p. 99). Pro-Yugoslav public intellectuals, such as the Croatian sculptor Ivan Meštrović or writer Ivo Andrić, made visible artistic contributions to Yugoslav cultural identity (Wachtel, 1998, pp. 108-127). Still, a major point of contention in the new Kingdom was its political organisation – mostly Serbian politicians wanted a strong central state, while those from the former Habsburg lands advocated federalism.⁷ While political compromise eluded interwar Yugoslavia (Djokić, 2007), even the most adamant federalists did not reject a common Yugoslav identity (Djokić, 2003, p. 144). Yugoslavia’s resurrection after the Second World War – following its occupation and partitioning by Axis powers during the war – testifies to the progress made on nation-building in the interwar period (Wachtel, 1998, p. 126).

2.3 Trade

Already before the First World War, future Yugoslav territories had engaged in significant trade. Serbia exported agricultural goods and imported Austrian and Hungarian manufactures. The majority of Serbian exports consisted of grains and livestock. Grains alone accounted for about a third of Serbian total exports in 1911 (Lampe and Jackson, 1982, p. 169). Physical and economic geography conditioned this trade. Landlocked Serbia could not reach distant markets via sea, but primarily traded with large, neighbouring Austria-Hungary. Specialising in agriculture, Serbia’s other neighbours had little demand for Serbian exports. From the 1870s until 1905, no less than 86% of Serbian exports went

⁷With a narrow margin, parliamentary representatives voted in favour of the constitution that, in 1921, established a centralised state under the motto of “one nation, one king, one state” (Calic, 2019, p. 74).

to the Habsburg Empire (Lampe and Jackson, 1982, p. 174). Official trade statistics report that, during this period, Austria-Hungary imported a steadily increasing volume of grain from Serbia.

Since 1881, ten-year bilateral treaties regulated Austro-Hungarian and Serbian trade relations. After several short-term extensions, the 1892 bilateral treaty expired in March 1906, and a provisional four-month agreement increased tariffs on trade. Since the two parties failed to reach an agreement by June 1906 now the even less favourable Austrian general tariff applied to Serbian exports. Negotiations reopened and an agreement was reached in August 1908, but the crisis over the annexation of Bosnia-Herzegovina prevented the treaty from being ratified. In July 1910, the parties reached a definitive trade agreement, which effectively re-established the terms of the 1908 agreement and kept tariffs relatively high until the First World War. This dispute over a new long-term trade agreement later came to be known as the Customs War.⁸ During the conflict, trade between Austria-Hungary and Serbia did not cease. From 1906 to 1910, on average, Austria-Hungary still accounted for 28% of Serbian exports, and already in 1911 the export share increased to 41% (Lampe and Jackson, 1982, p. 174).

3 Data

3.1 The sample

To measure market integration, we use a well-trying approach and look at price ratios between cities (Federico, 2012). The rationale is that price gaps cannot exceed trade costs for very long, as they give rise to arbitrage opportunities when they do. Specifically, we consider wholesale grain markets, which are particularly well-documented in the sources. The sample includes grain prices from a total of 173 cities in the years 1889-1913 and 1922-1928.⁹ The spatial distribution of the sample cities, illustrated in Figure 1, entails

⁸See Djordjević (1962) for a thorough history of the tariff conflict.

⁹There are data for 134 cities in the pre-war period, and 66 cities in the interwar. For 27 cities we have data covering both the pre-war and the interwar. We start about a quarter of a century before the war at around the time one of first censuses collecting ethnic data in our area was published. This is also early enough to have sufficient observations to identify effects of political and ethnic borders. Data

broad geographical coverage of whole of Yugoslavia. Across the pre-war and interwar periods, our sample is more or less evenly split between cities that had been part of the Austro-Hungarian Empire (68% and 56% of the sample, respectively) and cities that had been part of the Kingdom of Serbia (32% and 44% of the sample, respectively).¹⁰

Table 1 reports sample summary statistics. Our dataset is nearly evenly split between treatment group city-pairs that were divided by the Austro-Hungarian border and control group city-pairs that this border never divided. As expected, price ratios were higher on average in the treated city-pairs than in the control ones, as the latter, as well as never facing a border and the associated tariffs, were also better connected by transport. The means show that the control group city-pairs were significantly more religiously similar than the treated ones, but the standard deviations show that the variation of the measure was no less marked.

3.2 Grain prices

Grains are relatively homogenous commodities, and thus, in contrast to manufactures, the comparison of their prices suffers from relatively little noise. Another implication of homogeneity is that marked differences can be expected in the role of ethnic networks in domestic and international trade. On the one hand, information asymmetries that cut across these two types of trade are not particularly relevant for grains, which had known “reference prices” attached to easy to identify qualities. On the other hand, ethnic networks could still potentially mitigate obstacles to credible commitment to respecting trade contracts and agreements, which were comparatively relevant for international trade. In addition, as stressed also by Schulze and Wolf (2009, p. 118), since grains are primary products, their prices are unaffected by difficult to observe input-output linkages which can contribute to creating border effects.

We collect annual current wholesale price data for six types of grain (wheat, corn, rye, spelt, barley, oats) from period official statistics, published by Austrian, Hungarian,

collection was interrupted during the war and the first years of the new kingdom. We end before the Great Depression and the introduction of price controls (Pertot, 1937).

¹⁰While there are a few cities in the sample that were part of Montenegro before the war, we only have grain data from them after the war.

Croatian-Slavonian, Serbian, and Yugoslav authorities.¹¹ We convert all prices into dinar per 100 kg to ensure comparability both in the cross-section and over time. In the original sources, grain prices for cities in pre-war Serbia and all cities in Yugoslavia are given in dinar per 100 kg, and in cities in Croatia-Slavonia, Dalmatia, and Slovenia in crowns per quintal (a quintal is equivalent to 100 kg). We convert prices in crowns to dinars using exchange rates from the SEEMHN (2014) database.

3.3 Ethnicity

Our baseline measure of ethnic similarity between cities is standard: the likelihood that two random individuals from the two cities belong to the same group (Alesina and La Ferrara, 2005). While much of the literature on the economics of ethnicity focuses on linguistic variables to define ethnic groups, here we look at religions.¹² In our context there was relatively little linguistic variation across cities: in our sample, the mother tongue of an average of 80% of the people was Serbo-Croatian.¹³ Religion, rather than language, was the key marker of ethnic boundaries between Serbs (Christian-Orthodox), Croats and Slovenians (Catholic), and Bosnians (Muslim).¹⁴ As Calic (2019, p. 99), one of the most renowned international historians of Yugoslavia, writes “religious affiliation [...] was [...] the most distinctive criteria to demarcate between the various ethnic collectives”.

Data support this claim. Hungarian pre-war censuses differentiated between Serbs and Croats (Austrian censuses reported them jointly). For 89 sample cities in Croatia-Slavonia and Vojvodina, in 1910, the correlation is 99% between the share of Serbian and Orthodox

¹¹We provide details on sources in the appendix.

¹²Nikolić (2017) and Miladinović (2020) study the effect of religious similarity on price gaps in interwar Yugoslavia. While Nikolić (2017) does not find a significant effect of religious similarity, Miladinović (2020) finds a significant effect after 1929 and connects it to rising Serbian and Croatian nationalism. These findings are not necessarily inconsistent with our results, given that their samples include the 1930s and ours does not. Yet serious concerns can be raised on both previous studies. The authors rely on a small, ten market sample of noisy retail prices. Unlike wholesale prices, retail prices introduce noise by including consumer taxes and retailer mark-ups. Crucially, these studies neglect to consider that in the 1930s wheat prices were controlled by the state (Pertot, 1937) and hence were not determined by market forces as the underlying models implicitly assume.

¹³It is probable that many Slovenes as well as some minorities could speak Serbo-Croatian as a second language, which means that the share of population that was able to communicate in this language was even higher.

¹⁴According to the Yugoslav population census of 1921, approximately 47% of the population was Christian-Orthodox, 40% was Catholic, and 11% was Muslim.

population, and 85% between the share of Croat and Catholic population. This is strong empirical evidence that religion is a good proxy to differentiate between the main ethnic groups in Yugoslavia.

Religion has also the advantage that it arguably allows a cleaner identification of the ethnic effect on trade costs than language, as it is not influenced by the communication costs implied by different languages (Melitz, 2008; Melitz and Toubal, 2014). Nevertheless, we also look at linguistic similarity in one of the robustness checks. While relying on religion implies that secularism rather than nation-building per se may alter measured relationships, we do not expect this to be an issue in our context. Interwar Yugoslavia was a multi-confessional rather than secular state. This is confirmed also by the fact that the censuses detect patterns of religious identification across cities that were very stable over time, with hardly anybody identifying as an atheist, either before or after the war. One of the robustness checks uses provincial boundaries (Serbia, Croatia-Slavonia, etc.) instead of religious affiliation to identify ethnic groups and finds that the results do not change.

