The long-run trade costs of the American Secession*

Gabriel Felbermayr† and Jasmin Gröschl‡

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Abstract

Based on the US commodity flow survey, we show that the historical Union-Confederacy border is still a trade inhibiting force today. It reduces trade between US states by about 16 percent. This is more than any other border between two arbitrary state groupings in the data. The estimation is based on a theory-consistent gravity model. It is robust over available years, different levels of aggregation, to the inclusion of intra-state trade, to different measurement of geographical distance, or to alternative treatment of border states. Extending the model to a large array of contemporaneous controls, ranging from network variables, institutional or demographic variables, Heckscher-Ohlin or Linder terms, lowers the estimate only slightly. Adding historical variables cited in the literature as causal for the Secession, does not explain the border effect away. Finally, adding Western states to the analysis suggests that the estimated border effect is more than a North-South effect and may indeed by driven by the Secession.

Keywords: American Secession, Border Effect, Intranational Trade, Gravity, US State Level

JEL-Classification: F15, N72, N92, Z10

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†Ifo Institute for Economic Research at the University of Munich, Poschingerstr. 5, 81679 Munich, Germany; CESifo & GEP; felbermayr@ifo.de

‡Ifo Institute for Economic Research at the University of Munich, Poschingerstr. 5, 81679 Munich, Germany; groeschl@ifo.de

"The past is never dead, it's not even past".

(William Faulkner)

"The economic legacy of the war ... has all but faded"

(The Economist)

150 years after Confederate troops attacked Fort Sumter in South Carolina and the American Civil War set off, the nation is still divided over whether the war was fought over moral issues - slavery – or over economic policy. The war, that cost 620,000 American lives, more than any other military conflict, has traumatized America. It has retarded the economic development of the nation (Goldin and Lewis, 1975) and has left a long-lasting scar in the political and economic landscape of the US.

The secession of 11 Southern states occurred mainly because of deep differences with the North about slavery and import tariffs. The Southern economy was dominated by large-scale plantations of cotton, tobacco, rice, and sugar, whose profitability relied on forced labor. It exported crops to Europe and imported manufacturing goods from there. The North, dominated by smaller land-holdings, was rapidly urbanizing; slavery was practically abolished north of the Mason-Dixon Line by 1820.[1] Its infant manufacturing industries were protected by import tariffs against European competition.

Today, on average, the South is still poorer, more rural, more agricultural, less educated, more religious, and has different political views. The economic gap has narrowed (Michener and McLean, 1999), in particular after the end of segregation in the Sixties of the last century. But political disagreement, in particular on the role of federal government, continue to beset the country. In many ways, the former border between the Union and the Confederation marks still today a cultural divide.

This paper asks two main question. One is rather descriptive: To what extent does the former Union-Confederacy border affect today's trade between

[1]The Mason-Dixon Line settled a conflict between British Colonies in America and set the common borders of Pennsylvania, Maryland, Delaware, and West Virginia.

US states? In other words, does the former border still constitute a discontinuity in the economic geography of America? The modern literature has identified cultural differences across countries as impediments of international trade, but typically not within the same country. By offering an answer to this question, we contribute to a growing literature on the long-shadow of history for economic transactions (Nitsch and Wolf, 2009; Falck, Heblich, Lameli and Südekum, 2010; Head, Mayer and Ries, 2010). The second question is more analytic and much more difficult to address: Can the estimated border effect be interpreted as a genuine Union-vs-Confederation effect?

Employing the theory-consistent gravity model of Anderson and van Wincoop (AvW, 2003) for bilateral trade between states, we find a robust, statistically significant, and economically meaningful trade-inhibiting effect of the former border. In the preferred 1993 data, on average, the historical border reduces trade between states of the former Confederation or Union by between 22 and 16 percent. In comparison, the Canada-US border restricts trade by 155 to 165 percent. Running a million placebos, we show that the 'Secession effect' is larger than any other border effect estimated between other groupings of US states.It is also extremely robust. Employing alternative methodologies (Feenstra, 2004; Baier and Bergstrand, 2009), using different years of the commodity flows survey (1997, 2002, 2007), drawing on sectoral rather than aggregate bilateral trade data (as Chen, 2004), measuring transportation costs differently (travel time instead of sheer geographical distance), including intra-state trade (to account for the home-market bias), or experimenting with different allocations of border states (for which adherence to the Union or the Confederation is historically not obvious), does not change the results. The estimated border effect represents an ad valorem tariff equivalent of about 2 to 8 percent. Interestingly, the effect is stronger (and more robust) in the food, manufacturing, and chemicals sectors than in mining, which is characterized by a completely standardized good, or machinery, where the pattern of specialization across North and South is very strong.

In a second step, we add a large array of contemporaneous variables to the analysis that are not present in the rather parsimonious AvW model, but which are related to the heterogeneity between Northern and Southern states. We add variables that are meant to capture migrant networks, ethnic networks, or religious networks. While these variables matter empirically, they do not help to reduce the estimated border effect. We account for cultural differences expressed by different colonial relationships across states, for different patterns of urbanization, and for additional geographical variables. We include variables that relate to the institutional setup of states (the importance of trade unions or minimum wages), or that measure differences in the judicial system. We control for differences in endowment proportions (Heckscher-Ohlin effects), or for differences in the structure of the states' economies. Finally, we add demographic factors and test the Linder hypothesis. Most of these controls have some explanatory power, but they do not undo the border effect. The estimate falls from 16 to 13 percent. This finding survives the same battery of robustness checks applied to the parsimonious AvW model.

Third, we acknowledge that the North-South divide, marked by the Secession, is likely not to be exogenous. The literature suggests that it is related to differences between Northern and Southern states in their endowment with land, or in the size and structure of agricultural production (Engerman and Sokoloff, 2000, 2005). The emergence of the divide may have to do with historical ethnic patterns, historical educational achievements of the population, or institutional differences as captured, e.g., by the historical malaria incidence (Acemoglu, Johnson and Robinson, 2002). Finally, and most importantly, it may result from the incidence of slavery. Not all of these variables matter empirically for contemporaneous trade patterns, but they cannot easily be excluded from the explanation of bilateral trade on conceptual grounds. Including them into the gravity equation does not undo the 'Secession effect'. Quite to the opposite, the estimated effect actually goes up. Finally, we extend the analysis to Western states, but keep the same coding of the border. Thus, we add pairs

of states which have been completely unaffected by the Secession. Then, the border dummy essentially captures whether two states have been on opposing sides of the Civil War rather than belonging to the North or the South. We continue to find a border effect (7 to 13 percent), which we may now attribute to the Secession.

The literature offers explanations of border effects in terms of 'political barriers', 'artefact', and 'fundamentals'. The first should be largely absent in an integrated economy such as the US. The second relates to difficulties in separating the impact of border-related trade barriers from the impact of geographical distance (Head and Mayer, 2002) or to problems of statistical aggregation (Hillberry and Hummels, 2008). We deal with these issues by using alternative measures of trade costs and by a large amount of placebo exercises. We view our results as consistent with the 'fundamentals' approach: historical events have shaped cultural determinants of trade which still matter today.

The literature on border effects was pioneered by McCallum (1995), who finds that trade volumes between Canadian provinces were about 22 times larger than those between Canada and the US in 1988. Subsequent research[2] shows that states usually trade 5 to 20 times more domestically than internationally. Few studies have moved from simply exploring border barriers to investigating and explaining potential causes. Trade barriers on the national level that increase the costs for commodities crossing a border, such as tariffs, quotas, exchange rate variability, transaction costs, and regulatory differences appear as foremost and obvious aspirants in causing domestic shipments to exceed international volumes. However, researchers find little evidence in support of the conjecture (Wei 1996; Hillberry 1999). Recent studies illustrate that the impact of borders also extends to the sub-national level (Wolf 1997, 2000; Hillberry and Hummels 2003; Combes et al. 2005; Buch and Toubal 2009; Nitsch and Wolf

[2]Helliwell (1997, 1998, 2000, 2002), Wei (1996), Hillberry (1999, 2002), Wolf (1997, 2000), Nitsch (2000), Parsley and Wei (2001), Hillberry and Hummels (2003), Anderson and van Wincoop (2003), Chen (2004), Feenstra (2004), Combes et al. (2005), Millimet and Osang (2005), Baier and Bergstrand (2009), Buch and Toubal (2009), Nitsch and Wolf (2011) to name only a few.

2011), implying that additional reasons for high local trade levels must exist. They conclude that intermediate and final commodity producers conglomerate to avoid transaction costs (Wolf 1997; Hillberry 1999).

The remainder of the paper is structured as follows. Section I. provides us with the empirical strategy. Section II. describes the benchmark gravity results, placebo estimations and a sensitivity analysis. Section III. uses a large array of contemporaneous controls to address a potential omitted variables problem. While Section IV. attempts to explain the 'Secession effect' by historical variables and by adding Western states to the analysis. The last section concludes.

I. Empirical Strategy and Data

A. Empirical Strategy

Historical events and the persistence of cultural borders might affect trade patterns within the US today. To evaluate the impact of the rift between the North and the South on their contemporaneous trade flows, we proxy trade costs by geographical distance and the historical border between the former alliance of states in the Union and the Confederacy. We use theoretically-motivated econometric methods in a gravity setup to assess the effect of the Secession and the subsequent war on contemporaneous trade patterns between the North and the South.

First, we estimate a nonlinear least squares (NLS) model suggested by Anderson and van Wincoop (2003). The procedure minimizes the sum-of-square residuals of the stochastic form

$$\ln z_{ij} = \ln\left(\frac{x_{ij}}{Y_i Y_j}\right) = \beta_0 + \beta_1 \text{Border}_{ij} + \beta_2 \ln \text{Distance}_{ij} - \ln P_i^{1-\sigma} - \ln P_j^{1-\sigma} + \epsilon_{ij}, \quad (1)$$

where $\ln z_{ij}$ relates to the log of bilateral exports between i and j relative to the states' GDPs, β_0 is a constant across state pairs, $\beta_1 = -\alpha(\sigma - 1)$ and $\beta_2 =$

$-\rho(\sigma-1)$. $\text{Border}_{ij} = (1-\delta_{ij})$ represents the historical border line between Union and Confederate states, which takes a value of unity if states in the pair historically belonged to opposing alliances and zero otherwise. A negative coefficient represents a preference for less cross-border trade between states of the Union and the Confederacy relative to more trade among states of the own alliance. $\ln \text{Distance}_{ij}$ accounts for the log of distance between states. $\ln P_i^{1-\sigma}$ and $\ln P_j^{1-\sigma}$ represent multilateral resistance terms, while σ denotes the elasticity of substitution. ϵ_{ij} captures the error term. However, multilateral resistance terms are not observable. To solve for the vector of multilateral resistance terms as an implicit function of observables (border, distance, income shares) and model parameters (β_1, β_2), we follow Anderson and van Wincoop (2003) and assume symmetric trade costs across states.

$$P_j^{1-\sigma} = \sum_k P_k^{\sigma-1}\theta_k e^{\beta_1 \text{Border}_{kj} + \beta_2 \ln \text{Distance}_{kj}}, \tag{2}$$

where k denotes the number of market-equilibrium conditions and $\theta_k = Y_k/Y^W$ as the income shares. Substituting the implicit solutions for $P_k^{\sigma-1}$ in (2), the system of equations to be estimated is then

$$\ln \boldsymbol{z} = h(\boldsymbol{Distance}, \boldsymbol{\delta}, \boldsymbol{\theta}, \beta_0, \beta_1, \beta_2) + \boldsymbol{\epsilon}, \tag{3}$$

where $\boldsymbol{Distance}$, $\boldsymbol{\delta}$, $\boldsymbol{\theta}$, and $\boldsymbol{\epsilon}$ are vectors that contain all the elements of the corresponding variables and $h(\cdot)$ is the right-hand side of equation (1) after solving implicitly for the equilibrium multilateral resistance terms.

In a second approach we follow a large strand of literature (Hummels 1999; Feenstra 2004; Redding and Venables 2004) and apply origin and destination fixed effects in an OLS gravity regression (FE). We estimate

$$\ln z_{ij} = \beta_0 + \beta_1 \text{Border}_{ij} + \beta_2 \ln \text{Distance}_{ij} + \boldsymbol{\gamma X_{ij}} + \beta_3^i \nu_i + \beta_4^j \nu_j + \epsilon_{ij}, \tag{4}$$

where all variables are denoted as above. $\boldsymbol{X_{ij}}$ denotes a vector of additional controls that we switch on and off. ν_i and ν_j relate to importer and exporter specific vector of idiosyncratic characteristics that account for multilateral indexes, $\beta_3^i = \ln(\tilde{P}^i)^{\sigma-1}$ and $\beta_4^j = \ln(\tilde{P}^j)^{\sigma-1}$. These vectors capture time-invariant origin and destination specific determinants, such as geographical characteristics and historical or cultural facts. ϵ_{ij} constitutes a robust error term. Alternatively, we distinguish two indicator variables to evaluate cross-region trade directly instead of allowing a single variable (Border$_{ij}$) to measure cross-border trade barriers. We follow the literature and include an indicator variable that equals one if trade takes place exclusively between states of the North and zero otherwise (North-North$_{ij}$), and an indicator that is unity if trade solely occurs between states of the South (South-South$_{ij}$). A positive coefficient represents a preference for trade within a region opposed to cross-border trade.

In order to complete the econometric methods used, we explore a third approach following Baier and Bergstrand (2009). Fixed effects measure multilateral resistance (MR) as the coefficients of origin and destination, while using explicit MR terms should produce similarly efficient results. We retrieve MR terms according to Baier and Bergstrand (2009).

$$\ln z_{ij} = \beta_0 + \beta_1 \text{Border}_{ij} + \beta_2 \ln \text{Distance}_{ij} + \boldsymbol{\gamma X_{ij}} + \beta_3 \text{MRBorder}_{ij} + \beta_4 \text{MRDist}_{ij} + \epsilon_{ij}, \tag{5}$$

where all coefficients are denoted as above. $\beta_3 = \alpha(\sigma-1)$, $\beta_4 = \rho(\sigma-1)$, and MR terms for distance and border are referred to as MRDist$_{ij}$ and MRBorder$_{ij}$. Coefficients for $\ln$ Distance$_{ij}$ (Border$_{ij}$) and MRDist$_{ij}$ (MRBorder$_{ij}$) are restricted to have oppositely-signed values.

B. Data Sources

For within- and cross-state trade flows we focus on bilateral export data from the 1993, 1997, 2002, and 2007 *Commodity Flow Survey* (CFS) collected by the

Bureau of Transportation Statistics. The CFS tracks shipments in net selling values in millions of dollars between or within states. The CFS covers 200,000 (100,000; 50,000; 100,000) representative US firms for 1993 (1997; 2002; 2007). Note that sample size has been a major issue as discussed by Erlbaum et al. (2006). Data reliability has been a key concern due to the reduction in sample size in the years following the 1993 survey. Hence, we focus on the 1993 CFS as our benchmark, which is the most comprehensive of the four years as it represents 25 percent of US firms in the registry and is extensively used in the border effect literature. GDP by state stems from the *Regional Economic Accounts*, provided by the Bureau of Economic Analysis. Bilateral distance is calculated as the great circle distance between state capitals. Intrastate distance is measured in accordance to the research by Anderson and van Wincoop (2003), Feenstra (2004), as well as Baier and Bergstrand (2009). Hence, we deploy the measure suggested by Wei (1996) that takes the quarter of the distance between a state and its closest neighbor.

Our primary sample consists of 28 US states divided into two groups that originate from the split caused by the Secession. The **South** comprises 11 states, while the **North** consists of 17 states, as listed in Table 1. The border states are excluded from the sample as their affinity to either of the two alliances is unsettled. This is among other things attributable to their geographical position and strong ties to both groups during the war. In addition, we exclude the District of Columbia as trade data are generally very poor.

It is well known that Northern states differ along a number of important dimensions from Southern states. Table 2 shows averages and standard deviations (for the year of 1993) of the variables used in this study, differentiating between North and South. Southern states have on average substantially larger shares of Afro-Americans (22.9 versus 7.4 per cent) in their resident populations; the share of Christians is higher while the share of Jewish citizens is smaller (0.8 versus 2.1 per cent). The percentage of urban population in total residents is lower in South than in North (65.6 versus 72.9). Historically (as

TABLE 1 – SAMPLE

North = Union	South = Confederacy	Excluded = Border States
Connecticut	Alabama	Delaware
Illinois	Arkansas	Kentucky
Indiana	Florida	Maryland
Iowa	Georgia	Missouri
Kansas	Louisiana	West Virginia
Maine	Mississippi	
Massachusetts	North Carolina	
Michigan	South Carolina	
Minnesota	Tennessee	
New Hampshire	Texas	
New Jersey	Virginia	
New York		
Ohio		
Pennsylvania		
Rhode Island		
Vermont		
Wisconsin		

of 1860), average farm sizes were substantially larger in the South than in the North; this gap has closed since then. The same is true for educational outcomes (illiteracy and average schooling). The GDP per capita average across the South is about 12 per cent lower than the average across the North. The most dramatic differences in 1993 data pertain to institutional variables: The North is much more unionized than the South. All Northern states had a minimum wage while only 45 percent of the Southern states had one. In 1993, 64 percent of Southern states voted Republican while only 12 of Northern states did.

North-South differences are also clearly visible when looking at pairs of states. Table 9 in the Appendix differentiates between the sample of all pairs ($N = 768$) and the sample of pairs that involve states from both sides of the historical border ($N = 364$). The network variables (denoted by the operator $\times$) display larger means in the full sample of pairs, while means of variables based on state differences (denoted by Δ) are larger in the North-South sample.

TABLE 2 – SUMMARY STATISTICS BY STATE, 1993

Unit of Observation: State Level					
Sample	North (N = 17)		South (N = 11)		Description
Variable	Mean	Std. Dev.	Mean	Std. Dev.	
Black Share	7.412	5.519	22.855	7.871	Percentage share of blacks in population.
Jewish Share	2.105	2.339	0.809	1.285	Percentage share of Jewish in population.
Christian Share	86.882	3.059	91.636	3.139	Percentage share of Christian in population.
Other Religion Share	1.131	0.786	0.919	0.416	Percentage share of people with other religion in population.
No Religion Share	7.647	1.998	5.000	1.673	Percentage share of people with no religion in population.
Urban Share	72.853	16.095	65.655	12.098	Percentage share of urban population in population.
ln 1860 Cropland	15.038	1.045	15.228	0.806	1860 cropland in 1,000 acres.
ln 1860 Farm Size	4.785	0.184	5.940	0.291	1860 average farm size in acres.
ln 1860 Population Density	3.338	1.384	2.454	0.929	1860 population by square km.
ln 1860 Illiteracy Rates	1.604	0.415	2.683	0.303	1860 share of non-slave illiterate in population.
1860 Slave Share	0.020	0.046	34.506	14.304	1860 slaves in population.
1860 Free Black Share	1.018	0.999	1.170	1.326	1860 free blacks in population.
1860 French Share	0.302	0.202	0.254	0.619	1860 French in population.
1860 Spanish Share	0.004	0.005	0.032	0.076	1860 Spanish in population.
1860 Irish Share	6.890	4.303	0.918	1.057	1860 Irish in population.
1860 German Share	4.772	4.244	0.886	1.271	1860 German in population.
1860 British Share	4.250	2.216	0.306	0.204	1860 (American) British in population.
1860 Malaria Risk	0.126	0.073	0.351	0.057	1860 Malaria Risk Index.
ln Capital-Labor Ratio	11.610	0.261	11.520	0.227	Capital relative to Labor.
ln High-Low Skilled Ratio	0.264	0.316	-0.256	0.256	Bachelor to high school degree of population 25 and older.
ln Average Schooling	2.579	0.023	2.538	0.023	Years of Schooling.
ln Cropland	7.821	2.223	8.574	0.656	Cropland in 1,000 acres.
ln Farm Size	5.309	0.570	5.574	0.424	Average farm size in acres.
ln Agricultural To Total Output	-4.515	0.687	-4.159	0.427	Agricultural relative to total output in million US $.
ln Manufacturing To Total Output	-1.615	0.250	-1.661	0.364	Manufacturing relative to total output in million US $.
ln Population	15.237	1.009	15.534	0.624	Total Population in thousands.
ln Population Density	5.175	1.145	4.602	0.485	Population by square km.
ln Fertility	4.127	0.071	4.184	0.065	Live births per 1,000 women 15-44 years of age.
ln Income Per Capita	10.194	0.134	10.073	0.117	Total GDP per capita.
Union Membership	18.106	5.470	8.436	2.826	Percentage of union membership.
Union Density	19.812	5.218	10.382	3.009	Percentage of union density.
Minimum Wage	1	0	0.454	0.522	1 if state has minimum wage, 0 otherwise.
Republican	0.118	0.332	0.636	0.505	1 if republican state in last presidential election, 0 otherwise.
Judiciary Election	1.824	0.883	1.182	0.405	1 if judiciary in state is elected, 0 otherwise.

Notes: Data sources as in Table 9.