We collect data on religion and language from population censuses. For the interwar period, we use data from the Yugoslav population census of 1921. Figure 2 illustrates the religious composition of sample cities in 1921. For the pre-war, we use Austrian (1890-1910) and Hungarian (1900-1910) censuses for cities in Slovenia, Dalmatia, and Croatia-Slavonia. There is hardly any within variation: the sample correlation coefficient between the 1900 and 1910 censuses is 99.9% for religion and 99.8% language. Hence, we interpolate between census years and extrapolate with the data from the closest year otherwise. Serbian censuses only distinguish the Christian-Orthodox religion and Serbian language from “others” for most cities and offer a refined religious distinction only for 17 county capitals in 1910. A comparison of the observed religious shares and language data in 1921 and in the 1910 census reveals that there was hardly any variation over time.¹⁵ Therefore, for cities in Serbia, we hold the 1921 religious shares constant (both for the pre-war and the subsequent years).

¹⁵For instance, the correlation coefficient of the religious shares in 1910 and 1921 in the 17 county capitals with refined data is 99.8%.

3.4 Transport costs

To correctly identify border and ethnic effects it is essential to control for important time-variant confounding variables, which in our setting are transport costs and tariffs. To estimate transport costs, we rely on a least cost pathways approach. In recent years, this approach has become the gold standard in empirical works in trade economics (Allen and Arkolakis, 2019; Donaldson and Hornbeck, 2016). Using geographic information system (GIS) software and historical information, we create a new dataset on predicted city-pair freight transport costs. In short, we first construct a multi-modal transport network database that maps where freight could move along each transport mode (railway, waterway, and ordinary road). We then calculate transport cost parameters that apply to a given unit length of each transport mode. Lastly, we compute lowest-cost freight routes along the network for given cost parameters.¹⁶ Figure 3 presents the evolution of mean transport costs between sample city-pairs. As a result of opening of new railway lines, the cost of transporting goods fell over time.

3.5 Trade tariffs

We control for Austro-Hungarian tariffs on grain imported from Serbia. As mentioned before, cities in our pre-war sample are located either in Austria-Hungary or in Serbia. Grain trade between these countries essentially operated in one direction: Serbia exported grains to Austria-Hungary. This requires us to control for tariffs imposed by Austria-Hungary on imports from Serbia. We collect grain-specific tariffs from all tariff treaties signed between Austria-Hungary and Serbia between 1882 and 1910. Figure 4 shows the evolution of Austro-Hungarian tariffs on Serbian grains. For most of the period (1889-1905), tariffs were rather modest: on average, in our sample they were equivalent to 4% of the grain price in Serbia. With the outbreak of the customs war between Austria-Hungary and Serbia (1906) tariffs shot up and stayed relatively high until the First World War. During this time tariffs were on average equivalent to 37% of the price of Serbian grain. In Yugoslavia, by 1922, there were no internal tariffs on trade (Nikolić, 2017, p. 65).

¹⁶We discuss the computation of transport costs in more detail in the appendix.

4 Empirical specification and results

4.1 The border effect

How large was the border effect between Serbia and the Austro-Hungarian Empire? To begin with, we compare trends in price dispersion between city-pairs that were divided by the border (treatment group) with city-pairs that were not divided by the border (control group). As the panels are unbalanced, the trends are estimated with city-pair/grain fixed-effects panel regressions over year dummies. In other words, we estimate separately the following equation for the treatment and control groups of city-pairs:

$$\left| \ln\left(\frac{p_{igt}}{p_{jgt}}\right) \right| = \alpha_{ijg} + \sum_{t=1889}^{1927} \beta_t D_t + \varepsilon_{ijgt} \quad (1)$$

Where p_{igt} and p_{jgt} are the prices of grain g in cities i and j in year t , and each D_t is a dummy equal to one in year t .

The patterns shown in Figure 5 are clear. There was hardly any change between city-pairs that remained in the same state. By contrast there was a large drop, by around 20%, in price dispersion between city-pairs that were no longer divided by a border after the war. In consequence, average price ratios in both sets of city-pairs became essentially the same, hovering around 120%. In passing, we notice that these trends sit uneasily with a persistent role of ethnicity in segmenting markets. They can only be consistent with an important role for ethnicity in explaining the border effect if this role changed over time. In fact, even factoring in exchange rate risk and other administrative obstacles to international trade, persistent ethnic boundaries should imply that significant differences between the treated and control samples remain after the war, whereas we find that they had vanished. We shall return to this issue in Section 4.2.

Before the war, price dispersion between treated city-pairs was rather volatile, and there is little evidence of a consistent pre-treatment trend. To precisely measure the border effect, however, we need to control for the fact that part of the price dispersion between cities across the border was caused by tariffs. Moreover, on average, city-pairs in the treatment group were more distant from one another than city-pairs in the control

group (394 km vs. 177 km). Thus, the development of railway lines is expected to reduce transport costs between cities initially divided by a border more than for the others. We therefore estimate the border effect controlling for transport costs and tariffs. As usual in the market integration literature (Brenton et al., 2014), we also include grain/cities fixed effects, which control for time-invariant characteristics affecting market access and local grain prices, such as altitude or land quality, as well as grain/year fixed effects aimed at capturing other factors behind changes in price dispersion over time, such as improved communication in the wake of the spread of telephone and telegraph lines. Specifically, we estimate the following equation:

$$\begin{aligned} \left| \ln\left(\frac{P_{igt}}{P_{jgt}}\right) \right| = & \alpha_{ig} + \alpha_{jg} + \tau_{gt} + \beta_1 \textit{Border}_{ijt} + \beta_2 \ln(\textit{Tariff factor}_{ijgt} + 1) \\ & + \beta_3 \ln(\textit{Transport cost factor}_{ijgt}) + \varepsilon_{ijgt} \end{aligned} \quad (2)$$

Where \textit{Border}_{ijt} is 1 if cities i, j were separated by the Austro-Hungarian border at time t , and 0 otherwise. $\textit{Tariff factor}_{ijgt}$ are grain specific trade tariffs on exports from Serbia, where the average price was 45% lower, divided by the grain price at origin. $\textit{Transport cost factor}_{ijgt}$ are transport costs divided by the grain price at origin (the lower price). In the absence of a consumption price index, nominal transport costs are computed by reflating the real transport costs with the trend of grain prices. In turn, this trend is computed with a fixed effects panel regression of grain prices over year dummies.¹⁷ The normalisation of observable nominal costs by price of origin is needed to ensure that costs are measured as “iceberg costs” on both sides of the estimating equation. The variables α_{ig} and α_{jg} are city-grain dummies controlling for unobserved time-invariant factors like non-perfect homogeneity of products or differences in institutional quality across cities; τ_{gt} are year-grain dummies controlling for unobserved time changes, such as those stemming from weather shocks or improvements in the communication infrastructure that can potentially affect iceberg trade costs differently across grains.¹⁸ Lastly, ε_{ijgt} is

¹⁷In the pre-war years, there is a close match between our index and the Austro-Hungarian Consumer Price Index (SEEMHN, 2014): their correlation coefficient is 83%.

¹⁸The simultaneous use of cross-sectional and temporal fixed effects has recently come under attack.

the error term. Standard errors are clustered by city-pair/grain and allow for arbitrary correlation within the cross-sectional units.

Equilibrium price ratios are expected to be equal to trade costs between trading markets, but otherwise they can move at random until they become as wide as costs (Federico, 2012). Hence only trading city-pairs yield clean measures of trade cost, and the inclusion of other city-pairs in the analysis can cause attenuation bias in the estimated coefficients. Data on urban trade flows are hard to come by and, just as in other similar studies, observing inter-city trade is not feasible. We can, however, infer that trade will not be profitable between cities with price gaps lower than observable trade costs (i.e., transport costs plus tariffs). In what follows the sample which excludes such cities will be referred to as “restricted sample”.

Table 2 presents the baseline estimates of the border effect. All the variables have the expected sign and are highly significant. There is also a good fit, particularly with the restricted sample (column 2). There is some evidence of attenuation bias in the full sample (column 1) for the tariff and transport cost coefficients, but the border effect emerges as remarkably robust: it is only slightly lower in the restricted than in the full sample. The border coefficient corresponds to an increase in the average price ratio by 10 percentage points, a value that is consistent with the trends shown in Figure 5.¹⁹

Reassuringly, the sizes of our border coefficients are also very much in line with the majority of those found by other similar studies (Figure 6). Nevertheless, our coefficients are significantly higher than those estimated by Schulze and Wolf (2009) and by Aker et al. (2014). The latter looks rather low in comparative perspective, lending credence to Aker et al.’s (2014) claim that in their context, present-day Niger-Nigeria’s trade, bilateral price-gap understate the true extent of the border effect. Our coefficient is significantly higher than that found by Schulze and Wolf (2009) for provincial borders of the Austro-Hungarian Empire before the First World War. This difference suggests that borders between independent polities were higher than “internal borders”, whose effects

However, the emerging consensus is that studies where the treated group does not change over time are robust (De Chaisemartin and d’Haultfoeuille, 2020; Imai and Kim, 2020). This condition applies to our key treatments here, border and religious similarity.

¹⁹Equal to $[\exp(\beta_1)-1]$ times the average price ratio (1.297), where β_1 is the border coefficient.

did not include exchange rate risk and other administrative obstacles to international trade. Thus, internal borders only partially anticipated future trade barriers.

Assuming that the trade elasticity with respect to trade costs is -3.71 (the median value in Head and Mayer’s (2014, Table 5) survey of 32 econometric estimates), our border coefficients correspond to a decline in trade by 29 to 32%.²⁰ These values are very much in line with Anderson and van Wincoop’s (2003) downward revision of border effects estimated with naïve gravity models: for them, national borders reduce trade by between 20 and 50%.