II. The Effect of the American Secession

A. Benchmark Results

Estimating equations (3), (4) and (5) allows us to asses the average impact of the border on North-South trade relative to within region flows by means of standard procedures applied in the border literature. Table 3 provides our benchmark results, where we have a total of 768 observations for 1993. Estimates of

the NLS model are depicted in column (1), where coefficients of state income terms are constrained to unity. In line with the gravity literature, the estimated trade elasticity of distance is very close to -1. The coefficient on the border variable in column (1) indicates that the border reduces trade flows between the North and the South by 20 percent ($e^{-0.218}-1$) in 1993, which is statistically significant.[3] Hence, when we adapt the Anderson and van Wincoop (2003) NLS setting to our sample of states, we find a tariff equivalent of the border of 2.5 to 11.5 percent.[4] Compared to international border effects, this is quite a reasonable amount for a barrier to trade on the subnational level caused by an event more than a century ago. Anderson and van Wincoop (2003) find that within trade is 5.2 times higher than cross-border trade for the Canada-US case, which indicates a tariff equivalent of 20 to 128 percent.[5]

In column (2) we estimate equation (4) using origin and destination fixed effects, which account for the unobserved importer and exporter specific characteristics. Our model explains 87 percent of the variation in trade patterns. Under fixed effects, cross-border trade is on average 1.17 times smaller than within region trade. Hence, the border equals a tariff of 2 to 8 percent. The FE estimate is very close to that obtained under NLS. This is in line with Feenstra (2004), who also finds a slightly smaller but comparable effect to the Anderson and van Wincoop (2003) estimation in the Canada-US case. If we allow for two indicator variables to evaluate cross-state trade directly instead of using a single variable in column (3), we find that trade within the South is 1.78 times larger than cross-border trade with the North in 1993. Contrarily, the North trades 1.3 times less within the region than across the border. This result is interest-

[3]Note that this is the same as to say that "within" trade is bigger by factor 1.24 ($e^{0.218}$) than "between" trade.

[4]Broda, Greenfield and Weinstein (2006) estimate elasticities of substitution with a median of 3.8 and a mean of 12.1. The elasticity of substitution they estimate for the US is 2.4. We follow the recent literature and calculate tariff equivalents according to a range of the elasticity of substitution between 3 and 10.

[5]Feenstra (2004) assumes elasticities of σ = 5, 10 and 20 and thus finds a tariff equivalent of 9 to 50 percent. He notes that the lower end is reasonable, while the upper end of this estimate is definitely too high.

ing as we expect to find a prositive sign on both indicator variables. The strong positive impact on within South trade and the much smaller negative impact on within North trade could relate to current account imbalances within the US. As states in the North run on average a current account surplus the North trades more with the South than with itself. States in the South, however, run on average a current account deficit and thus trade more among themselves *and* with the North.

Estimating equation (5), we directly include MR terms into the gravity estimation as suggested by Baier and Bergstrand (2009) for 1993 in column (4) and (5). The adjusted explanation power of the estimation slightly falls to 75 percent, while the border estimate remains very close compared to the fixed effects (FE) estimation. The border impeding trade effect between the North and the South persists with a magnitude of 15 percent. In column (5), we find that trade within the South is 1.59 times larger than cross-border trade in 1993, while the coefficient for the North turns insignificant.

In a next step we explore the CFS data in more detail, as disaggregated trade flows at the commodity level are available. This is in the spirit of Hillberry (1999), who estimated commodity specific border effects for products traded between Canada and the US in 1993. We pool over all commodities available in the specific year. As commodities are subject to varying transportation costs, we include origin$\times$commodity and destination$\times$commodity fixed effects following Chen (2004). For 1993, results for the pooled commodity FE estimation are depicted in Table 3 column (6). We find that the border reduced North-South trade by 9 percent.

B. 1 Million Placebo Estimations

To see whether the border effect between the North and the South is a statistical artifact between these state groups in the US, we calculate placebo effects for other combinations of states in our benchmark sample. We randomly assign states of our originally 28 states into either of the two groups (17 Northern or

TABLE 3 – BASIC BORDER EFFECT RESULTS

Year of Data	1993 (N = 768)					
Data	Aggregated					Commodity
Specification	AvW NLS	Fixed Effects		OLS with MR Terms		Chen (2004) FE
	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy$_{ij}$	-0.218***	-0.157***		-0.157***		-0.090***
	(0.04)	(0.03)		(0.04)		(0.02)
South-South Dummy$_{ij}$			0.578***		0.462***	
			(0.10)		(0.08)	
North-North Dummy$_{ij}$			-0.264***		-0.050	
			(0.09)		(0.05)	
ln Distance$_{ij}$	-0.979***	-1.108***	-1.108***	-1.055***	-1.039***	-0.978***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Fixed Effects						
Importer	n.a.	YES	YES	-	-	-
Exporter	n.a.	YES	YES	-	-	-
Importer×Commodity	n.a.	-	-	-	-	YES
Exporter×Commodity	n.a.	-	-	-	-	YES
Multilateral Resistance	n.a.	-	-	YES	YES	-
Adjusted R^2	n.a.	0.874	0.874	0.751	0.759	0.636
F-Test	n.a.	61.93	61.93	308.61	272.11	n.a.

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. n.a. means not applicable. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. Pooling over all commodities in 1993, we have in column (6) 13,303 observations. States in Sample as in Table 1. District of Columbia is excluded.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

11 Southern states). By repeating this a million times, we find a negative and significant impediment to trade in 13.4 percent of the cases.

Running a million placebos, we show that the "Secession effect" is larger than any other border effect estimated between other groupings of US states.If the states of the North and the South get mingled up too excessively in the two groups, the border effect either becomes much smaller or vanishes completely. In placebos of 1 million random replications, we find a negative significant border effect in most of the bins where 1–3 states are exchanged, but only in very few cases if 4 or more states are randomly assigned into opposing groups (compare part (a) of Figure 1), even though 71 percent of our observations are located

in the bins where 5–7 states are exchanged. On average we find that the absolute border effect shrinks towards zero the more states are exchanged with regard to the benchmark sample (compare part (b) of Figure 1).[6] These findings suggests that the effect is not present between any assignment of individual states into groups, but that the Secession and the subsequent Civil War tore the two groups of states even further apart, so that a trade barrier exists between them up to the present day.

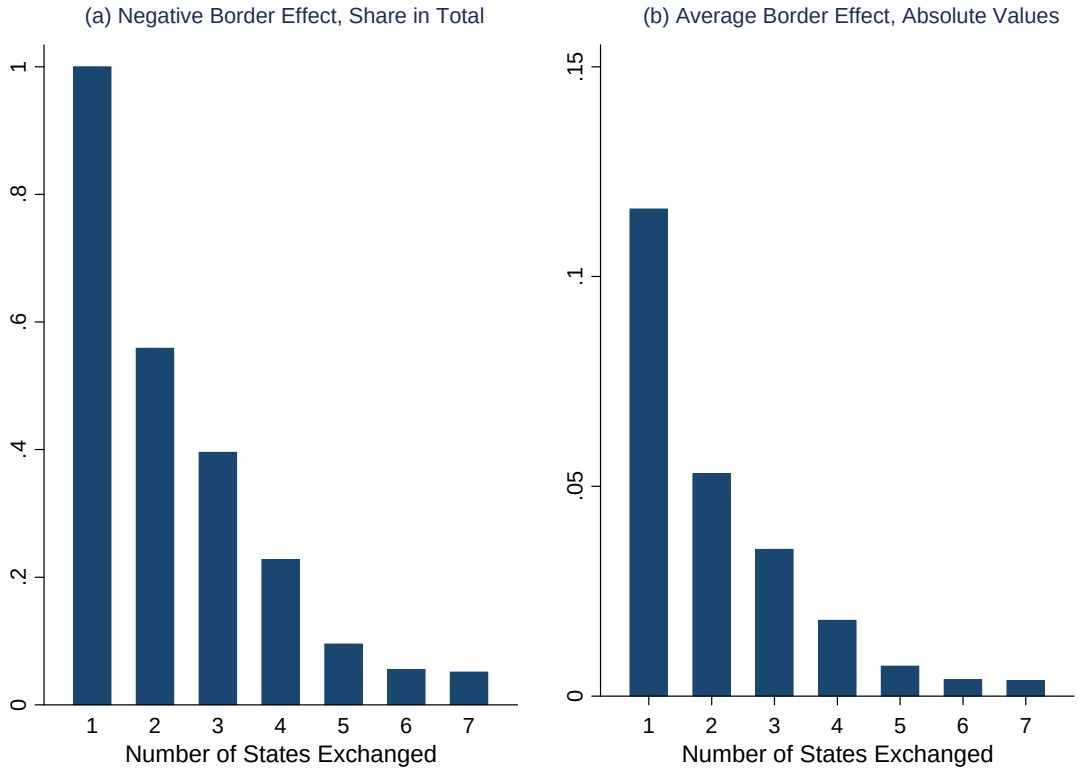

FIGURE 1. FREQUENCY AND AVERAGE SIZE OF BORDER EFFECT

So far, only "old" US states were included in the regression. In a further approach, we include the whole US and look into groups of states in our benchmark year that did not receive a "treatment" as the North and the South did by the Secession and the subsequent war. Results can be found in Table 10 in

[6] A slightly negative and significant border effect can still be found in a sample where quite a few states are switched to the opposing group if the states that are exchanged are small, such as Vermont or New Hampshire.

the Appendix. First, we use the fact that coastal US states differ tremendously from states in the interior. In Table 10 column (1) and (2), we find no significant negative border effect between coastal and interior states. Furthermore, we find that the Interior trades more among itself, while the coastal regions do not. This result is not surprising given the geographic constellation of the groups. Nevertheless, the estimate on cross-state trade within the Interior is only half the size of the effect on South-South trade found in section *A.*. Second, when we separate states into Eastern and Western states in Table 10 column (4) to (6), we even find that the East and the West trade significantly more with another in the NLS setting. But, no significant barrier to trade exists when we use the FE setup. Even more strikingly is the finding that the East trades more within the region. As no stable border effect can be replicated for either of the two exemplary cases examined, we can conclude that the border barrier in the North-South case is a rather unique phenomenon within the US.[7]

C. Sensitivity Analysis

Table 4 Panel A provides summary results for the 28 state sample under the Anderson and van Wincoop (2003) NLS setup, FE, OLS with MR terms and the pooled commodity FE estimation for 1997. Panel B depicts estimates for all setups for 2002 and Panel C for 2007. Full results can be found in Table 11 in the Appendix. The impact of the historical border on trade patterns remains negative and significant in all years and across specifications.

In Panel A, we have 766 observations available for 1997. The border effect persists in hampering cross-border trade by 12 percent in column (A1) and using MR terms directly in column (A3). While the FE approach reduces trade between the North and the South by 9 percent in column (A3). When we use the pooled commodity structure and deploy importer and exporter specific commodity effects following Chen (2004) in column (A4), we obtain a slightly larger

[7]We will later also use our original sample of 28 states as treated states and include the West as states that where not treated by the war. When we include western states into the sample, we still find a negative trade impeding border effect between the North and the South.

cross-border trade impeding effect of 13 percent. Panel B presents a similar picture for the year 2002, with 739 non-zero observations, where the border reduces trade by 13.5 to 17.6 percent, depending on the specification used. Results for 2007 are depicted in Panel C, where we have 768 observations. The results suggest that the trade hampering effect of the border persists. Trade is reduced by 12.5 to 18 percent.