4.2 The role of ethnicity

To what extent did the border effect reflect the different ethnic make-up of Serbian and Austro-Hungarian cities? As discussed before, in our context the most important marker of ethnic boundaries was religion, rather than language. Our measure of ethnic similarity is the likelihood that two random individuals from the two cities have the same religion. Formally:

$$\text{Religious similarity} = \sum_{k=1}^n (a_{i,t}^k * a_{j,t}^k) \quad (3)$$

Where $a_{i,t}^k$ and $a_{j,t}^k$ are the population shares of religion k in cities i and j in year t . Its size ranges from zero to one. Thus, the size of its coefficient can be directly compared to those found in other studies using dummies (Aker et al., 2014; Robinson, 2016).

We start to answer our question by looking at whether we find, like Schulze and Wolf (2009) do, that the border effect becomes smaller when we control for ethnicity. We subsequently relax the assumption that the effect of ethnicity on trade costs is time invariant in two ways. Firstly, we allow the effect of ethnicity to be different before and after the unification of Yugoslavia. This specification tests the hypothesis that the effect of ethnicity persists after political borders change. Secondly, we let ethnic similarity interact with time. This specification allows the effect of ethnicity to start varying even before

²⁰Equal to -3.71 times $[\exp(\beta_1)-1]$, where β_1 is the border coefficient. Ossa’s (2015) estimate of the trade elasticity for agricultural products is -3.44 and thus is very close to the value we selected.

political borders change. Otherwise put, we estimate the following equations:

$$\begin{aligned} |\ln(\frac{P_{igt}}{P_{jgt}})| &= \alpha_{ig} + \alpha_{jg} + \tau_{gt} + \beta_1 \textit{Border}_{ijt} + \beta_2 \ln(\textit{Tariff factor}_{ijgt} + 1) \\ &+ \beta_3 \ln(\textit{Transport cost factor}_{ijgt}) + \beta_4 \textit{Religious similarity}_{ijt} + \varepsilon_{ijgt} \end{aligned} \quad (4a)$$

$$\begin{aligned} |\ln(\frac{P_{igt}}{P_{jgt}})| &= \alpha_{ig} + \alpha_{jg} + \tau_{gt} + \beta_1 \textit{Border}_{ijt} + \beta_2 \ln(\textit{Tariff factor}_{ijgt} + 1) \\ &+ \beta_3 \ln(\textit{Transport cost factor}_{ijgt}) + \beta_4 \textit{Religious similarity}_{ijt} \\ &+ \beta_5 \textit{Religious similarity}_{ijt} * \textit{Yugoslavia}_t + \varepsilon_{ijgt} \end{aligned} \quad (4b)$$

$$\begin{aligned} |\ln(\frac{P_{igt}}{P_{jgt}})| &= \alpha_{ig} + \alpha_{jg} + \tau_{gt} + \beta_1 \textit{Border}_{ijt} + \beta_2 \ln(\textit{Tariff factor}_{ijgt} + 1) \\ &+ \beta_3 \ln(\textit{Transport cost factor}_{ijgt}) + \beta_4 \textit{Religious similarity}_{ijt} \\ &+ \beta_5 \textit{Religious similarity}_{ijt} * \textit{year}_t + \varepsilon_{ijgt} \end{aligned} \quad (4c)$$

Where $\textit{Yugoslavia}_t$ is a dummy equal to one after the unification of Yugoslavia, \textit{year}_t is the year and otherwise the notation is as before.

Table 3 presents the baseline results on the role of ethnicity. In columns 1 and 2, religious similarity has the expected sign and is statistically significant. Its sizes are in line but slightly lower than that of the analogous coefficient estimated by Schulze and Wolf (2009, Table 4, column 2), which has an average value of -0.066. The border coefficients do become lower than in Table 2, but not by very much, 15% or 23% with the full and restricted samples, respectively. As it turns out, however, this contribution is much too low because it neglects to consider that the effect of ethnicity varied over time.

For the following specification (columns 3 and 4), the values of the ethnicity coefficient before the unification of Yugoslavia increases significantly and become very similar to the just-mentioned estimates by Schulze and Wolf (2009, table 4, column 2).²¹ In addition, our interaction analysis soundly rejects the hypothesis that that there was a persistent effect

²¹Our absolute values are also significantly higher than those of the coefficients of places with the same ethnicity within present-day Malawi (-0.012) (Robinson, 2016) and within Niger or Nigeria (-0.010) but are lower than the coefficient for places across the Niger-Nigeria border (-0.085) (Aker et al., 2014, table 3, column 5).

of religious similarity: summing up the baseline and interaction coefficients imply that after the formation of Yugoslavia the sign of the coefficient had switched from negative (-0.050 or -0.066) to positive (0.047 or 0.059).²² This specification detects a much greater fall in the border coefficient than in the specification without interaction, with drops by 46% (full sample) or 70% (restricted sample), suggesting that ethnicity was indeed an important factor behind the border effect before the war.

The final specification (columns 5 and 6) confirms that market-segmenting effect of ethnic diversity vanished over time: in 1889 the coefficient of *Religious similarity* was -0.077 or -0.117, as compared to 0.012 or 0.035 in 1928. These are economically large changes: the corresponding effects on the average price ratio are -10 or -14 percentage points in 1889, compared to 2 or 5 percentage points in 1928. Furthermore, the crucial role played by ethnicity in explaining the border effect before the war is confirmed, albeit not as extremely as in the previous specification: its coefficient declines by 30% for the full sample and 54% for the arguably more reliable restricted sample, as compared to the estimated border effect without controlling for ethnicity (Table 2).

In short, we find consistent evidence that differences in the religious make-up of Serbian and Austro-Hungarian cities explain a large share of the border effect. However, the effect of religious diversity vanished over time and was no longer present by the time cities had become part of the same Yugoslav state.

4.3 The mechanism

On the one hand, if ethnicity only mattered when cities were in different countries because reputation was particularly important for international trade, then one would expect agents to change their behaviour only in the treated cities, which became part of the same state after the unification of Yugoslavia. If, on the other hand, following nation-building, ethnic identification changed over time and religious diversity no longer undermined trust, trade costs should change across both the treatment and control groups of city-pairs

²²If we let the effect of religious similarity vary across religious groups, we find that the same pattern, with religious similarity fostering market integration before but not after the unification of Yugoslavia, was followed by both main religious groups, Orthodox and Catholics, while the results are mixed for the small denominations. The results are not reproduced for reasons of space but are available upon request.

already before unification, as South-Slavic mobilisation gathered momentum. We start by looking at whether and to what extent the declining role of religious similarity was widespread by re-running the specification in columns 5 and 6 of Table 3 separately for the treatment and control groups of city-pairs.

Table 4 reports the results of this exercise. The results of the restricted sample (columns 2 and 4) suggest that supply was the main driver: they detect an initially important role for religious similarity in both the treatment and control samples, which vanishes over time at a similar pace. To be sure, the fitted value of the religious similarity coefficient in 1889 (equal to the baseline plus the interaction coefficient times 1889) are higher for the treated sample than for the control sample: -0.089 vs. -0.050 , consistent with a more important role for ethnic ties in international than in domestic trade. However, consistent with the nation-building hypothesis, there is hardly any difference in the total change to the coefficient in the two samples: 0.087 vs. 0.090 . The difference between treatment and control samples is more marked for the full sample (columns 1 and 3), which nevertheless confirms that the importance of religious similarity in promoting market integration declined also in the control sample.

Next, we look at timing of the transition, again using the same specification as in columns 5 and 6 of Table 3, but interacting religious similarity with year dummies, instead of year. The result of this test, presented in Figure 7, confirms a crucial role for nation-building. Both the full and the restricted samples agree that the turning point was from 1906. Even if agents were forward-looking, twelve years before the unification of Yugoslavia seems far too early for them to plausibly anticipate that they no longer needed to rely on ethnic ties to enter into credible contracts because they would be soon part of the same state.

5 Robustness checks

5.1 Results by grain

Our first robustness check looks at whether the results are driven by a specific grain (Table 5). To this aim we re-run the regression with all the baseline variables and that with the interaction between *Religious similarity* and *year* including only observations from one type of grain at a time. For reasons of space, for this and the following robustness checks, we present only the results with the restricted sample, which are expected to be particularly reliable.

Under the specifications without the interaction analysis (odd numbered columns), for all grains, all the coefficients have the expected sign and are nearly always highly significant. The only notable exception in this respect is the border effect when including only spelt price gaps. In this case the sample size is much smaller than for all the other grains and therefore the standard errors are bound to be comparatively large. For all grains, in the specifications with the interaction coefficient (even numbered columns), the two key results are confirmed: firstly, initially religious similarity fosters market integration, but this effect wanes over time; secondly, controlling for this dynamic implies that the border effect becomes much reduced, consistent with the hypothesis that the changing role of religious similarity was an important factor behind an initially powerful border effect and its eventual disappearance. Moreover, in the interaction analyses, too, all the controls have the expected sign and are highly significant, regardless of the grain.

5.2 Balanced and similar samples

Next we check the robustness of the results using other sub-samples (Table 6). Non-random missing observations in unbalanced samples can cause bias in the estimated coefficients (Baltagi, 2008). We have no reason to expect a survival bias in our sources that systematically biases the results in favour of our findings.²³ Nevertheless, for the sake

²³As a rule, authorities recorded prices in cities that they deemed the most important at the time. Authorities in both Serbia and Croatia-Slavonia collected prices in many cities before the First World War. Once these polities became part of Yugoslavia, some of their cities lost importance compared to

of robustness, we re-run the key specifications with a balanced panel, which includes only city-pairs for which we have observations in all the years between 1899 to 1910 and between 1922 and 1928 (the largest feasible balanced panel) (columns 1 and 2). In addition, we re-run the two same specifications also with two sub-samples of similar city-pairs. The first one includes only provincial capitals (based on the provincial division of Yugoslavia from 1918 to 1922) (columns 3 and 4). The second sample of similar city-pairs includes observations only from cities close to the border (within 50 km) between Austria-Hungary and Serbia (columns 5 and 6). The purpose of this exercise is twofold. Firstly, focusing on similar city-pairs minimises variations in unobservable confounders. Secondly, by looking at these two different samples, we can gain insights into whether there is evidence of heterogeneity in the dynamics that we observe across groups of cities.

The signs of the coefficients are once again all in agreement with our baseline results, with only a few exceptions: the average effect of religious similarity is positive according to two of the specifications (columns 1 and 3) and that of the border coefficient becomes negative in one specification with the interaction term (column 4). Nevertheless, these three coefficients are either insignificant or only barely significant. Again, we find significant reductions in the size of the border effect once we allow the effect of religious similarity to vary over time, although the reduction was less marked for the cities near the borders (column 6) than for the other two samples (columns 2 and 4). This difference is associated with a comparatively slow change in the effect of religious similarity in the sample of cities near the border. In all three samples, the effect of religious similarity was negative at the beginning of our period, in 1889. It became positive at the end of the period in 1928 for the balanced panel and the sample with provincial capitals. For the sample of cities close to the border by 1928 the sign was still negative, though its absolute value was smaller than at the beginning. This is not because near the border religious differences were particularly salient: on the contrary, for this sample the 1889 coefficient of religious similarity, -0.103, had a lower absolute value than for the balanced sample (-0.150) and especially than for the provincial capitals (-0.243). However, the pace of change in the

other cities in the new Kingdom. Thus, while Yugoslav authorities collected data for at least one city in each of its provinces, they reduced the coverage of cities in Serbia and Croatia-Slavonia.

effect of religious similarity was much slower for cities near the border than for the other two samples, as seen in the interaction coefficients. Cities near the border were particularly likely to engage in international trade and were thus particularly exposed to the effects of institutional changes following the unification of Yugoslavia. Provincial capitals, in contrast, were more exposed than other cities to the actions of competing elites, including nation-building. Hence a much faster decline in the market-segmenting effects of religious differences in the provincial capitals than in cities near borders corroborates our contention that the key mechanism at work was not changing incentives to rely on ethnic ties to facilitate contract enforcement, but nation-building.

5.3 “Dark trade costs” and pre-treatment trends

Our next robustness check looks at what happens to the key results when we include two additional controls (Table 7). In the international trade literature, traditionally distance is used as a proxy for transport costs. Recently, scholars like Head and Mayer (2013) have advocated a broader understanding of the effect of distance on trade costs, whereby much of it is due to so-called “dark trade costs”, i.e. unobservable impediments to trade stemming from information frictions. In our sample, there is a fairly strong negative association between religious similarity and distance, with a correlation coefficient of -0.59. Hence, one potential concern is that our religious similarity variable is picking up these effects, rather than those of ethno-religious similarity.²⁴ We therefore control for (log of) distance as well (columns 1-3).

One well-known threat to identification in difference-in-differences sets up, like this one, is represented by violations of the assumption that in the absence of a treatment the dependent variable of the treated and the control groups would have followed parallel trends. The standard way to address this concern is to allow the treatment and control samples to follow different trends (Angrist and Pischke, 2008). In columns 4 and 5, we

²⁴A related concern is that the falling influence of *Religious similarity* may be picking up differences in the effect of provincial borders between the Empire and Yugoslavia. However, our results are robust to the inclusion of controls for provincial borders, even if their effect is allowed to change after the war. The coefficients of *Religious similarity* and *Religious similarity*year*100* become -6.999*** and 0.364***, where *** denotes significance at the 1% level.

therefore add these trends to our key baseline specifications.

The main results are again confirmed. All the signs are consistent with the baseline specifications. Including distance leads to an only modest reduction in the average effect of religious similarity (column 1). In all specifications, allowing the effect of religious similarity to vary over time implies that this variable increased market integration at the beginning but not at the end of our period (the values of the religious similarity coefficient in 1889 range from -0.174 to -0.113, while those in 1928 range from 0.038 to 0.081). Taking this dynamic into account leads to a sharp reduction of the size of the border coefficient. If we allow the effect of distance as well to vary over time, we detect a fall in the effect of distance over time, which is consistent with the expectation that information frictions are greater across than within states. Indeed, under this specification the border effect disappears altogether. The trends turn out to be very similar for the treated and control samples, in spite of the fact that their signs change across specifications (columns 4 and 5).

5.4 Alternative measures of ethnic similarity

Our last battery of robustness checks uses alternative measures of ethnic similarity. In particular, firstly, following Schulze and Wolf (2009), we try an alternative measure of religious similarity, equal to one minus a measure of heterogeneity similar to the Herfindahl index:

$$\textit{Religious similarity (Herfindahl)} = 1 - \frac{1}{2} \sum_{k=1}^n (a_{i,t}^k - a_{j,t}^k)^2 \quad (5)$$

The notation is as before. Like the baseline measure, this variable ranges between zero (no similarity) and one (perfect match). However, there is an only imperfect correspondence between the intermediate values of the two variables. Their sample correlation coefficient is 93%. Secondly, as anticipated before, we also include a variable measuring linguistic similarity, again using the two alternative measures defined by equations (3) and (5), but applying them to shares of individuals belonging to linguistic rather than religious groups. Thirdly, we measure ethnic similarity with a dummy equal to one when two cities

belong to the same province. We code cities based on the provincial division of Yugoslavia from 1918 to 1922 into Serbia (consisting of North Serbia and South Serbia), Montenegro, Bosnia-Herzegovina, Dalmatia, Croatia-Slavonia, Slovenia, and Vojvodina (Banat, Bačka and Baranja). This provincial division is attractive as it corresponds closely both to the pre-war subdivisions of Yugoslavia's predecessor states and to the political borders of Yugoslav successor states. While this measure of ethnic similarity is inferior to our baseline measure since it neglects to consider ethnic heterogeneity within provinces and ethnic similarity across them, it has nevertheless the advantage of being able to capture more ethnic groups (e.g. Slovenians and Croatians shared the same Catholic religion and thus are considered as one single group by our baseline measure).

The results with the alternative measure of religious similarity (Table 8, columns 1, 3, 6 and 7) are quantitatively very similar and qualitatively identical to those obtained with the baseline measure. Using the dummy equal to one for city-pairs in the same province to capture ethnic similarity yields very similar results, too (columns 8 and 9). The only notable difference is that the average effect of ethnic similarity on price gaps is no longer negative. The addition of the linguistic variables (columns 2, 4 and 5) has no visible effect on the size of the religious similarity coefficients, unless the effect of the language variable is allowed to change over time, but also in such cases the qualitative results remain unaltered.

As anticipated, in the light of the widespread use of Serbo-Croat in our cities (section 3.4), there is little evidence that linguistic differences fragmented markets. On the contrary, on average, more linguistically similar city-pairs had greater price gaps than less similar ones. This may be just a statistical fluke: the size of the coefficients of the linguistic variables are significantly lower than those of the religious variables. Nevertheless, the positive sign could also capture comparatively developed trading infrastructure with places where linguistic minorities specialised in the production of sought after goods. As Montalvo and Reynal-Querol (2017) stress, gains from inter-ethnic trade increase with ethnic specialisation. Either way, the negative sign of the interaction coefficients implies that the size of this effect declined over time. The same remark applies to all other

measures of ethnic similarity: religious similarity or being part of the same province only favoured market integration in the early part of our period and no longer mattered towards the end. The interaction analyses from Table 7 are remarkably consistent in this respect: the range for the relevant coefficient in 1889 is -0.228 to -0.138; the same range for 1928 is 0.029 to 0.069. Moreover, once again, taking into account the vanishing effect of ethnic borders leads to a marked decrease in the size of the border coefficient. The proportional decrease, as compared to the border effect without ethnic controls (Table 2), is consistently large with a range of 51% to 120%.

6 Conclusion

This article has investigated the relationships between borders, ethnic diversity, and trade costs by focusing on three questions: can ethnic diversity explain the border effect? Does the impact of ethnicity on trade costs persist after borders change? Why? To answer these questions, we used a unique historical case: the formation of a paradigmatic multi-ethnic state, Yugoslavia. We compiled a very large panel of inter-urban price gaps spanning the period before and after Yugoslav political unification. We examined the effect of the Austro-Hungarian border on increasing inter-urban trade costs within the area of Yugoslavia, by comparing city-pairs that were separated by this border (treatment group) to those that were not (control group). We found that the border effect was statistically significant and similar in size to those found in other contexts. Religious similarity – the key marker of shared ethnicity in the Yugoslav context – accounted for a large share of this border effect. These results are robust across grains and other sub-samples, the inclusion of additional controls, and the use of a battery of measures of ethnic differences.