TABLE 4 – SENSITIVITY ANALYSIS

Data	Aggregated			Commodity
Specification	AvW NLS	FE	OLS with MR Terms	FE Chen (2004)
	PANEL A: 1997 (N = 766)			
	(A1)	(A2)	(A3)	(A4)
Border Dummy$_{ij}$	-0.128***	-0.091***	-0.126***	-0.138***
	(0.04)	(0.03)	(0.04)	(0.03)
Adjusted R^2	n.a.	0.866	0.737	0.816
	PANEL B: 2002 (N = 739)			
	(B1)	(B2)	(B3)	(B4)
Border Dummy$_{ij}$	-0.175***	-0.146***	-0.150***	-0.194***
	(0.04)	(0.04)	(0.05)	(0.03)
Adjusted R^2	n.a.	0.860	0.715	0.805
	PANEL C: 2007 (N = 768)			
	(C1)	(C2)	(C3)	(C4)
Border Dummy$_{ij}$	-0.175***	-0.134***	-0.144***	-0.199***
	(0.04)	(0.03)	(0.05)	(0.03)
Adjusted R^2	n.a.	0.881	0.743	0.788

Notes: Constant, fixed effects and MR terms not reported. Robust standard errors reported in parenthesis. Pooling over all commodities in 1997 (2002; 2007), we have in column (4) 11,275 (7,721; 12,772) observations. Column (4) includes Importer×Commodity and Exporter×Commodity following Chen (2004). AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. States in Sample as in Table 1. District of Columbia is excluded.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

In a further step, we decompose commodities into different sectors – Agriculture, Mining, Chemical, Machinery, and Manufacturing, a list of classifica-

tions can be found in the Appendix – and estimate the border effect for each sector separately. Results on the sector specific border effects are depicted in Table 12 in the Appendix. Estimates point out that the barrier to trade can primarily be attributed to trade impeding effects in agriculture, chemicals and manufacturing for nearly all years. The impact on mining and machinery products is less determined. We find that agricultural products face on average the strongest border barrier, which reduces North–South trade by 16 to 26.6 percent for agricultural goods.

In Table 13, we try actual travel distance as an alternative measure of distance, as the Department of Transportation reports that in our sample years between 71 and 75 percent of shipments were transported by truck. When we use actual travel distance, we find virtually identical effects compared to the benchmark setting. For 1993, the border reduces North-South trade by 5 to 11.2 percent, compared to a 9 to 20 percent reduction when using the great circle distance. Results for all years are reported in Table 13 in the Appendix. When we exclude intrastate trade from the observations in Table 14 in the Appendix, we find almost identical results with slightly stronger coefficients in magnitude for all years.

To make sure that the excluded border states do not bias our results, we now assign the border states that were formerly excluded from the sample once to the slave states in the South in Table 15 column (1) to (3) and once to the Union in Table 15 column (4) to (6), as they officially never seceded. The results are, as expected, very similar but slightly smaller in magnitude in both cases compared to our original findings in Table 3. This supports our conjecture that it is historically not clear which alliance the border states should be assigned to and underpins our argument to exclude them from the benchmark.

III. Trying to Explain the Border Effect Away

A. Benchmark Results

The Union-Confederation border disappeared in 1965. Nonetheless, our results suggest that the US is still today not a single market. Since it is hard to distinguish between cultural (preference-related) and cost-related (technology or policy) border effects, we do not draw any welfare conclusions. In this section we investigate, whether we can identify variables that explain the border effect away, i.e., whether observable characteristics of state pairs, omitted in the parsimonious AvW-regressions above, bias the estimated coefficient. We include a large number of contemporaneous determinants of trade, that are discussed in the empirical literature, stepwise into the regression. If data is not bilateral in nature, we bilateralize variables by either taking the absolute difference of variables in state i and state j, denoted by the operator Δ, or by using the product of variables in state i and state j, denoted by the operator $\times$. The product of parameters relates to *network* effects between pairs, while the Δ operator focuses on the *difference* between state pairs.[8] Table 5 reports results for our benchmark year 1993. All estimations include origin and destination fixed effects.

Column (1) of Table 5 adds a single geographical variable to the basic setup: adjacency. This variable is routinely included in gravity equations, but does not figure in the AvW setup. Due to the omission of border states from our baseline estimations, the incidence of adjacency is smaller for pairs affected by the border. If adjacency increases trade, its omission would bias the border effect away from zero. This is exactly what we find: the border effect falls (in absolute terms) from -0.157 (Table 3 column (2)) to -0.115. In accordance with the literature, adjacency increases bilateral trade by about 45 percent.[9]

In column (2) we account for the impact of ethnic, religious, or cultural networks (Rauch 1999; Rauch and Trindade 2002; Combes et al. 2005) and migration within the US (Helliwell 1997; Head and Ries 1998; Millimet and Osang

[8]We tried a range of other variables and combinations, as well as network and difference parameters separately and combinations thereof. The results are robust to these modifications.

[9]Clearly, not including adjacency also biases the distance coefficient upwards.

2007). Table 2 shows that the North and the South differ along racial and religious lines. The literature reasons that common culture and tastes increase trade flows because the facilitate the conclusion of contracts and instil trust and mutual understanding. Hence, networks reduce informational, transaction and search costs. Migration and networks might matter as they increase trade but are negatively associated with the border. We thus expect the border coefficient to be overestimated if migration and network effects are omitted. To test the impact of networks we include (i) cross-state migration stocks of people residing in one state but were born in another (taken from the *American Community Survey Decennial Census*, 1990 and 2000); (ii) the product of the share of Afro-Americans in total state population (stemming from the *Population Estimates Program*); (iii) the product of the Jewish population in total state population (from the *American Jewish Yearbook*); and (iv) self-reported affinity to Christianity, other religious groups, or no religion (from *Religious Identification and Social Change – ARIS 2008 Report*), into the estimation. We find that migration networks, high shares of Afro-Americans, of population shares affiliated to Buddhism, Hinduism or Islam, and of people not self-identifying with any religious group spur trade flows. A 1 percent increase in the bilateral migration stock indicates an increase in trade by 22 percent in column (2).[10] If we include network controls, the border still reduces trade by 11.7 percent (not shown in Table 5). This can explicitly be attributed to networks of blacks. The majority of Afro-Americans still live in the South, which indicates that trade among the states of the South is much stronger than that with the North.

Column (2) also contains a variable measuring home bias. Specifically, we follow the literature and include an indicator variable that is unity for within state trade and zero otherwise. The estimate is significant in column (2) and suggests that trade is on average 24 percent larger within a state than across states. Our estimate is half the size what is on average found in the literature on the US, using identical data but more parsimonious models (Wolf 2000; Hill-

[10] A similar effect has been identified by Combes et al. (2005) for trade within France.

berry and Hummels 2003; Millimet and Osang 2007; Coughlin and Novy 2009). The home bias effect relates to informational frictions, such as transaction and search costs, that lead to spacial clustering of economic activity within states. However, as we control for networks that partly capture these determinants the home bias effect is strongly reduced. In addition, common colonial heritage, also included in column (2), is widely discussed in the cross-country trade literature. As the US was colonized by different colonial powers, we are also able to control for the impact of colonial heritage within the United States. To capture that state characteristics and particularly institutions were partly shaped by colonial times, we construct an indicator variable for common colonizers – Britain, France and Spain. We find that a common colonizer significantly increases bilateral trade between a pair of states by about 19 percent. While most of those network variables matter statistically, they do not reduce the estimated border effect. If at all, they leave it slightly higher (at 12.4 percent).

Column (3) examines the impact of labor market and political institutions. We control for labor market institutions by including dissimilarities in union membership and density from Hirsch et al. (2001), as well as a dummy for the existence of minimum wage legislation provided the US Department of Labor. In theory, differences in labor market institutions (union coverage, union density, the existence of minimum wage legislation) could increase bilateral trade, because differential legislation acts as a source of comparative advantage (Cunat and Melitz, 2009). In our analysis, we find that institutional differences tend to reduce trade (albeit statistical precision of estimates is low). This may signal that institutional differences are caused by some deeper differences in cultural norms and that the latter discourage trade by more. Column (3) also controls for differences in the political alignment in the 1992 presidential election (Bill Clinton against George Bush sen.) and whether states elect or appoint the judiciary. Voting behavior has no statistically measurable effect on trade, while the difference in judiciary appointment procedure turns out to depress bilateral trade flows. The estimated border effect, however, remains virtually unchanged.

Column (4) includes controls for the difference in relative factor endowments of states – premising on Heckscher-Ohlin trade theory. Omitting differences in the endowment structure or factor proportions might lead to an upward bias of the border coefficient, as differences in factor proportions should increase trade flows and appear to be more pronounced when the border is present (see Table 9). To measure contemporaneous differences in relative factor proportions and human capital accumulation, we include the absolute difference in (i) capital-labor shares from Turner et al. (2008); (ii) shares of high and low skilled in the population[11]; (iii) average years of schooling for the population over 25 from Turner et al. (2007); (iv) cropland from the *National Resource Inventory Summary Report*; (v) average farm size from the *Census of Agriculture* ; (vi) agricultural relative to total output; and (vii) manufacturing relative to total output from the Bureau of Economic Analysis. As in other gravity exercises, classical Heckscher-Ohlin variables do not show up statistically significant, though both the parameters on the difference in the capital-labor ratio and the difference in relative skill endowment bear the right sign. Differences in the availability of crop land reduce bilateral trade flows, as do differences in the share of manufacturing output. Contemporaneous differences in factor endowments do not capture the border, which still reduces North-South trade by 12.2 percent in this setup.

Column (5) includes demographic variables such as the difference in contemporaneous population and population density from the *Population Estimates Program*, as well as fertility rates from the *Vital Statistics of the United States*. Common demographic features across states may suggest common preferences, so that bilateral trade is larger for such states. The estimated parameters, however, are insignificant throughout. The estimated border effect remains negative and significant.

Finally, following the literature on the Linder effect ((Thursby and Thursby

[11]We measure high skilled by a Bachelor's degree or above and low skilled by a High School degree or below. Data is collected from the *Census of Population* and the *American Community Survey*.

1987; Bergstrand 1989; Hallak 2010), we include the difference in the log of per capita income. The hypothesis is that states with dissimilar GDP per capita should have dissimilar preference structures and, hence, trade less. Since the border correlates negatively with GDP per capita in the data, omitting the Linder term may bias the estimate of the border effect away from zero. This is, however, not what we find. In column (6), we fail to find support for the Linder hypothesis; the estimated border effect does not move.