Through an innovative research design, we then explored the mechanism at work. We found that the drop in the relevance of religious similarity to explain price gaps was common to city-pairs in both the treated and control groups. This fall in the importance of religious differences began about twelve years before the unification of Yugoslavia. These patterns are consistent with the hypothesis that the idea of a Yugoslav nation, increasingly

promoted across broad social strata since the turn of the century, fostered the formation of a new ethnic identity with linguistic commonality trumping religious differences. The nation-building hypothesis is also corroborated by the fact that we observe a much faster decline in the market-segmenting effect of religion between the provincial capitals than between the cities close to the border. Our results provide new evidence on the effects of nation-building on economic behaviour. They highlight how ethnic identities can shift swiftly and challenge the common assumption in the economics of ethnicity that ethnic identities are historically persistent. Ethnicity matters, but it's not destiny.

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Appendix

A.1 Data sources

Grain prices come from the following sources:

Österreichisches Statistisches Handbuch [Austrian Statistical Handbook], 1894(-1913). Vienna, 1895–1915: Central Statistical Commission.

Mjesečni statistički izvještaji [Monthly Statistical Reports], 1889(-1913). Zagreb, 1889-1913: Royal State Statistical Office in Zagreb.

Magyar statisztikai évkönyv [Hungarian Statistical Yearbook], 1894(-1913). Budapest, 1895-1915: Royal Hungarian Central Statistical Office.

Državopis [Statebook], 1889. Belgrade, 1890: State Statistical Office.

Statistika cena poljoprivrednih proizvoda [Price statistics of agricultural goods], 1890-1895. Belgrade, 1897: State Statistical Office.

Statistički godišnjak [Statistical yearbook], 1893(-1910). Belgrade, 1895-1914: State Statistical Office.

Obradjena zemlja i žetveni prinosi [Farmland and crop yield], 1922-(1928). Belgrade, 1924-1931: Ministry of Agriculture.

Religious and linguistic shares are from the following population censuses:

Die Ergebnisse der Volkszählung vom 31. Dezember 1890 (1900, 1910) in den im Reichsrath vertretenen Königreichen und Ländern [The results of the census of December 31, 1890 (1900, 1910) in the Kingdoms and Countries represented in the Imperial Council]. Vienna, 1893 (1903, 1914): Central Statistical Commission.

A Magyar Szent Korona Országainak 1900 (1910) évi népszámlálása [The 1900 (1910) census of the Countries of the Hungarian Holy Crown]. Budapest, 1902 (1912): Royal Hungarian Central Statistical Office.

Definitivni rezultati popisa stanovništva [Definitive results of the population census], 1921. Belgrade, 1931: General State Statistics.

A.2 Transport costs estimation

We start by digitising historical maps of transport networks in Yugoslavia in 1922 (Deroko, 1922), 1926 (Deroko, 1926), and 1937 (Jošt, 1937). We “georeference” these maps to Yugoslav borders. We then trace railway and waterway lines using GIS software to create a digital map of network connections by rail, sea, navigable rivers and canals. Using information from Stehlik (1951) on the exact opening date of each railway, starting in 1937, we move backward and remove non-existent lines in preceding years to recreate railway connections for each year in our sample. Figure A.1 illustrates waterways and the evolution of railways for selected benchmark years between 1889 and 1928. Lastly, we connect cities to the rail and water network. Relying on the modern road network, we create ordinary road routes from the geographical centre (centroid) of each city to each nearby type of transport route. To compute distances, we take into account the topography of the terrain.

We calculate transport cost parameters on the relative costs of railway, water, and road transport based on good specific railway and waterway transport tariffs, and archival data on road transport costs. We collect railway and waterway tariffs from official publications of railway and waterway authorities. In both cases tariff data are for transport of 100 kg of grains. The original sources classify railway tariffs by distance classes, and waterway tariffs by connections between all 64 river ports in Yugoslavia. Based on our network database, we estimate the kilometre distance between each pair of these river ports. We use these river distance estimates to classify waterway tariffs into distance classes following the railway tariff classification.

We draw on archival data and our network database to estimate ordinary road transport cost. On the request of the Ministry of Trade and Industry, in 1925 and 1926, local authorities reported estimates of road transport costs. Reports specified the cost in dinar of transporting 100 kg of unspecified goods by ordinary road from a given city to the nearest railway or waterway station, including the name of the station, but not the distance from the city to this station. We rely on our network database to estimate distances from the cities that reported transport costs and were not part of the railway

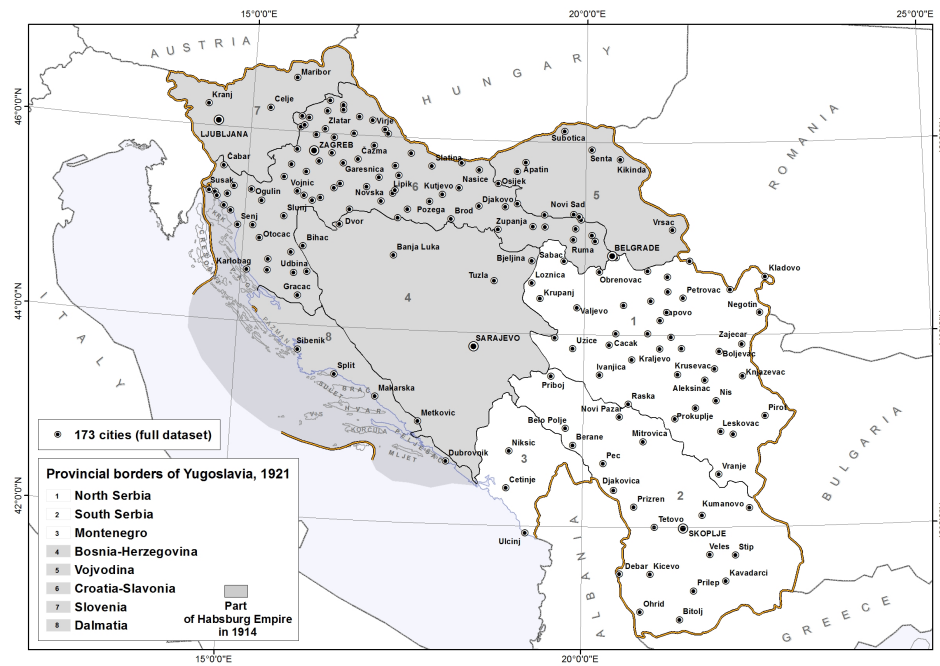
or waterway network (for cities located on the railway or waterway network transport distance, and hence transport costs, by road are essentially zero). Dividing reported transport costs by estimated distances we obtain an estimate of the cost of ordinary road transport in dinar per kilometre.

Applying these transport cost parameters, we calculate the relative costs of railway, waterway, and ordinary road transport by distance classes. At the city-pair distance of 100 km, for example, transport by water costs half as much as transport by rail, while transport by road is approximately 30 times more expensive than transport by rail. These relative transport costs estimates are reassuringly similar to the ones reported by Donaldson and Hornbeck (2016) for late nineteenth century U.S. As in their case, freight rates are held constant across time. By contrast, thanks to our detailed railway and waterway tariff data, our transport cost relatives vary across distance classes.

In a final step we predict city-pair freight transport costs. We employ the complete network database and relative transport costs to calculate the lowest-cost route between each pair of cities in kilometres and then multiply these estimates with appropriate transport costs. As a result we get time-varying city-pair freight transport costs. As a robustness check, we also calculate transport costs for shortest routes. Lowest cost routes are always equal or longer than shortest routes, but never more expensive, thus confirming the robustness of our calculations.

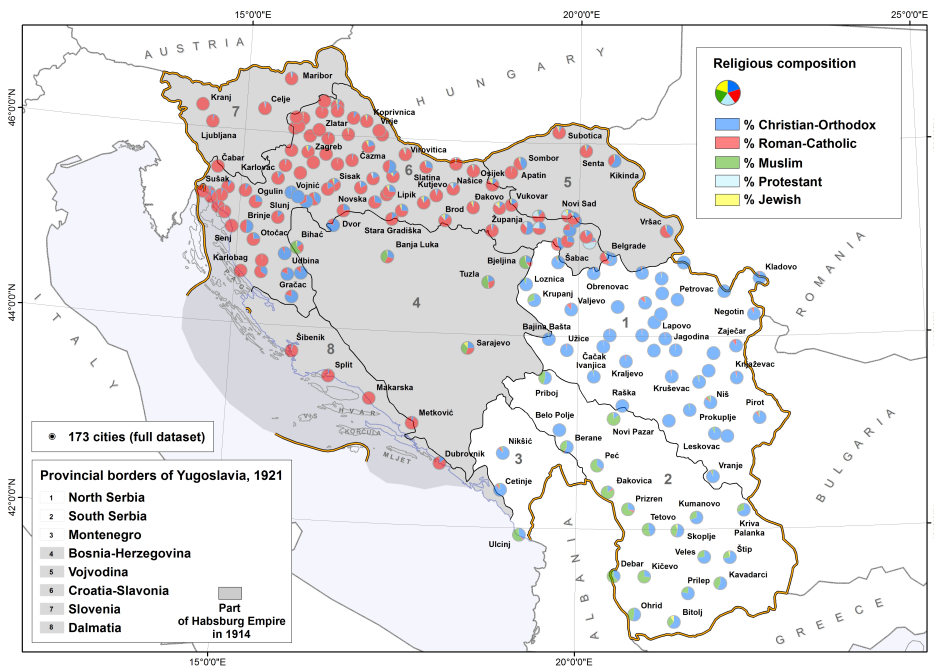
Figures

Figure 1: Map of interwar Yugoslavia, illustrating sample cities and the pre-war border