Column (6) represents our most comprehensive, hence preferred, model. There, the border effect is 13 percent. It explains more than 91 percent of the variation in bilateral trade patterns, 85 percent of which are attributable to included parameters and controls. A model that explains bilateral trade solely using importer and exporter fixed effects can only explain 6 percent of the variation in the dependent variable.

B. Sensitivity Analysis

Table 6 summarizes sensitivity analysis pertaining to the comprehensive model in column (6) of Table 5; details are relegated to Table 16 in the Appendix. Panel A deploys the FE approach. Our baseline border effect of -0.130 is reported in column (A1). We find a negative and significant border effect for 1993 and 2002, while the effect for 1997 and 2007 remains negative but insignificant. Results based on the commodity flow survey from 1997 onwards suffer from the fact that the number of firms surveyed is only 50 or 25 percent of those surveyed in 1993. In Panel B we turn to our model that includes MR terms directly in the estimation. The border barrier turns out to be strong in 1993 and 1997 using the MR approach. If we use the pooled commodity FE setup with importer$\times$commodity and destination$\times$commodity fixed effects following Chen (2004) in Panel C we find strong trade impeding effect for all years. Overall, we can conclude that the findings on the border effect compare well, both qualitatively and quantitatively, to our earlier results. The border reduces cross-border trade by 7 to 19 percent, depending on the year and the specification.

TABLE 5 – CONTEMPORANEOUS CONTROLS, 1993 (fixed-effects estimation)

	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy$_{ij}$	-0.115***	-0.124***	-0.126***	-0.122***	-0.132***	-0.130***
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Geographical Controls						
ln Distance$_{ij}$	-0.980***	-0.580***	-0.570***	-0.562***	-0.548***	-0.550***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Adjacency$_{ij}$	0.446***	0.335***	0.341***	0.362***	0.381***	0.384***
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Network Controls/Home Bias						
ln Migration Stock$_{ij}$		0.182***	0.179***	0.151***	0.150***	0.147***
		(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
×Black Share$_{ij}$		0.000**	0.001***	0.001***	0.001**	0.001**
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
×Jewish Share$_{ij}$		-0.009**	-0.008*	-0.006	-0.006	-0.006
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
×Christian Share$_{ij}$		0.001	0.002	0.001	0.002	0.002
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
×Other Religion Share$_{ij}$		0.051**	0.053**	0.051**	0.041	0.041
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
×No Religion Share$_{ij}$		0.011**	0.011**	0.011**	0.009*	0.009*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
×Urban Share$_{ij}$		4.001***	3.860***	4.317***	4.344***	4.369***
		(0.74)	(0.77)	(0.90)	(1.12)	(1.12)
Home Bias$_{ij}$		0.243**	0.290**	0.360***	0.349***	0.362***
		(0.12)	(0.12)	(0.13)	(0.13)	(0.13)
Common Colonizer$_{ij}$		0.186***	0.193***	0.168***	0.166***	0.167***
		(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Labor Market/Political Institutions						
ΔUnion Membership$_{ij}$			-0.027	-0.031	-0.036*	-0.038*
			(0.02)	(0.02)	(0.02)	(0.02)
ΔUnion Density$_{ij}$			0.029	0.033	0.038*	0.040*
			(0.02)	(0.02)	(0.02)	(0.02)
ΔMinimum Wage$_{ij}$			-0.210	-0.253*	-0.225	-0.223
			(0.15)	(0.15)	(0.15)	(0.15)
ΔRepublican$_{ij}$			0.001	-0.000	0.001	0.001
			(0.03)	(0.03)	(0.03)	(0.03)
Judiciary Election$_{ij}$			-0.058*	-0.066**	-0.065**	-0.064**
			(0.03)	(0.03)	(0.03)	(0.03)
Heckscher-Ohlin Controls						
Δ ln Capital-Labor Ratio$_{ij}$				0.167	0.162	0.119
				(0.15)	(0.15)	(0.18)
Δ ln High-Low Skilled Ratio$_{ij}$				0.059	0.062	0.065
				(0.09)	(0.09)	(0.09)
Δ ln Average Schooling$_{ij}$				-0.924	-1.016	-1.236
				(1.12)	(1.13)	(1.24)
Δ ln Cropland$_{ij}$				-0.048***	-0.045***	-0.045***
				(0.02)	(0.02)	(0.02)
Δ ln Farm Size$_{ij}$				0.050	0.024	0.020
				(0.05)	(0.07)	(0.07)
Δ ln Agricultural to Total Output$_{ij}$				0.027	-0.002	-0.002
				(0.04)	(0.04)	(0.04)
Δ ln Manufacturing to Total Output$_{ij}$				-0.203**	-0.177*	-0.155
				(0.10)	(0.10)	(0.11)
Demography						
Δ ln Population$_{ij}$					-0.029	-0.030
					(0.03)	(0.03)
Δ ln Population Density$_{ij}$					0.039	0.039
					(0.04)	(0.04)
Δ ln Fertility$_{ij}$					-0.493	-0.466
					(0.41)	(0.41)
Linder Hypothesis						
Δ ln Income per Capita$_{ij}$						0.132
						(0.28)
Adjusted R^2	0.884	0.909	0.909	0.911	0.911	0.911
F-Test	75.74	140.97	130.74	127.53	122.69	121.30

Notes: Importer and exporter fixed effects included in all regressions. Constant and fixed effects not reported. Robust standard errors reported in parenthesis. The operator Δ denotes the absolute difference of variables in state i and state j. The operator × denotes the product of variables in state i and state j. As before, we have N = 768 observations in the model.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 6 – CONTROLS, ALTERNATIVE SAMPLES AND MODELS: SUMMARY RESULTS

Year of Data	1993	1997	2002	2007
PANEL A: FIXED EFFECTS				
	(A1)	(A2)	(A3)	(A4)
Border Dummy$_{ij}$	-0.130***	-0.056	-0.119*	-0.008
	(0.04)	(0.05)	(0.06)	(0.06)
Observations	768	766	739	768
Adjusted R^2	0.911	0.904	0.893	0.914
PANEL B: OLS WITH MR TERMS				
	(B1)	(B2)	(B3)	(B4)
Border Dummy$_{ij}$	-0.114**	-0.201***	-0.092	-0.045
	(0.04)	(0.05)	(0.07)	(0.06)
Observations	768	766	739	768
Adjusted R^2	0.853	0.866	0.829	0.852
PANEL C: POOLED COMMODITY FE (Chen, 2004)				
	(C1)	(C2)	(C3)	(C4)
Border Dummy$_{ij}$	-0.215***	-0.118***	-0.072*	-0.126***
	(0.04)	(0.03)	(0.04)	(0.04)
Observations	13,303	11,275	7,721	12,772
Adjusted R^2	0.655	0.836	0.822	0.804

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include parameters of Table 5 column (6) as additional controls. Full results are reported in Table 16 in the Appendix.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

When we decompose the trade data into sectors and include the additional controls, results suggests that the trade impeding effect is mainly caused by barriers to manufacturing products in all years. Compared to our earlier results, the border effect is negative but less robust for agriculture and chemicals – except for 2002. Mining and machinery products again depict in most cases an indistinguishable coefficient from zero. Table 17 in the Appendix depicts the results.

IV. Accounting for the Endogeneity of the Secession

A. Benchmark Results

The economic literature on the emergence of military conflict shows that strong bilateral trade links decrease the probability that two countries go to war, while multilateral openness increases the odds of conflict (Martin, Mayer, and Thoenig, 2008). If determinants of bilateral trade are persistent over time, the border could not be considered exogenous in the statistical sense.[12]. Historical bilateral trade data is, however, not available. But, one can include historical variables that may, through their impact on historical trade patterns, affect the probability of conflict (and, hence, the incidence of the border). Moreover, through persistent effects on, e.g., institutions, history might affect contemporaneous trade flows (Eichengreen and Irwin 1998). We therefore include historical variables into the model.Absolute differences in historical variables are positively correlated to the border, as the Secession and the subsequent Civil War were strongly related to dissimilarities in endowments and the economic structure between the North and the South. In addition, it is reasonable to believe that historical determinants have had an impact on the development of institutions. The settlement structure might have induced networks along cultural lines that survived over time. The discussion is related to the literature on the long-term impact of factor endowments and institutions (Engermann and Sokoloff 2000, 2005; Acemoglu et al. 2002; Nunn 2008; Galor et al. 2009). Historical variables derived from the 1860 census are also directly depicted in Table 2 for Northern and Southern states.

We proceed by including controls that capture initial conditions and structural differences among US states related to the Secession episode in Table 7. All models include our additional contemporaneous controls from Table 5 column (6) and importer as well as exporter fixed effects. According to Engerman and Sokoloff (2000, 2005), dissimilarities in land endowments may have perpetuated income inequality, which could be relevant for contemporaneous trade. Moreover, differences in land endowments were crucial for the emergence of

[12] See Heinemeyer et al. (2008) on endogenous borders.

TABLE 7 – CONTEMPORANEOUS AND HISTORICAL CONTROLS, 1993 (fixed-effects estimation)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Border Dummy$_{ij}$	-0.230***	-0.132***	-0.159***	-0.155*	-0.130***	-0.141***	-0.235**
	(0.07)	(0.04)	(0.06)	(0.08)	(0.04)	(0.04)	(0.10)
Controls as of Table 5 column (6) included	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Historical Controls							
$\Delta \ln$ 1860 Cropland$_{ij}$	-0.023						-0.032
	(0.02)						(0.02)
$\Delta \ln$ 1860 Farm Size$_{ij}$	0.138*						0.117
	(0.08)						(0.09)
$\Delta \ln$ 1860 Population Density$_{ij}$		0.022					0.017
		(0.02)					(0.02)
$\Delta \ln$ 1860 Illiteracy Rates$_{ij}$			0.004				0.008
			(0.00)				(0.00)
Δ1860 Slave Share$_{ij}$				0.001			-0.002
				(0.00)			(0.00)
Δ1860 Free Black Share$_{ij}$				0.040**			0.049**
				(0.02)			(0.02)
$\times$1860 French Share$_{ij}$					0.404***		0.420***
					(0.14)		(0.15)
$\times$1860 Spanish Share$_{ij}$					-10.084		-9.972
					(9.02)		(8.97)
$\times$1860 Irish Share$_{ij}$					-0.003**		-0.003**
					(0.00)		(0.00)
$\times$1860 German Share$_{ij}$					0.002*		0.001
					(0.00)		(0.00)
$\times$1860 British Share$_{ij}$					0.004*		0.005*
					(0.00)		(0.00)
Δ1860 Malaria Risk$_{ij}$						0.180	0.143
						(0.25)	(0.30)
Observations	768	768	768	768	768	768	768
Adjusted R^2	0.911	0.911	0.911	0.911	0.914	0.911	0.915
F-Test	120.54	119.90	122.90	120.56	131.88	120.23	136.75

Notes: Importer and exporter fixed effects included in all regressions. Constant and fixed effects not reported. Robust standard errors reported in parenthesis. The operator Δ denotes the absolute difference of variables in state i and state j. The operator $\times$ denotes the product of variables in state i and state j. All models include parameters as of column (6), Table 5 as additional controls.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

conflicting interests between the North and the South. We have experimented with direct measures of inequality (Gini coefficients), but without success. Historical factors may also correlate do cultural differences that have triggered the Civil War and that are still relevant for bilateral trade today. To account for this possibilities, we include historical differences (along with the contemporaneous ones; not shown) in (i) cropland; (ii) average farms size; (iii) population density; and (iv) illiteracy rates of the non-slave population. In column (1) to (3), we find that none of these variables matter statistically, except for historical farm size differences which are significant on the 10 percent level. Including farm size increases rather then decreases the border coefficient to 23 percent. This is surprising as historical farm size differences correlate positively with the border.