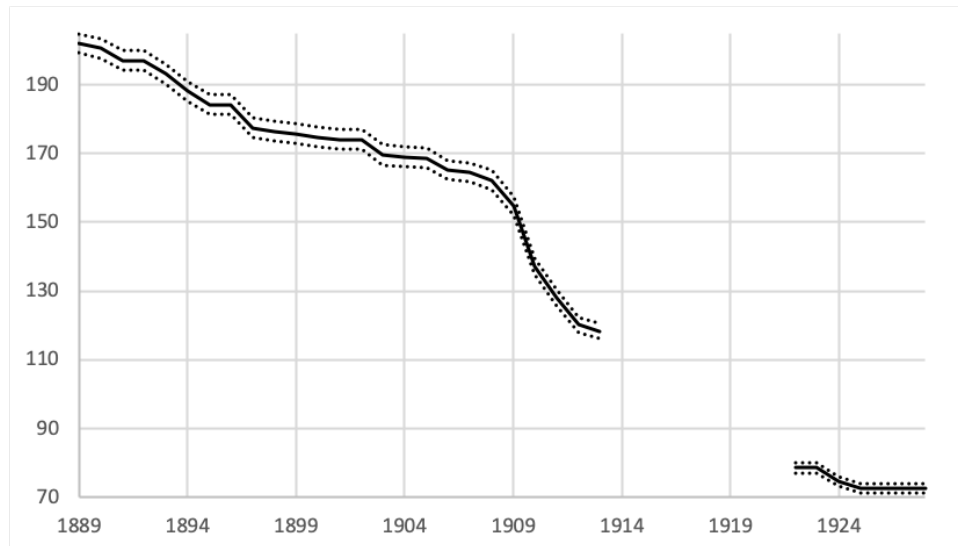
Sources: own illustration. See the text for details.

Figure 2: Religious composition of sample cities in 1921



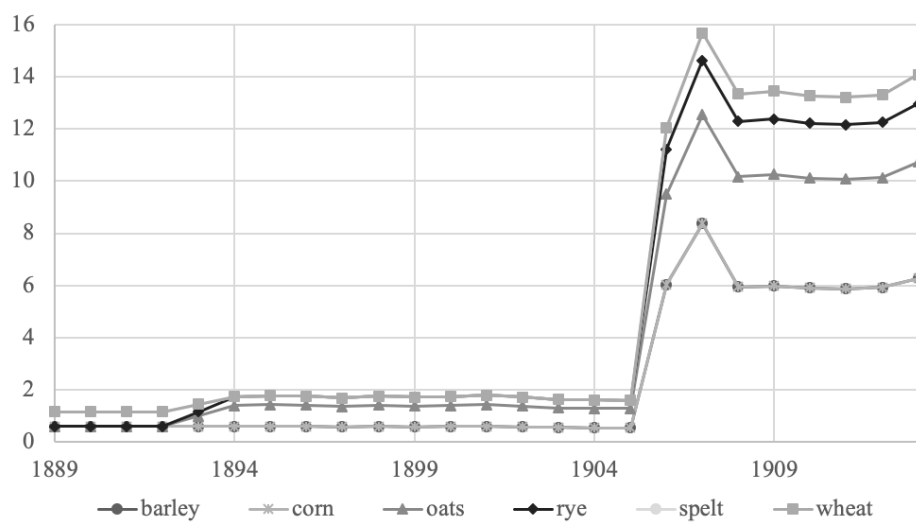
Sources: own illustration. See the text for details.

Figure 3: Mean transport cost (with 95% confidence interval) between city-pairs in the sample (1925/6 dinar per 100kg)



Sources: see the text.

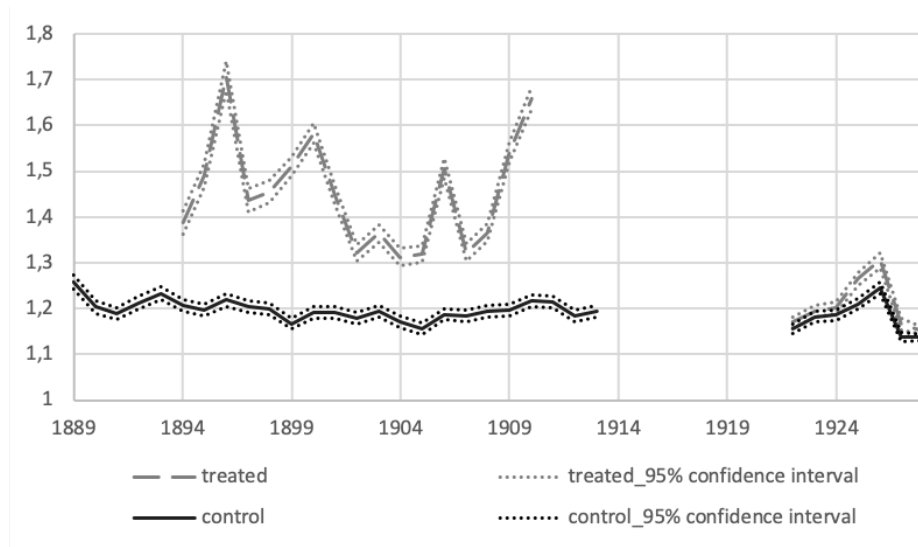
Figure 4: Tariffs for imports from Serbia by grain (dinar per 100kg), 1889-1913



Notes: Austro-Hungarian tariff in crowns per quintal converted to dinar per quintal at the exchange rate.

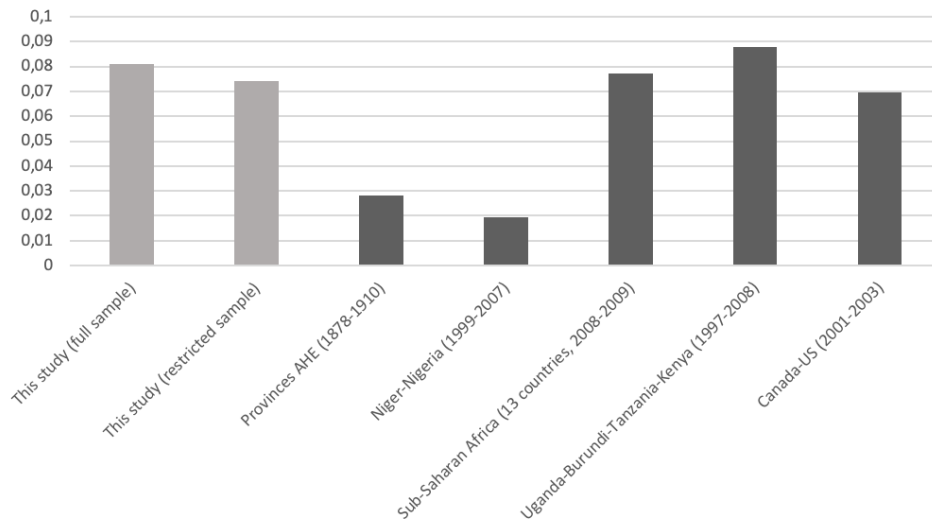
Source: see the text.

Figure 5: Trends in price ratios in “treatment” and “control” samples (fixed-effects panel)



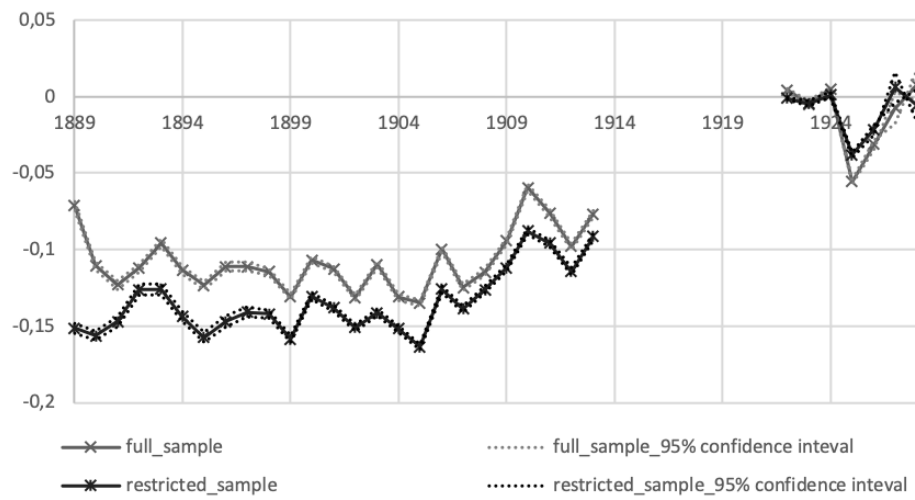
Notes: The trends are estimated with fixed-effects panel regressions of price dispersion over year dummies. City-pairs in the treatment sample were separated by the Austro-Hungarian border; city-pairs in the control sample were not. Standard errors are clustered by city-pair/grain.
Sources: see the text.

Figure 6: Comparison of border coefficients across studies



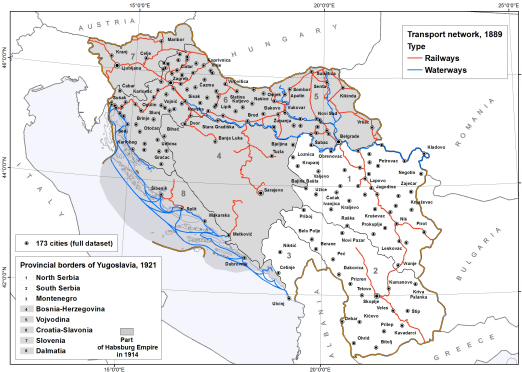
Notes: AHE=Austro-Hungarian Empire. Uganda-Burundi-Tanzania-Kenya’s border’s coefficient is for staple food.
Sources: Provinces AHE: Schulze and Wolf (2009); Niger-Nigeria: Aker et al. (2014); Sub-Saharan Africa: Brenton et al. (2014); Uganda-Burundi-Tanzania-Kenya: Versailles (2012); Canada-US: Broda and Weinstein (2008).

Figure 7: *Trend of the religious similarity coefficient, 1889-1927 (reference year = 1928)*

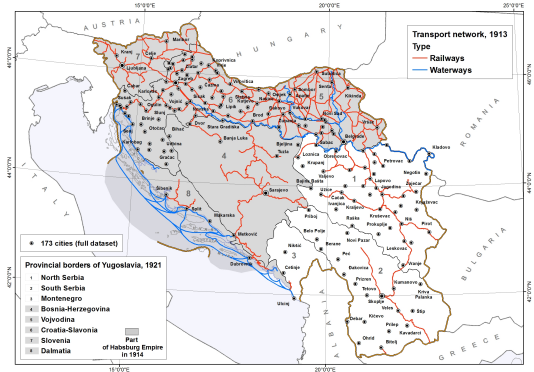


Notes: The trends are estimated with Equation 2 adding interactions between religious similarity, as defined in Equation 3, and year dummies. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. Standard errors are clustered by city-pair/grain.
Source: see the text.

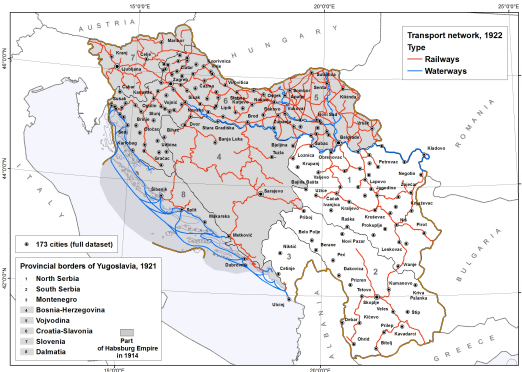
Figure A.1: Navigable waterways and the evolution of the rail network



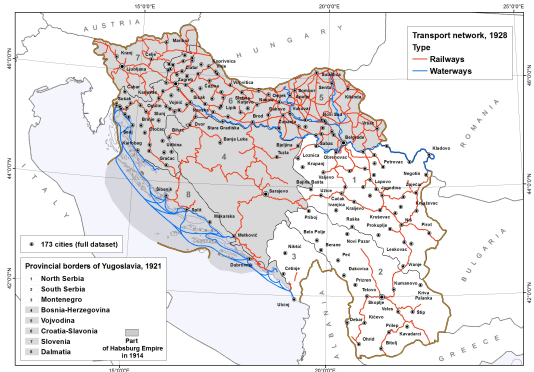
(a) 1889



(b) 1913



(c) 1922



(d) 1928

Notes: own illustration. See the text for details.

Tables

Table 1: *Sample summary statistics*

| Variable | Sample | N | Mean | Std. dev. | Min. | Max. |
|----------------------------------|-----------|---------|---------|-----------|-------|---------|
| Price ratio | All | 576,242 | 1.297 | 0.305 | 1 | 8 |
| Transport (1925/6 dinar/quintal) | All | 576,242 | 136.484 | 156.254 | 2.53 | 1003.82 |
| Tariff (dinar/quintal) | All | 576,242 | 3.515 | 4.457 | 0 | 15.693 |
| Religious similarity | All | 576,242 | 0.487 | 0.348 | 0 | 1 |
| Price ratio | Treatment | 230,657 | 1.434 | 0.356 | 1 | 6.907 |
| Transport (1925/6 dinar/quintal) | Treatment | 230,657 | 161.328 | 166.475 | 2.75 | 1003.82 |
| Tariff (dinar/quintal) | Treatment | 230,657 | 3.923 | 4.6 | 0 | 15.693 |
| Religious similarity | Treatment | 230,657 | 0.195 | 0.234 | 0 | 0.991 |
| Price ratio | Control | 345,585 | 1.205 | 0.222 | 1 | 8 |
| Transport (1925/6 dinar/quintal) | Control | 345,585 | 119.902 | 146.721 | 2.53 | 799.97 |
| Tariff (dinar/quintal) | Control | 345,585 | 0 | 0 | 0 | 0 |
| Religious similarity | Control | 345,585 | 0.682 | 0.265 | 0.007 | 1 |

Notes: Std. dev.=standard deviation, Min.=minimum, Max.=maximum. Price ratios are computed with the higher price in the numerator. City-pairs in the treatment sample were separated by the Austro-Hungarian border; city-pairs in the control sample were not.

Table 2: *The border effect*

| Dependent variable: $ \ln(\frac{P_{igt}}{P_{jgt}}) $ | (1) | (2) |
|--|---------------------|---------------------|
| Border | 0.081*** (22.46) | 0.074*** (20.32) |
| Ln(tariff factor+1) | 0.129*** (64.95) | 0.217*** (79.20) |
| Ln(transport cost factor) | 0.053*** (63.58) | 0.070*** (72.60) |
| Cities-grain dummies | yes | yes |
| Year-grain dummies | yes | yes |
| Restricted sample | no | yes |
| R-squared | 0.490 | 0.536 |
| N | 576,242 | 420,028 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps.

Table 3: The role of ethnicity

| Dependent variable: $ \ln(\frac{p_{igt}}{p_{jgt}}) $ | (1) | (2) | (3) | (4) | (5) | (6) |
|--|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| Border | 0.070*** (18.74) | 0.060*** (16.05) | 0.044*** (10.4) | 0.022*** (5.23) | 0.057*** (14.87) | 0.034*** (9.02) |
| Ln(tariff factor+1) | 0.128*** (64.52) | 0.215*** (78.77) | 0.128*** (64.43) | 0.215*** (78.36) | 0.125*** (63.48) | 0.214*** (78.44) |
| Ln(transport cost factor) | 0.051*** (61.78) | 0.067*** (69.69) | 0.052*** (62.63) | 0.068*** (70.96) | 0.053*** (62.34) | 0.071*** (73.05) |
| Religious similarity | -0.033*** (-12.96) | -0.041*** (-16.62) | -0.050*** (-17.05) | -0.066*** (-24.24) | -4.356*** (-24.44) | -7.483*** (-35.23) |
| Religious similarity*Yugoslavia | | | 0.097*** (18.62) | 0.125*** (24.08) | | |
| Religious similarity*year*100 | | | | | 0.227*** (24.31) | 0.390*** (35.08) |
| City-grain dummies | yes | yes | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes | yes | yes |
| Restricted sample | no | yes | no | yes | no | yes |
| R-squared | 0.491 | 0.538 | 0.492 | 0.540 | 0.492 | 0.541 |
| N | 576,242 | 420,028 | 576,242 | 420,028 | 576,242 | 420,028 |

Notes: the estimated equations are 4a-4c. t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. The interaction coefficients between *Religious similarity* and *year* are multiplied by 100 to facilitate their comparison.

Table 4: The role of ethnicity in treatment and control samples

| Dependent variable: $ \ln(\frac{p_{igt}}{p_{jgt}}) $ | (1) | (2) | (3) | (4) |
|--|----------------------|----------------------|-----------------------|-----------------------|
| Border | 0.049*** (5.49) | 0.039*** (4.03) | | |
| Ln(tariff factor+1) | 0.117*** (53.58) | 0.227*** (72.48) | | |
| Ln(transport cost factor) | 0.064*** (66.93) | 0.096*** (59.62) | 0.036*** (49.74) | 0.043*** (46.63) |
| Religious similarity | -7.860*** (-9.67) | -4.442*** (-4.51) | -1.754*** (-10.51) | -4.266*** (-19.68) |
| Religious similarity*year*100 | 0.410*** (9.63) | 0.230*** (4.46) | 0.092*** (10.47) | 0.223*** (19.6) |
| City-grain dummies | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes |
| Restricted sample | no | yes | no | yes |
| Treated | yes | yes | no | no |
| R-squared | 0.569 | 0.642 | 0.392 | 0.436 |
| N | 230,657 | 145,754 | 345,585 | 274,274 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. City-pairs in the treatment sample were separated by the Austro-Hungarian border; city-pairs in the control sample were not. The interaction coefficients between *Religious similarity* and *year* are multiplied by 100 to facilitate their comparison.