Slavery was one issue that led to the split of the nation, as the South with its large plantations relied heavily on slave labor while the industrialized North did not. Hence, we expect the legacy of slavery to partly capture the border barrier in column (4). However, we find that differences in the shares of slaves in 1860 exert no impact on bilateral trade patterns and do not explain away the border barrier.[13] Interestingly, the inclusion of the absolute difference in shares of free blacks in 1860 exerts a positive and significant effect on contemporaneous trade patterns.

In addition, similarities in culture due to similar settlement structures in US states before the war could have induced business networks that have survived over time and still affect trade. We therefore include the product in the shares of French, Spanish, Irish, British and German settlers in 1860. While Spanish heritage has no particular impact on trade, Irish heritage decreases bilateral trade significantly in column (5). States with a large share of French settlers trade more amongst each other. The same is true for states with high shares of

[13]If we use the difference in the share of slaves in 1840, rather than that in 1860, where there where still slaves also living in the North, we still find robust results on the border effect but an insignificant coefficient close to zero for the slave share. In column (7), the effect of differences in 1840 slaves is still zero, while that of the illiterate of the non-slave population turns significant and positive. The border effect remains negative and significant on the 1 percent level.

German or British settlers, but the impact of German networks vanishes with the inclusion of further parameters in subsequent columns.

According to Acemoglu, Johnson and Robinson (2002), historical climatic differences measured by the incidence of malaria, may have affected the characteristics and quality of institutions. In the present case, it is conceivable that the high risk of malaria in the South has led to acceptance of slavery by the local elite and may therefore constitute a deep reason for the conflict. It may also, through its lasting effect on institutions, affect contemporaneous trade flows. So, we include the malaria risk index in 1860 from Hong (2007). We find neither a significant effect on trade nor does historical climate explain away the border. In the last column, we include all historical controls simultaneously in our model. All in all, we find that the border reduces trade by 23.5 percent, even when we include historical determinants that we intend to capture initial structural differences between the alliances.

B. Treated Versus Non-Treated

From the previous analysis, one cannot conclude that the Secession has *caused* the observed border effect in contemporaneous trade data. Including historical variables, that relate to the deep reasons for the Civil War, into the model, goes some way in dealing with reverse causation. However, it fails to account for unobserved shocks that both make the odds for Secession and today's bilateral trade flows larger. Unfortunately, no instrument is ready-to-use in an IV approach.

One way to nudge the analysis closer to identifying a causal effect consists in separating the *whole* of the US – including the West – into states that underwent a treatment by the Secession and the Civil War and states that were not treated by these historical events. We separate the states into three groups – the North, the South, and the West –, still excluding border states, the District of Columbia, Alaska and Hawaii.[14] The border dummy is unity for states that

[14]West includes all US states that were not assigned to the North, the South or the border

found themselves on opposite sides of the Civil War and zero for all other pairs of states. Adding Western states increases the control set in that it adds state pairs that are not so much characterized by their belonging to North or South than by their absence of a past shaped by the Civil War.

TABLE 8 – ADDITIONALLY INCLUDING THE WEST, 1993

Specification	Fixed Effects		OLS with MR Terms	
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.066*		-0.129***	
	(0.03)		(0.04)	
South–South Dummy$_{ij}$		0.260***		0.181***
		(0.06)		(0.06)
North–North Dummy$_{ij}$		-2.592**		0.342***
		(1.21)		(0.03)
West–West Dummy$_{ij}$		-0.014		0.294***
		(0.08)		(0.08)
ln Distance$_{ij}$	-0.372***	-0.370***	-0.653***	-0.570***
	(0.05)	(0.05)	(0.03)	(0.03)
Additional Controls	YES	YES	YES	YES
Observations	2,137	2,137	2,137	2,137
Adjusted R^2	0.845	0.846	0.725	0.736
F-Test	105.35	105.63	192.88	190.51

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include parameters as of column (7), Table 5 available for all US states as additional controls.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 8 reports the results. All models include additional contemporaneous controls.[15] In columns (1) and (3), we find for the FE regressions and for OLS with MR terms a significant trade impeding effect of the Secession treatment. The effect ranges between 6 and 12 percent. In addition, we again find in column (2) that the South trades more amongst each other while the North trades less – this effect is much stronger than when we exclude the West from the sample. There seems not to be any particular trade effect within Western

states in Table 1, excluding the District of Columbia, Alaska and Hawaii.

[15]Historical controls are not available for most of the Western states before the war, as these were only Territories in 1860.

states. The picture looks different when we directly control for multilateral resistance in column (4). In this setup, all regions trade significantly more among themselves than with states of the other regions.[16]

V. Concluding Remarks

This paper supports the conjecture that the economic legacy of history in the US has all but faded. The defunct border within the US, caused by cultural and endowment differences that led to the Secession and the subsequent Civil War, still has an effect on within US trade today. We find substantial evidence of a trade impeding effect of the border between states of the former Union and the Confederacy. In placebo estimations, we find supportive evidence that the trade hampering effect is unique to North-South state pairs.

Our analysis assesses the effect of the border on bilateral trade patterns. We use a gravity approach to identify the impact of the border between the North and the South in the US. We find that the border reduces cross-border trade by 6 to 20 percent on the sub-national level that is unique to the state groups. This indicates that the border barrier is equivalent to a tariff of 1 to 8 percent. We find that the trade barriers in mainly caused by frictions in the shipment of manufacturing products. The result is robust to various years, econometric methods, the inclusion of several controls related to historical and contemporaneous dissimilarities between the North and the South, and the separation into treated and untreated pairs. Two observations stand out: the border effect relates massively to networks of blacks, which signals a discrimination issue within the US; and, including historical farm size differences into the estimation inflates the trade hampering effect, which relates well to the literature on the long-run impact of endowment differences.

[16] Results are pretty similar for the other years and can be found in Table 18 in the Appendix.

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Appendix

TABLE 9 – SUMMARY STATISTICS AND DATA SOURCES, 1993

Unit of Observation: Pairs of States					
Sample	Full (N = 768)		North–South (N = 364)		Data Source
Variable	Mean	St. Dev.	Mean	St. Dev.	
$\ln z_{ij}$	-16.130	1.084	-16.590	0.637	Commodity Flow Survey; Bureau of Economic Analysis.
Border$_{ij}$	0.474	0.5	1.000	0.000	own calculations.
$\ln$ Dist$_{ij}$	6.736	0.855	7.131	0.410	Anderson and van Wincoop (2003).
Adjacency$_{ij}$	0.147	0.354	0.000	0.000	own calculations.
$\ln$ Migration Stock$_{ij}$	9.936	1.785	9.501	1.524	American Community Survey.
$\times$Black Share$_{ij}$	184.306	211.97	172.135	141.202	Population Estimates Program.
$\times$Jewish Share$_{ij}$	2.545	6.192	1.665	4.180	The American Jewish Yearbook.
$\times$Christian Share$_{ij}$	7877.5	475.79	7961.91	375.65	ARIS 2008 Report.
$\times$Other Religion Share$_{ij}$	1.096	1.056	1.038	0.888	ARIS 2008 Report.
$\times$No Religion Share$_{ij}$	43.612	21.336	38.286	15.999	ARIS 2008 Report.
$\times$Urban Share$_{ij}$	0.490	0.146	0.478	0.133	Census of Population and Housing.
Home Bias$_{ij}$	0.036	0.188	0.000	0.000	own calculations.
Colonizer$_{ij}$	0.564	0.496	0.530	0.500	own calculations.
$\Delta \ln$ 1860 Cropland$_{ij}$	1.028	0.827	1.028	0.776	Census of Agriculture 1860.
$\Delta \ln$ 1860 Farm Size$_{ij}$	0.667	0.529	1.150	0.328	Census of Agriculture 1860.
$\Delta \ln$ 1860 Population Density$_{ij}$	1.356	1.112	1.477	1.059	Census of Population and Housing 1860.
$\Delta \ln$ 1860 Illiteracy Rates$_{ij}$	6.216	5.179	9.897	4.690	Census of Population and Housing 1860.
Δ1860 Slave Share$_{ij}$	20.724	20.236	39.662	10.869	Census of Population and Housing 1860.
Δ1860 Free Black Share$_{ij}$	1.155	1.039	1.212	1.046	Census of Population and Housing 1860.
$\times$1860 French Share$_{ij}$	0.080	0.227	0.754	0.215	Census of Population and Housing 1860.
$\times$1860 Spanish Share$_{ij}$	0.000	0.002	0.000	0.001	Census of Population and Housing 1860.
$\times$1860 Irish Share$_{ij}$	20.582	34.582	6.217	8.991	Census of Population and Housing 1860.
$\times$1860 German Share$_{ij}$	10.662	22.872	4.229	8.391	Census of Population and Housing 1860.
$\times$1860 British Share$_{ij}$	7.241	11.656	1.287	1.124	Census of Population and Housing 1860.
Δ1860 Malaria Risk$_{ij}$	0.144	0.106	0.224	0.088	Hong (2007).
$\Delta \ln$ Capital-Labor Ratio$_{ij}$	0.271	0.215	0.277	0.210	Turner et al. (2008).
$\Delta \ln$ High-Low Skilled Ratio$_{ij}$	0.434	0.312	0.560	0.324	Census of Population; American Community Survey.
$\Delta \ln$ Average Schooling$_{ij}$	0.034	0.025	0.044	0.027	Turner et al. (2007).
$\Delta \ln$ Cropland$_{ij}$	1.855	1.619	1.895	1.380	National Resource Inventory Summary Report.
$\Delta \ln$ Farm Size$_{ij}$	0.551	0.463	0.561	0.466	Census of Agriculture.
$\Delta \ln$ Agricultural To Total Output$_{ij}$	0.682	0.511	0.709	0.488	Bureau of Economic Analysis.
$\Delta \ln$ Manufacturing To Total Output$_{ij}$	0.318	0.258	0.338	0.260	Bureau of Economic Analysis.
$\Delta \ln$ Population$_{ij}$	0.960	0.744	0.947	0.702	Population Estimates Program.
$\Delta \ln$ Population Density$_{ij}$	1.063	0.82	1.092	0.770	Population Estimates Program.
$\Delta \ln$ Fertility$_{ij}$	0.077	0.065	0.082	0.069	Vital Statistics of the United States.
$\Delta \ln$ Income Per Capita$_{ij}$	0.144	0.119	0.158	0.128	Bureau of Economic Analysis; Population Estimates Program.
ΔUnion Membership$_{ij}$	7.376	5.54	10.044	5.499	Hirsch et al. (2001).
ΔUnion Density$_{ij}$	7.198	5.375	9.784	5.413	Hirsch et al. (2001).
ΔMinimum Wage$_{ij}$	0.083	0.121	0.083	0.105	US Department of Labor.
ΔRepublican$_{ij}$	0.434	0.496	0.604	0.490	The American Presidency Project.
Judiciary Election$_{ij}$	0.452	0.498	0.434	0.496	own calculations.