Table 5: Results by grain

| Dependent variable: | Barley | | Corn | | Oat | | Rye | | Spelt | | Wheat | |
|-------------------------------|----------------------|------------------------|----------------------|-----------------------|-----------------------|------------------------|----------------------|------------------------|---------------------|----------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Border | 0.053*** (6) | 0.007 (0.66) | 0.045*** (6.14) | -0.019** (-2.44) | 0.073*** (7.86) | 0.006 (0.58) | 0.144*** (16.35) | 0.078*** (7.66) | 0.020 (0.13) | -0.036 (-0.24) | 0.098*** (14.81) | 0.069*** (9.03) |
| Ln(tariff factor+1) | 0.147*** (17.56) | 0.189*** (21.18) | 0.230*** (36.92) | 0.289*** (40.87) | 0.183*** (31.54) | 0.227*** (35.88) | 0.154*** (18.87) | 0.199*** (23.03) | 0.452*** (9.34) | 0.618*** (11.31) | 0.083*** (14.79) | 0.104*** (17.23) |
| Ln(transport cost factor) | 0.076*** (26) | 0.076*** (26.42) | 0.085*** (47.15) | 0.085*** (47.56) | 0.065*** (30.1) | 0.064*** (30.48) | 0.055*** (21.63) | 0.057*** (22.33) | 0.152*** (11.45) | 0.149*** (11.35) | 0.037*** (21.03) | 0.038*** (21.48) |
| Religious similarity | -0.020*** (-3.07) | -10.496*** (-11.06) | -0.039*** (-9.03) | -15.419*** (-21.3) | -0.089*** (-16.33) | -14.401*** (-16.64) | -0.044*** (-7.45) | -14.176*** (-15.64) | -0.046 (-1.14) | -62.957*** (-6.1) | -0.009** (-2.04) | -6.755*** (-9.6) |
| Religious similarity*year*100 | | 0.549*** (11.05) | | 0.806*** (21.27) | | 0.750*** (16.53) | | 0.740*** (15.61) | | 3.303*** (6.1) | | 0.353*** (9.6) |
| City-grain dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Restricted sample | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| R-squared | 0.553 | 0.555 | 0.505 | 0.512 | 0.594 | 0.599 | 0.575 | 0.579 | 0.569 | 0.580 | 0.537 | 0.538 |
| N | 81,413 | 81,413 | 103,533 | 103,533 | 90,864 | 90,864 | 56,748 | 56,748 | 3,430 | 3,430 | 84,040 | 84,040 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%, ** significant at 5%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. The interaction coefficients between *Religious similarity* and *year* are multiplied by 100 to facilitate their comparison.

Table 6: *Balanced and similar samples*

| Dependent variable: $ \ln(\frac{p_{igt}}{p_{jgt}}) $ | (1) | (2) | (3) | (4) | (5) | (6) |
|--|---------------------|-----------------------|--------------------|-----------------------|----------------------|----------------------|
| Border | 0.079*** (7.29) | -0.011 (-0.63) | 0.034 (1.34) | -0.106* (-1.75) | 0.116*** (14.64) | 0.091*** (11.37) |
| Ln(tariff factor+1) | 0.112*** (13.96) | 0.102*** (12.61) | 0.098* (1.69) | 0.102* (1.78) | 0.228*** (47.11) | 0.227*** (46.97) |
| Ln(transport cost factor) | 0.071*** (13.96) | 0.072*** (14.62) | 0.148*** (9.99) | 0.150*** (9.9) | 0.066*** (34.73) | 0.068*** (34.99) |
| Religious similarity | 0.025 (0.69) | -15.130*** (-7.76) | 0.043* (1.88) | -15.476*** (-3.12) | -0.053*** (-4.86) | -3.519*** (-7.15) |
| Religious similarity*year*100 | | 0.793*** (7.75) | | 0.806*** (3.13) | | 0.181*** (6.98) |
| City-grain dummies | yes | yes | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes | yes | yes |
| Restricted sample | yes | yes | yes | yes | yes | yes |
| Sample | Balanced | Balanced | Capitals | Capitals | Frontier | Frontier |
| R-squared | 0.510 | 0.516 | 0.668 | 0.673 | 0.665 | 0.665 |
| N | 11,203 | 11,203 | 1,279 | 1,279 | 72,514 | 72,514 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%, ** significant at 5%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. The balanced sample only include city-pairs that have observations in all the years between 1899 to 1910 and between 1922 and 1928 (the largest feasible balanced panel). The capitals sample include only observations from the provincial capitals. The frontier sample includes observations only from cities close to the border (within 50 km) between Austria-Hungary and Serbia. The interaction coefficients between *Religious similarity* and *year* are multiplied by 100 to facilitate their comparison.

Table 7: *Dark trade costs and pre-treatment trends*

| Dependent variable: $ \ln(\frac{p_{igt}}{p_{jgt}}) $ | (1) | (2) | (3) | (4) | (5) |
|--|-----------------------|-----------------------|------------------------|-----------------------|------------------------|
| Border | 0.058*** (15.75) | 0.033*** (8.86) | -0.002 (-0.57) | 0.079*** (21.11) | 0.011** (2.33) |
| Ln(tariff factor+1) | 0.215*** (78.78) | 0.214*** (78.45) | 0.255*** (82.62) | 0.205*** (72.45) | 0.246*** (77.64) |
| Ln(transport cost factor) | 0.064*** (62.15) | 0.068*** (65.47) | 0.066*** (62.9) | 0.070*** (70.42) | 0.069*** (69.82) |
| Religious similarity | -0.036*** (-13.93) | -7.376*** (-34.52) | -12.015*** (-40.81) | -0.051*** (-19.58) | -12.509*** (-30.02) |
| Religious similarity*year*100 | | 0.385*** (34.41) | 0.627*** (40.74) | | 0.653*** (29.91) |
| Ln(distance) | 0.007*** (8) | 0.005*** (6.22) | 1.316*** (27.69) | | |
| Ln(distance)*year*100 | | | -0.069*** (-27.59) | | |
| Treated*year*100 | 0.058*** (15.75) | 0.033*** (8.86) | -0.002 (-0.57) | 0.106*** (12.28) | -0.331*** (-18.86) |
| Control*year*100 | 0.215*** (78.78) | 0.214*** (78.45) | 0.255*** (82.62) | 0.109*** (12.56) | -0.331*** (-18.77) |
| City-grain dummies | yes | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes | yes |
| Restricted sample | yes | yes | yes | yes | yes |
| Pre-treatment trends | no | no | no | yes | yes |
| R-squared | 0.538 | 0.541 | 0.543 | 0.539 | 0.543 |
| N | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%, ** significant at 5%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. The interaction coefficients between *Religious similarity* and *year* and between *Treated* or *Control* and *year* are multiplied by 100 to facilitate their comparison.

Table 8: Alternative measures of ethnic similarity

| Dependent variable: $ \ln(\frac{P_{igt}}{P_{jgt}}) $ | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|--|----------------------|-----------------------|----------------------|-----------------------|-----------------------|-----------------------|------------------------|---------------------|------------------------|
| Border | 0.060*** (16.17) | 0.058*** (15.34) | 0.058*** (15.49) | 0.032*** (8.41) | 0.019*** (4.53) | 0.036*** (9.74) | 0.011*** (2.65) | 0.075*** (20.22) | -0.015*** (-3.08) |
| Ln(tariff factor+1) | 0.215*** (78.77) | 0.215*** (78.71) | 0.215*** (78.75) | 0.214*** (78.37) | 0.224*** (74.5) | 0.210*** (76.62) | 0.231*** (74.72) | 0.217*** (79.21) | 0.296*** (85.98) |
| Ln(transport cost factor) | 0.067*** (69.68) | 0.067*** (69.82) | 0.067*** (69.78) | 0.071*** (73.17) | 0.070*** (71.67) | 0.072*** (73.39) | 0.070*** (71.48) | 0.070*** (72.49) | 0.069*** (72.81) |
| Religious similarity | | -0.041*** (-16.76) | | -7.488*** (-35.26) | -9.425*** (-26.89) | | | | |
| Religious similarity*year*100 | | | | 0.390*** (35.11) | 0.492*** (26.78) | | | | |
| Religious similarity (Herfindahl) | -0.042*** (-17.2) | | -0.042*** (-17.3) | | | -6.637*** (-37.22) | -10.969*** (-30.51) | | |
| Religious similarity (Herfindahl)*year*100 | | | | | | 0.346*** (37.03) | 0.573*** (30.41) | | |
| Linguistic similarity | | 0.023*** (3.22) | | 0.023*** (3.46) | 2.744*** (7.99) | | | | |
| Linguistic similarity*year*100 | | | | | -0.143*** (-7.92) | | | | |
| Linguistic similarity (Herfindahl) | | | 0.016** (2.44) | | | 0.018*** (2.81) | 5.682*** (15.3) | | |
| Linguistic similarity (Herfindahl)*year*100 | | | | | | | -0.297*** (-15.26) | | |
| Same province | | | | | | | | 0.009** (2) | -12.654*** (-36.95) |
| Same province*year*100 | | | | | | | | | 0.658*** (37.02) |
| City-grain dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Year-grain dummies | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Restricted sample | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| R-squared | 0.538 | 0.538 | 0.538 | 0.541 | 0.541 | 0.541 | 0.542 | 0.537 | 0.541 |
| N | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 | 420,028 |

Notes: t-statistics with standard errors clustered by city-pair/grain are given in parentheses. *** significant at 1%, ** significant at 5%. The restricted sample includes only city-pairs with observable trade costs lower than price gaps. The interaction coefficients between *Religious similarity* and *year* and between *Linguistic similarity* and *year* are multiplied by 100 to facilitate their comparison.

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