Notes: Data from the Bureau of Economic Analysis stem from the Regional Economic Accounts. Contemporaneous variables if not stated otherwise. The operator Δ denotes the absolute difference of variables between state i and state j. The operator $\times$ denotes the product of variables in state i and state j.

TABLE 10 – PLACEBO COAST-INTERIOR AND EAST-WEST, 1993

	Coast-Interior (N = 2,137)			East-West (N = 2,137)		
Specification	AvW NLS	Fixed Effects		AvW NLS	Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy$_{ij}$	0.021	-0.015		0.113***	-0.037	
	(0.04)	(0.03)		(0.03)	(0.03)	
Coast-Coast$_{ij}$/East-East Dummy$_{ij}$			-0.244*			0.491***
			(0.14)			(0.16)
Interior-Interior$_{ij}$/West-West Dummy$_{ij}$			0.273**			0.075
			(0.14)			(0.06)
ln Distance$_{ij}$	-0.860***	-1.220***	-1.220***	-0.865***	-1.211***	-1.211***
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Fixed Effects						
Importer	n.a.	YES	YES	n.a.	YES	YES
Exporter	n.a.	YES	YES	n.a.	YES	YES
Adjusted R^2	n.a.	0.788	0.788	n.a.	0.788	0.788
F-Test	n.a.	67.39	67.39	n.a.	67.45	67.45

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Coast: Connecticut, California, Delaware, Florida, Georgia, Maine, Massachusetts, Maryland, New Hampshire, New Jersey, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, South Carolina, Virginia, Vermont, Washington. Interior: Alabama, Arizona, Arkansas, Colorado, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, South Dakota, Tennessee, Texas, Utah, West Virginia, Wisconsin, Wyoming. West: Arizona, Arkansas, California, Colorado, Idaho, Iowa, Kansas, Louisiana, Minnesota, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, South Dakota, Texas, Utah, Washington, Wyoming. East: Alabama, Connecticut, Delaware, Florida, Georgia, Illinois, Indiana, Iowa, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, New Hampshire, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Vermont, Virginia, West Virginia, Wisconsin. District of Columbia, Hawaii and Alaska excluded. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. Significance levels as in Table 3.

TABLE 11 – SENSITIVITY ANALYSIS VARIOUS YEARS

Data	Aggregated			Commodity
Specification	AvW NLS	FE	OLS with MR Terms	Chen (2004) FE
Year of Data		**1997** (N = 766)		
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.128***	-0.091***	-0.126***	-0.138***
	(0.04)	(0.03)	(0.04)	(0.02)
ln Distance$_{ij}$	-0.978***	-1.104***	-1.032***	-1.140***
	(0.03)	(0.03)	(0.04)	(0.01)
Adjusted R^2	n.a.	0.866	0.737	0.816
F-Test	n.a.	54.69	265.69	n.a.
Year of Data		**2002** (N = 739)		
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.175***	-0.146***	-0.150***	-0.194***
	(0.04)	(0.04)	(0.05)	(0.02)
ln Distance$_{ij}$	-1.071***	-1.136***	-1.066***	-1.091***
	(0.03)	(0.03)	(0.04)	(0.01)
Adjusted R^2	n.a.	0.860	0.715	0.805
F-Test	n.a.	62.11	265.44	n.a.
Year of Data		**2007** (N = 768)		
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.175***	-0.134***	-0.144***	-0.199***
	(0.04)	(0.03)	(0.05)	(0.02)
ln Distance$_{ij}$	-1.087***	-1.180***	-1.116***	-1.216***
	(0.03)	(0.03)	(0.04)	(0.01)
Adjusted R^2	n.a.	0.881	0.743	0.788
F-Test	n.a.	70.23	307.61	n.a.
Fixed Effects				
Importer	n.a.	YES	-	-
Exporter	n.a.	YES	-	-
Importer×Commodity	n.a.	-	-	YES
Exporter×Commodity	n.a.	-	-	YES
Multilateral Resistance	n.a.	-	YES	-

Notes: Constant, fixed effects and MR terms not reported. n.a. means not applicable. Robust standard errors reported in parenthesis. States in sample as in Table 1. District of Columbia is excluded. Pooling over all commodities in 1997 (2002; 2007), we have in column (4) 11,275 (7,721; 12,772) observations. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. Significance levels as in Table 3.

TABLE 12 – SECTORAL (fixed-effects estimation)

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
Year of Data			**1993**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.309***	0.022	-0.227***	-0.022	-0.068
	(0.08)	(0.23)	(0.08)	(0.07)	(0.06)
ln Distance$_{ij}$	-1.346***	-1.595***	-1.209***	-1.047***	-1.089***
	(0.04)	(0.08)	(0.05)	(0.05)	(0.04)
Observations	4,815	1,336	3,078	4,324	11,990
Adjusted R^2	0.746	0.757	0.671	0.657	0.757
Year of Data			**1997**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.244***	-0.303*	-0.095	-0.072	-0.200***
	(0.08)	(0.17)	(0.06)	(0.06)	(0.05)
ln Distance$_{ij}$	-1.600***	-2.119***	-1.308***	-1.176***	-1.168***
	(0.06)	(0.09)	(0.05)	(0.05)	(0.04)
Observations	5,490	2,655	3,215	3,455	7,620
Adjusted R^2	0.783	0.774	0.755	0.733	0.803
Year of Data			**2002**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.176*	-0.210	-0.216***	0.005	-0.271***
	(0.09)	(0.36)	(0.08)	(0.07)	(0.06)
ln Distance$_{ij}$	-1.469***	-2.028***	-1.264***	-1.138***	-1.252***
	(0.05)	(0.11)	(0.05)	(0.05)	(0.04)
Observations	4,470	1,629	2,820	3,205	7,080
Adjusted R^2	0.773	0.762	0.722	0.695	0.774
Year of Data			**2007**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.308***	-0.101	-0.302***	-0.020	-0.277***
	(0.07)	(0.16)	(0.06)	(0.07)	(0.05)
ln Distance$_{ij}$	-1.594***	-2.338***	-1.246***	-1.168***	-1.263***
	(0.05)	(0.07)	(0.04)	(0.05)	(0.04)
Observations	4,171	1,914	3,116	3,472	7,436
Adjusted R^2	0.816	0.813	0.766	0.679	0.808

Notes: Importer and exporter fixed effects included in all regressions. Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Commodities pooled into sectors as listed in Table 19 and 20 in the Appendix. Significance levels as in Table 3.

TABLE 13 – ALTERNATIVE DISTANCE MEASURE (fixed-effects estimation)

Year of Data	1993		1997		2002		2007	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Border Dummy$_{ij}$	-0.119***		-0.053*		-0.105***		-0.093***	
	(0.03)		(0.03)		(0.04)		(0.03)	
South-South Dummy$_{ij}$		0.458***		0.172		0.514***		0.103
		(0.10)		(0.11)		(0.14)		(0.12)
North-North Dummy$_{ij}$		-0.220**		-0.066		-0.304**		0.082
		(0.09)		(0.10)		(0.13)		(0.11)
ln Travel Distance$_{ij}$	-1.156***	-1.156***	-1.149***	-1.149***	-1.184***	-1.184***	-1.230***	-1.230***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Fixed Effects								
Importer	YES	YES	YES	YES	YES	YES	YES	YES
Exporter	YES	YES	YES	YES	YES	YES	YES	YES
Observations	768	768	766	766	739	739	768	768
Adjusted R^2	0.878	0.878	0.869	0.869	0.864	0.864	0.886	0.886
F-Test	61.88	61.88	57.56	57.56	63.97	63.97	72.35	72.35

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. True travel distance between states – obtained from Google – used as distance measure. Significance levels as in Table 3.

TABLE 14 – VARIOUS YEARS W/O INTRASTATE TRADE (fixed-effects estimation)

Year of Data	1993		1997		2002		2007	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Border Dummy$_{ij}$	-0.177***		-0.109***		-0.156***		-0.148***	
	(0.03)		(0.03)		(0.03)		(0.03)	
South-South Dummy$_{ij}$		0.685***		0.290***		0.690***		0.292**
		(0.10)		(0.10)		(0.21)		(0.13)
North-North Dummy$_{ij}$		-0.330***		-0.072		-0.378*		0.004
		(0.09)		(0.10)		(0.21)		(0.12)
ln Distance$_{ij}$	-1.045***	-1.045***	-1.033***	-1.033***	-1.100***	-1.100***	-1.134***	-1.134***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Fixed Effects								
Importer	YES	YES	YES	YES	YES	YES	YES	YES
Exporter	YES	YES	YES	YES	YES	YES	YES	YES
Observations	740	740	738	738	711	711	740	740
Adjusted R^2	0.826	0.826	0.806	0.806	0.806	0.806	0.836	0.836
F-Test	61.05	61.05	48.50	48.50	53.44	53.44	67.76	67.76

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Intrastate trade excluded from observations. Significance levels as in Table 3.

TABLE 15 – SENSITIVITY ANALYSIS: ALLOCATION OF BORDER STATES, 1993

	Border States in South			Border States in North		
Specification	AvW NLS	Fixed Effects		AvW NLS	Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy$_{ij}$	-0.154***	-0.104***		-0.150***	-0.131***	
	(0.03)	(0.03)		(0.04)	(0.03)	
South-South Dummy$_{ij}$			0.659***			0.263***
			(0.10)			(0.06)
North-North Dummy$_{ij}$			0.208***			-0.331***
			(0.05)			(0.09)
ln Distance$_{ij}$	-0.983***	-1.117***	-1.117***	-0.987***	-1.112***	-1.112***
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Fixed Effects						
Importer	n.a.	YES	YES	n.a.	YES	YES
Exporter	n.a.	YES	YES	n.a.	YES	YES
Observations	1,057	1,057	1,057	1,057	1,057	1,057
Adjusted R^2	n.a.	0.845	0.845	n.a.	0.846	0.846
F-Test	n.a.	66.75	66.75	n.a.	68.03	68.03

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Column (1) to (3) allocates border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to South as listed in Table 1. North as in Table 1. Column (4) to (6) allocates border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to North as listed in Table 1. South as in Table 1. District of Columbia excluded. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. Significance levels as in Table 3.

TABLE 16 – ADDITIONAL CONTROLS, ALTERNATIVE SAMPLES AND MODELS: SUMMARY RESULTS

Year of Data	1993			1997			2002			2007		
Data	Aggregated		Commodity	Aggregated		Commodity	Aggregated		Commodity	Aggregated		Commodity
Specification	FE	OLS with MR Terms	FE Chen (2004)	FE	OLS with MR Terms	FE Chen (2004)	FE	OLS with MR Terms	FE Chen (2004)	FE	OLS with MR Terms	FE Chen (2004)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Border Dummy$_{ij}$	-0.130***	-0.114**	-0.215***	-0.056	-0.201***	-0.118***	-0.119*	-0.092	-0.072*	-0.008	-0.045	-0.126***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)	(0.06)	(0.07)	(0.04)	(0.06)	(0.06)	(0.04)
ln Distance$_{ij}$	-0.550***	-0.717***	-0.291***	-0.482***	-0.497***	-0.452***	-0.557***	-0.781***	-0.517***	-0.622***	-0.795***	-0.582***
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.03)	(0.06)	(0.06)	(0.04)	(0.06)	(0.06)	(0.04)
Fixed Effects												
Importer	YES	-	-	YES	-	-	YES	-	-	YES	-	-
Exporter	YES	-	-	YES	-	-	YES	-	-	YES	-	-
Importer × Commodity	-	-	YES	-	-	YES	-	-	YES	-	-	YES
Exporter × Commodity	-	-	YES	-	-	YES	-	-	YES	-	-	YES
Multilateral Resistance	-	YES	-	-	YES	-	-	YES	-	-	YES	-
Additional Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adjusted R^2	0.911	0.853	0.655	0.904	0.866	0.836	0.893	0.829	0.822	0.914	0.852	0.804
Observations	768	768	13,303	766	766	11,275	739	739	7,721	768	768	12,772

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include parameters as of column (7), Table 5 as additional controls. Significance levels as in Table 3.

TABLE 17 – SECTORAL INCLUDING CONTROLS (fixed-effects estimation)

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
Year of Data			**1993**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.284**	-0.388	-0.240**	-0.162	-0.180**
	(0.11)	(0.36)	(0.11)	(0.11)	(0.09)
ln Distance$_{ij}$	-0.665***	-0.478*	-0.299**	-0.312***	-0.236***
	(0.12)	(0.27)	(0.12)	(0.12)	(0.09)
Additional Controls	YES	YES	YES	YES	YES
Observations	4,815	1,336	3,078	4,324	11,990
Adjusted R^2	0.788	0.827	0.738	0.711	0.808
Year of Data			**1997**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.165	-0.434	-0.050	-0.205**	-0.153**
	(0.13)	(0.33)	(0.12)	(0.10)	(0.07)
ln Distance$_{ij}$	-0.727***	-0.688**	-0.517***	-0.503***	-0.487***
	(0.13)	(0.27)	(0.14)	(0.15)	(0.07)
Additional Controls	YES	YES	YES	YES	YES
Observations	5,490	2,655	3,215	3,455	7,620
Adjusted R^2	0.828	0.834	0.802	0.780	0.843
Year of Data			**2002**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.116	-1.073**	0.115	0.046	-0.107
	(0.17)	(0.53)	(0.15)	(0.13)	(0.10)
ln Distance$_{ij}$	-0.567***	-0.050	-0.682***	-0.547***	-0.618***
	(0.15)	(0.41)	(0.15)	(0.13)	(0.10)
Additional Controls	YES	YES	YES	YES	YES
Observations	4,470	1,629	2,820	3,205	7,080
Adjusted R^2	0.813	0.848	0.761	0.730	0.815
Year of Data			**2007**		
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.122	0.096	-0.014	-0.052	-0.293***
	(0.12)	(0.31)	(0.12)	(0.13)	(0.08)
ln Distance$_{ij}$	-0.772***	-1.353***	-0.782***	-0.514***	-0.568***
	(0.12)	(0.28)	(0.13)	(0.12)	(0.09)
Additional Controls	YES	YES	YES	YES	YES
Observations	4,171	1,914	3,116	3,472	7,436
Adjusted R^2	0.847	0.839	0.790	0.720	0.853

Notes: Importer and exporter fixed effects included in all regressions. Constant, controls and fixed effects not reported. Robust standard errors reported in parenthesis. Commodities pooled into sectors as listed in Table 19 and 20 in the Appendix. Significance levels as in Table 3.

TABLE 18 – ADDITIONALLY INCLUDING THE WEST: SENSITIVITY

Specification	Fixed Effects		OLS with MR Terms	
	1997 (N = 2,091)			
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.035		-0.189***	
	(0.04)		(0.04)	
South–South Dummy$_{ij}$		0.126*		-0.095
		(0.07)		(0.07)
North–North Dummy$_{ij}$		-0.537		-0.155***
		(1.19)		(0.03)
West–West Dummy$_{ij}$		0.049		0.252***
		(0.08)		(0.07)
ln Distance$_{ij}$	-0.334***	-0.326***	-0.584***	-0.584***
	(0.04)	(0.04)	(0.03)	(0.03)
Additional Controls	YES	YES	YES	YES
Adjusted R^2	0.837	0.837	0.751	0.753
F-Test	92.99	92.08	193.72	185.70
	2002 (N = 2,039)			
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.073*		-0.074*	
	(0.04)		(0.04)	
South–South Dummy$_{ij}$		0.110		-0.121*
		(0.07)		(0.07)
North–North Dummy$_{ij}$		-0.009		0.091**
		(0.05)		(0.05)
West–West Dummy$_{ij}$		-0.055		0.162**
		(0.08)		(0.07)
ln Distance$_{ij}$	-0.423***	-0.435***	-0.718***	-0.682***
	(0.05)	(0.05)	(0.03)	(0.03)
Additional Controls	YES	YES	YES	YES
Adjusted R^2	0.840	0.839	0.725	0.726
F-Test	88.57	88.05	184.83	176.99
	2007 (N = 2,125)			
	(1)	(2)	(3)	(4)
Border Dummy$_{ij}$	-0.046		-0.075*	
	(0.04)		(0.04)	
South–South Dummy$_{ij}$		0.068		-0.003
		(0.07)		(0.06)
North–North Dummy$_{ij}$		0.010		0.171***
		(0.05)		(0.04)
West–West Dummy$_{ij}$		0.037		0.198***
		(0.08)		(0.07)
ln Distance$_{ij}$	-0.463***	-0.456***	-0.728***	-0.677***
	(0.04)	(0.05)	(0.03)	(0.03)
Additional Controls	YES	YES	YES	YES
Adjusted R^2	0.853	0.853	0.740	0.741
F-Test	102.57	102.11	204.68	197.00
Fixed Effects				
Importer	YES	YES	-	-
Exporter	YES	YES	-	-
Multilateral Resistance	-	-	YES	YES

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include parameters as of column (7), Table 5 available for all US states as additional controls. Significance levels as in Table 3.

TABLE 19 – 1993 STANDARD TRANSPORTATION COMMODITY CODES (STCC)

Commodity	Meaning	Agriculture	Mining	Chemical	Machinery	Manufacturing
1	Farm Products	x				
8	Forest Products	x				
9	Fresh Fish or Other Marine Products	x				
10	Metallic Ores		x			
11	Coal		x			
13	Crude Petroleum, Natural Gas, Gasoline		x			
14	Non-metallic Minerals		x			
19	Ordinance or Accessories					
20	Food or Kindred Products	x				
21	Tobacco Products, excluding Insecticides	x				
22	Textile Mill Products					x
23	Apparel or Other Finished Textile Products					x
24	Lumber or Wood Products, excluding Furniture					x
25	Furniture or Fixtures					x
26	Pulp, Paper, Allied Products					x
27	Printed Matter					x
28	Chemicals or Allied Products			x		
29	Petroleum or Coal Products			x		
30	Rubber or Miscellaneous Plastics Products			x		
31	Leather or Leather Products					x
32	Clay, Concrete, Glass, Stone Products					x
33	Primary Metal Products					x
34	Fabricated Metal Products					x
35	Machinery, excluding Electrical				x	
36	Electrical Machinery, Equipment, Supplies				x	
37	Transportation Equipment				x	
38	Instruments, Photographic and Optical Goods				x	
39	Miscellaneous Products of Manufacturing					x
40	Waste or Scrap Materials					
41	Miscellaneous Freight Shipments					
99	LTL-General Cargo					

TABLE 20 – 1997, 2002, 2007 STANDARD CLASSIFICATION OF TRANSPORTED GOODS (SCTG)

Commodity	Meaning	Agriculture	Mining	Chemical	Machinery	Manufacturing
1	Live animals and live fish	x				
2	Cereal grains	x				
3	Other agricultural products	x				
4	Animal feed and products of animal origin, n.e.c.	x				
5	Meat, fish, seafood, and preparations	x				
6	Milled grain products, bakery products	x				
7	Other prepared foodstuffs, fats, oils	x				
8	Alcoholic beverages	x				
9	Tobacco products	x				
10	Monumental or building stone		x			
11	Natural sands		x			
12	Gravel and crushed stone		x			
13	Nonmetallic minerals n.e.c.		x			
14	Metallic ores and concentrates		x			
15	Coal		x			
17	Gasoline and aviation turbine fuel		x			
18	Fuel oils		x			
19	Coal and petroleum products, n.e.c.		x			
20	Basic chemicals			x		
21	Pharmaceutical products			x		
22	Fertilizers			x		
23	Chemical products and preparations, n.e.c.			x		
24	Plastics and rubber			x		
25	Logs and other wood in the rough	x				
26	Wood products					x
27	Pulp, newsprint, paper, and paperboard					x
28	Paper or paperboard articles					x
29	Printed products					x
30	Textiles, leather, articles of textiles or leather					x
31	Nonmetallic mineral products					x
32	Base metal in primary or semifinished forms					x
33	Articles of base metal					x
34	Machinery				x	
35	Electronic and office equipment and components				x	
36	Motorized and other vehicles (including parts)				x	
37	Transportation equipment, n.e.c.				x	
38	Precision instruments and apparatus				x	
39	Furniture, mattresses and supports, lamps					x
40	Miscellaneous manufactured products					x
41	Waste and scrap					
43	Mixed freight					